NATURAL SCIENCE

A MONTHLY REVIEW OF SCIENTIFIC PROGRESS

VOL. XI.

JULY—DECEMBER 1897

LONDON

J. M. DENT & CO.

67 SAINT JAMES'S STREET, S.W.

SUBJECT INDEX TO ARTICLES

THESE titles may be cut out and pasted on cards for a slip-catalogue. Those who do not wish to use the decimal notation as a guide to the subject matter can cut off the numbers.

507 **Bolton, H.** The Provincial Museum.—*Nat. Sci.*, XI., 387 **Dec., 1897**

550.1 **Howorth, H. H.** A New Scheme of Geological Arrangement and Nomenclature.—*Nat. Sci.*, XI., 321 **Nov., 1897**

551.47(98) **Smith, B. L.** Note on the warm Undercurrent in the Arctic Ocean between Greenland and Spitzbergen.—*Nat. Sci.*, XI., 48 **July, 1897**

551.79 **Oldham, R. D.** The Facetted Pebbles of India.—*Nat. Sci.*, XI., 197 **Sept., 1897**

565.3 **Bernard, H. M.** Fossil Apodidae.—*Nat. Sci.*, XI., 397 **Dec., 1897**

567 **Lankester, E. R.** The Taxonomic Position of the Pteraspidae, Cephalaspidae, and Asterolepidae.—*Nat. Sci.*, XI., 45 **July, 1897**

568 **Ridewood, W. G.** On the Restoration of Some Extinct Reptiles. *Nat. Sci.*, XI., 190 **Sept., 1897**

569.(8) **Ameghino, F.** South America as the Source of the Tertiary Mammalia. —*Nat. Sci.*, XI., 256 **Oct., 1897**

571.1 **Cunnington, W.** The Authenticity of Plateau Man.—*Nat. Sci.*, XI., 327 **Nov., 1897**

571.1 **M'Kay, G. R.** Evidence of the Antiquity of Man in East London, Cape Colony, with a Note on the Castor-Oil Plant.—*Nat. Sci.*, XI., 334 **Nov., 1897**

571.93(42.25) **Abbott, W. J. L.** Primeval Refuse Heaps at Hastings.—*Nat. Sci.*, XI., 40, 94 **July, Aug., 1897**

572.(94.2) **'Theta.'** Initiation Rites of the Arunta Tribe, Central Australia.—*Nat. Sci.*, XI., 119 **Aug., 1897**

575 **Bulman, G. W.** Bees and the Development of Flowers.—*Nat. Sci.*, XI., 100 **Aug., 1897**

575 **Hyatt, A.** The Influence of Woman in the Evolution of the Human Race.—*Nat. Sci.*, XI., 89 **Aug., 1897**

575 **Hutton, F. W.** The Place of Isolation in Organic Evolution.—*Nat. Sci.*, XI., 240 **Oct., 1897**

575 **Coste, F. H. P.** Professor Schiller on Darwinism and Design.—*Nat. Sci.*, XI., 408 **Dec., 1897**

575.1 **Hartog, M.** The Fundamental Principles of Heredity.—*Nat. Sci.*, XI., 233, 305 **Oct., Nov., 1897**

575.1 **Tayler, J. L.** The Relation of Acquired Modifications to Heredity.—*Nat. Sci.*, XI., 247 **Oct., 1897**

575.2 **Pearson, K.** On the Scientific Measure of Variability.—*Nat. Sci.*, XI., 115 **Aug., 1897**

NATURAL SCIENCE

A Monthly Review of Scientific Progress

No. 65—Vol. XI—JULY 1897

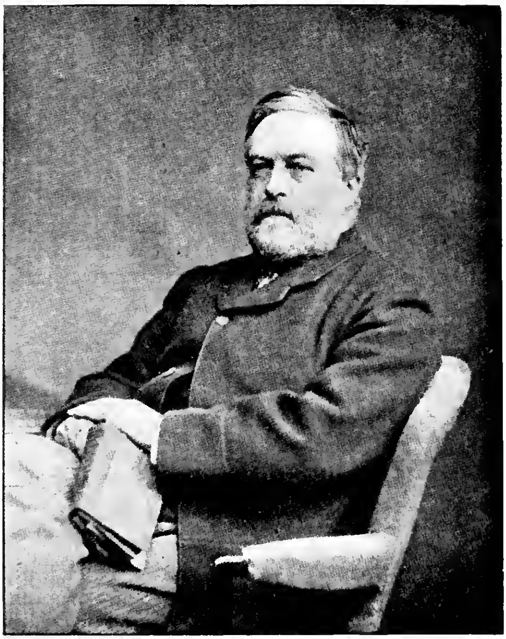
NOTES AND COMMENTS

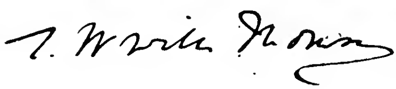
The Cambridge Graces

On few recent educational questions have we felt such difficulty in forming an opinion as on the proposal that the women students at Cambridge should be granted the degrees for which they may have passed the examinations. Anxious that the opportunities for women's education should be extended in every way, our natural sympathies were in favour of the proposals. There can be no doubt that those Girtonians and Newnhamites who enter the educational profession would find it a great advantage to have a Cambridge degree. To refuse it to women who have resided at Cambridge for the necessary number of terms, and have passed all the prescribed examinations, seems to us unfair and almost churlish. Women who have done the same work and stood the same tests as men, should surely be allowed the same certificate of efficiency. It seems to us hopeless to expect that popular opinion will ever estimate a Girton certificate as of equal value to a Cambridge degree, while the university refuses the latter. The present position of women students at Cambridge seems to us anomalous, illogical, and very unfair. But we doubt if the proposed changes would have improved matters. No one denies that had the graces been passed, there would have been a great increase in the number of women students at Cambridge. It is affirmed by some Cambridge authorities, whose opinions are usually entitled to respect, that the admission of women would have been followed by a considerable decrease in the number of male undergraduates. Suppose that in ten or twenty years' time, there are half as many women students as men. It would then be intolerable that the tutors and lecturers at the women's colleges and halls should have no voice in arranging the courses of study. If women be allowed the examinations and degrees, and form a third of the registered students, it would be quite unfair that they should not be eligible for all prizes and scholarships, and that their interests should not be protected by direct representation on the boards of studies. We should not grudge the women any prizes that they won. The competition of the best women would do the men nothing but good. The danger is that alterations would be made in the interests of the weaker of the women, which might enable men to gain degrees with even less work than at present. Moreover, a course of study suitable for men may not be suitable for women. So long as women only enter for the tripos examinations, this objection is not serious. But if women are to have degrees, it would be absurd to allow men to take an easy poll degree, and restrict women to the tripos. The training for an ordinary Cambridge degree may not be an ideal education for men, although it may do well enough for the average curate and country gentleman. But in an education of that standard the requirements of men and women are very different. If women are to be admitted to poll degrees, then alterations in the examinations for them ought to be made. Hence we are inclined to prefer a separate university for women to a mixed university, at which both men and women would have to put up with an hermaphrodite education suitable for neither. Moreover, if women be allowed to enter the university and share in the scholarships, there is no logical reason why girls should not be admitted in separate classes to the public schools, to the students of which so many of the best scholarships are restricted.

The Woman of the Future

One thing is still lacking to "the new woman." It is a beard. But this, according to Dr A. Brandt, she may hope to have in the course of some thousands, possibly hundreds, of years. Many women have beards already. Some take a pride in them, and utilise them as a source of income; others keep them in check by the use of depilatories, of forceps, or even a razor. As for moustaches they are so common as to pass without remark, and a tender shade upon the upper lip of a brunette may even be regarded as an added beauty. Still, at the present period in the evolution of our race, it is for the most part in old age that the beard comes to woman. The beard does not appear in man before puberty, and increases in strength with age, often compensating for a loss of hair on the head. Dr Brandt, therefore, writing in the *Revue Scientifique* for May 15, regards the beard not as an ancestral, but as in part a second and in part a senile character. Like other characters that first appear late in the life of individuals, it is likely to be accelerated in its development. In other words, hair will appear on the face at an earlier and earlier age, as time goes on, both in men and women. "Perhaps a day will dawn when we shall think a moustache in a woman less ugly than a bust deformed by the corset."





The Contributors to the 'Challenger' Reports

The fifty volumes of the 'Challenger' reports having been recently completed, a complimentary album was presented to the editor, Dr John Murray, by his colleagues. The album was itself a work of art, but its chief interest lay in the circumstance that it contained the photographs of all who had contributed to the literature of the Reports. As some of these authors are no longer among us, and others are dispersed over the four quarters of the globe, it was an arduous undertaking to fill up this portrait-gallery of scientific worthies. But Mr W. E. Hoyle, as Hon. Secretary to the presentation committee, met with the friendliest response to every application, and, with the assistance of Mr Walter Crane in the artistic department, successfully coped with all the difficulties that arose, whether expected or unexpected. After so much trouble had been expended, the happy thought occurred to Mr Hoyle that many who might never have a chance of seeing the original album, gleaming in purple and gold, would welcome an opportunity of possessing a copy of its contents. The Committee warmly approved of this suggestion, and the result is seen in a thin quarto volume (price, 12s. 6d.), uniform in size and binding with the reports. It contains reduced copies of the 88 portraits on 19 plates, together with reproductions in black and white of Mr Walter Crane's designs for the cover and dedication. As a specimen of the portraiture we are enabled to show our readers a likeness of the late Sir C. Wyville Thomson, the original director of the civilian staff. Whether the small edition of 200 copies will suffice for all who will wish to possess this interesting volume may be doubted. The publishers are Messrs Dulau & Co.

Phenacomys

The genus *Phenacomys* has no doubt existed at least as long as the genus *Homo*, and specimens of this small vole may even have been known to man, and have been hoarded by that acquisitive animal in his museums for a considerable period. We know, in fact, that a specimen obtained and presented by Mr J. K. Lord of the North American Boundary Commission, has been in the British Museum since 1863. None the less so short a time as eight years ago, zoologists were unaware of the existence of this genus, although the suspicions of a few may have been aroused by Nehring's description, in 1883, of some bones and teeth found in a cave in Southern Hungary. Within the last eight years, however, no less than nine living species of the genus have been described, all from Boreal North America. Ninety-five specimens have been at the disposal of

Mr Gerrit S. Miller, jun., who, in the *Proceedings* of the Biological Society of Washington (xi., pp. 77-87, April 21, 1897), considers that there are six valid species. These fall naturally into three groups, each of which occupies a different geographic region. They generally inhabit dry, grassy plains and mountain parks, and, except for some small characters in the teeth, are much like the ordinary *Microtus;* from the humid coast district of Oregon, however, there comes a long-tailed form, which appears to be strictly arboreal.

The Geographical Distribution of the Dragon-flies

One of the greatest surprises in store for the student at the outset of his study of zoo-geography is the discovery that animals so different in their powers of locomotion as mammalia and birds agree nevertheless so closely in geographical distribution, that a map representing the zoological areas of the one class, will, in its broad outlines, be equally applicable to the other. The obvious inference to be drawn from this circumstance, the inference, namely, that the actual means of progression, whether it be flight or swimming or running, are far less important factors in determining the dispersal of species than one would be led on *à priori* grounds to suppose, is still further supported by a recent paper on "The Geographical Distribution of Dragon-flies," published by Mr G. H. Carpenter in vol. viii. of the *Scientific Proceedings of the Royal Dublin Society.* Though this paper is chiefly nothing but a compilation from Mr W. F. Kirby's catalogue of the Odonata, it is nevertheless a valuable piece of work, since it represents in a concise and intelligible form the range of all the genera of the great order of powerful-flying insects, and is accompanied by a map showing how closely in the main their distribution tallies with that of the other orders and classes of terrestrial animals that have been faunistically studied. Seven regions are recognised: the Holarctic, comprising the Palaearctic of Sclater and Wallace, *plus* the Nearctic almost down to the fortieth parallel of latitude; the Ethiopian; the Mascarene; the Oriental; the Australian, including New Zealand; the Sonoran; and the Neotropical. It is interesting to note that there is no evidence of a Mediterranean region in the Old World corresponding to the Sonoran of the New, and that the species from Madagascar are no more Ethiopian than Oriental in their affinities. The advocates of Lemuria may get some satisfaction from the latter circumstance; but those who are in favour of an Antarctic area will not gain much support from the dragon-flies, since the species of these insects that inhabit Patagonia, Cape Colony, and New Zealand bear no witness to a former land connection between these countries.

The Arthropods of Funafuti

Part II. of the "Memoir on the Atoll of Funafuti," based on collections made by Mr Charles Hedley, was published by the Australian Museum, Sydney, on February 25, 1897. Its arrival gives us rather a shock, for we were under the impression that, when the Royal Society invited the Australian Museum to send a naturalist on their expedition, a stipulation was made that the representatives of the Royal Society should retain the right of prior publication. If such an agreement was not made, it ought to have been, in the interests not merely of justice, but, as this publication proves, of science also.

Mr W. J. Rainbow undertakes to describe the whole of the land arthropods, and his knowledge is scarcely commensurate with the undertaking. Whose knowledge could be? Probably it is not Mr Rainbow who is to blame, but the authorities of the Australian Museum, who, like the head officials in many other scientific institutions, seem to think that they have only to say to a subordinate, "Do!" and he doeth it, even though it be a task for which years of training are necessary. In recent zoology, just as in palaeontology, work of this kind can only be done well by the specialist having at his disposal large collections and complete libraries. We have no wish to be hard on an obedient servant, but we must justify our remarks by a few selected criticisms of Mr Rainbow's work.

On p. 97, the syllable "Nob." following "genus *Lispe*," is, no doubt, a printer's error for 'Latr,' since Latreille founded this genus in 1796. We let this pass, but discover from the figure that the single female specimen, here made the type of a new species of *Lispe*, is not a *Lispe* at all, and does not even belong to the same family. So far as may be judged from the figure, it is a *Coelopa* (family, Phycodromidae.) "Nob." again, this time following "genus *Ebenia*"! Mr Rainbow may have described a genus under the name *Ebenia*, though he gives no reference to the place where he published it; but the real *Ebenia* was founded by Macquart for a Brazilian species having no sort of affinity with the two specimens here described under that name. It is impossible to say whether the four species of Diptera described, three on the evidence of single specimens, are new or not; the descriptions are too short to be of value, the figures are atrocious, no reference is made to allied forms, and the author, while including family characters, ignores those of specific rank. With the exotic Muscidae (*sens. lat.*) in their present state of chaos, every attempt of inexperienced workers to describe new species is a distinct retarding of science.

Of Arachnida, 88 specimens were secured, and these are distributed by Mr Rainbow into 25 species, of which 15 are new. Fifteen! At any rate *Epeira ventricosa*, and probably some of the other species of *Epeira* described as new, appear to be nothing but representatives of that wide-spread, highly variable, and well-known species *Epeira theïs;* while *Hyllus ferox* and *H. audax*, supposed new species, have been placed in a wrong genus, and are probably old friends.

The scorpion described as "*Buthus brevicaudatus* sp. n." belongs to a totally different family from *Buthus;* it is perhaps the best known of all scorpions, and more than a century ago Fabricius named it *Hormurus australasiae.*

Fortunately for Mr Rainbow and for us, he does recognise that some of the species found on Funafuti are neither new nor peculiar to that island, but that they have been introduced by man. Among these are certain mosquitoes, which the natives catch with a kind of racquet, the meshes of which are made of the glutinous snares of orb-weaving spiders. White ants, *Calotermes marginipennis*, attack the coco-palms at a height of three to six feet above the ground, tunnelling their way through; as a result the trees are snapped off by the gales. It is probable that both the tree and the termite were introduced by human agency, *via* Hawaii, about two centuries ago.

The Crustaceans and Echinoderms of Funafuti

These animals have fallen into the more experienced hands of Mr T. Whitelegge. The Crustacea are the lords of the atoll, swarming into all vacant places. "The *Coenobita*," says Mr Hedley, "wander across from shore to shore, and dispute any stray edibles with the rats. Some crabs even take up their residence in the tree tops of Pandanus, while, as everybody knows, *Birgus* is as much at home on a palm bole as a squirrel on an oak. . . . Human habitations are not even secure from crabs. . . . Active as they are during the day, it is at night that the land crabs hold high carnival. On the beaches the Crustacea were everywhere abundant, particular species possessing each their special zone. About high tide mark on the windward shore promenaded *Grapsus maculatus*, a crowd of which scattered before the footsteps of a visitor, and sought refuge under loose coral blocks or in deep pools. Rolling over a slab of dead coral rock anywhere between tide marks, exposed the haunt of a little community of *Petrolisthes dentata* and *Leiolophus planissimus.* Intercepted in their efforts to escape, these would flatten themselves down to the surface of the stone so closely that the collector's fingers with difficulty grasped them. The deeper rock-pools at the border of the reef-flat, the chief home of *Salarius*, were usually tenanted by a few *Calcinus elegans*, whose brilliant red, blue, and white claws distinguished it as the dandy of the company. This species is never out of the range of the rough waves. The extreme windward portion of the reef left dry at low tide was but rarely attainable; *Aniculus*, whose bristly claws usually protruded from a stolen *Turbo* shell, was a distinctive feature of this zone. In the honey-combed pits of the millipore mounds that breasted the surf, cowered *Daira perlata*. The close resemblance of colour and contour to the surrounding rock rendered this crab difficult to detect, and when seen the creature's powers of adherence and the sweep of the Pacific rollers rendered it as difficult to seize."

Geograpsus crinipes, Dana, occurred in association with *Coenobita* and *Cardisoma*, at a distance from the sea, among broken coral rocks shaded by vegetation. This is the first instance of a grapsoid crab living wholly on dry land, and it displays interesting adaptations to terrestrial conditions. It breathes by hair-lined pores between the bases of the second and third pairs of walking-legs; and its colour, a dirty yellowish-white, harmonises with that of the coral fragments.

Athelgue aniculi is a new Epicarid living on *Aniculus typicus*, a hermit crab that lives in the shell of *Turbo setosus*, on the outer edge of the reef, most exposed to the surf.

The collection of echinoderms comprises 130 specimens, representing 19 species, mostly common forms :—7 echinoids, 3 asteroids, 3 ophiuroids, 6 holothurians. A detailed description is given of two specimens referred to *Culcita acutispina*. Most specimens of this starfish, as also of *Anthenea acuta*, common in Port Jackson, are unsymmetrical when dredged up; but, if placed on a level surface in sea-water, they soon regain their natural form, and may be killed in that state either by flooding them with fresh water or by placing them in strong spirit. Neglect to take these precautions may have led to the establishment of invalid species.

Granivorous Insects

Another of the excellent Bulletins (N.S. No. 8) issued by the Entomological Division of the U. S. A. Department of Agriculture has reached us. It is written by Mr F. H. Chittenden, and deals with insects which have been observed in North America to injure stored grain and other vegetable products. It is of interest to find included among these, certain beetles of the family Dermestidae and their grubs. These insects, which include the well-known 'Bacon Beetle,' have long been notorious as devourers of dried animal matter, but have not attracted attention as vegetarians. It appears, however, that some of them can find sustenance on stored grain, meal, and cloth.

A Study in Parasitism

Bulletin No. 5 (Technical Series) of the same department contains an exhaustive account by Mr L. D. Howard of the insect-enemies of the white-marked tussock moth (*Orgyia leucostigma*). The hairy caterpillars of this moth have become, during recent years, highly injurious to foliage trees in the cities of the northern States. It appears that the vast multiplication of these caterpillars began with the introduction of the European sparrow. This bird has well-nigh exterminated other caterpillars which used to compete with the 'tussocks' for a livelihood on the trees; it will not touch the hair-protected 'tussocks' itself, but it has largely driven out the native birds which used to feed on them. Fortunately, however, an army of insect-parasites keep the caterpillars from increasing beyond all bounds. Fifteen species of Hymenoptera and six of Diptera are described as laying their eggs in either larva or pupa of the *Orgyia*. These parasites are in their turn subject to attack by thirteen species of minute Hymenoptera. An alarming increase of the 'tussocks' at Washington during the summer of 1895 was accompanied by a corresponding increase in the parasites, so that the vast majority of the caterpillars were destroyed. The next year, however, owing to the work of the secondary parasites, the 'tussock' recovered its ground to a considerable extent. Two species of tiny hymenoptera were proved to be 'tertiary' parasites, their grubs devouring those of the secondaries. Finally, eleven species of Diptera act as scavengers, their grubs feeding on the dead pupae and cocoon-masses of the contending insect-armies.

A Study in Commensalism

It is cheering to turn from this summary of conflict and slaughter to another side of insect-life lately investigated by Mr C. Janet. In a little work of 62 pages ("Sur le *Lasius mixtus*, l'*Antennophorus uhlmanni*, &c.": Limoges, 1897) he describes the relations which subsist between social ants (*Lasius*) and certain gamasid mites and other arthropods which are found in their nests. The mites of the species *Antennophorus uhlmanni* are carried about by the worker ants; in their penultimate and adult stages they seem unable to make their own way about the nest, and wait feeling with their long front legs for an ant to which they can attach themselves. An ant normally carries three of the mites—one under the head and one on either side of the abdomen; these positions are the least inconvenient possible to the ant. The mites feed entirely on liquid disgorged by their hosts. An individual under an ant's head naturally draws his supply from the insect's mouth, especially when she disgorges to feed a comrade. A mite carried on the abdomen of an ant taps, with his two front pairs of feet, another ant which happens to be near, and for thus asking obtains food. No benefit to the ants from the presence of these mites is suggested, and the relation bears the aspect of disinterested charity.

Another species of mite, *Discopoma comata*, is on the other hand a true parasite. These gamasids attach themselves to the ant's abdomen, pierce the intersegmental membrane, and suck food-supplies.

A bristle-tail or thysanure, *Lepismina polypoda*, plays the part of pickpocket in the ant-colony. He comes stealthily between two ants when one is feeding the other, grabs the drop of liquid nourishment in its passage, and makes his escape as quickly as possible to levy contribution on other couples until his hunger is satisfied.

The Pigments of the Decapod Crustacea

Another interesting contribution has just been made to our knowledge of the pigments of the decapod crustacea by Miss M. I. Newbigin (*Journ. Physiol.*, vol. xxi., pp. 237-257, May 1897). She discusses the predominant characteristics of the pigments of *Homarus vulgaris, Nephrops norwegicus* and *Astacus nobilis.* The conclusions arrived at support recent observations on the same subject, but a speculative tone mainly towards the end of the paper rather spoils the general effect.

Thus we cannot accept the conclusion "that there is some connection between the little known substance in the muscle and the formation alike of the blue pigment and of chitin." The conclusion also that the red lipochrome is present as a calcium combination in the shell is only speculation. The pigment is laid down there before calcification takes place: in fact pigment and calcium salt are more or less independent of each other.

While in the earlier parts of the paper a distinction is drawn between the yellow and red pigments, in the 'summary' the former is said to be partially derived from the latter "under the influence of certain reagents." A 'lipochromogen' is not a 'compound' of a lipochrome: it is a precursor of one. The return to Krukenberg's term 'hepatochrome' would certainly be unfortunate, as the organ in which such pigment or rather pigments occur is not a liver but mainly a pancreas, and although no band in red was recognised in extracts of the digestive gland by Miss Newbigin there is a chlorophylloid pigment, which is better named enterochlorophyll, present in most if not in all cases.

There is no mention of this spectrum given by the pigment itself when the red hypodermis is examined by the spectrum eye-piece by means of reflected light.

We fail to find any evidence in this paper that the ‘chemistry of the crustacean pigments has been advanced beyond the old stage of solution and colour-change with reagents. What we want is isolation and purification before we can say that our knowledge of the subject has been largely increased.

However, in spite of these minor blemishes (and all new work must be imperfect), we welcome this paper and hope the author may continue her observations.

The Embryonic Shell in Bivalves

The shell, or rather pair of shelly valves of the young bivalve, that forms before the little creature is hatched, is usually very different in appearance to that enclosing the fully formed animal. The prodissoconch, as it has been termed, is small, unornamented, and may frequently be seen capping the umbones of the older shell. It has been considered the sole stage prior to the exclusion of the embryo from the egg-shell. Mr Félix Bernard, however, (*Comptes Rendus Acad. Sci. Paris*, vol. cxxiv. pp. 1165-8) has detected an earlier stage which he calls the *protostracum*, and he has been able to find this protostracum on the summit of each prodissoconch studied by him. The Glochidium stage in the Unionidae is the equivalent of this protostracum. In most cases the hinge-line of the protostracum undergoes no increase during the completion of the prodissoconch stage. It consists of a series of interlocking crenulations, for which Mr Bernard accepts Dall's suggested name of provinculum, with a central ligament pit. The development of the Heterodonts is so rapid, that, as a rule, the crenulations have no time to form, but are superseded by the true teeth, whose development has already been commented on in these pages (vol. ix. p. 358). This acceleration of development of the hinge is correlative with the earlier incubation of the individuals of this group, entailing an earlier use of the shell with a consequent stimulation of the hinge through function.

The free swimming larval stage, which has its foot adapted for creeping, and free mantel lobes, guiltless of siphons, and whose gills are situated in the rear, whilst it possesses the velum characteristic of all larval mollusca, is common to all Pelecypoda and representative of the ancestral form.

The Reproduction of Diatoms

The two chief methods by which diatoms have hitherto been supposed to reproduce themselves are (1) by simple vegetative division, and (2) by the formation of so-called *auxospores*, which are usually the result of conjugation. The former method of division is of unusual interest in the diatoms, since, owing to the peculiar arrangement of their valves by which one fits inside the other, a formation of progressively smaller and smaller individuals is a necessary result of simple division. The growth in size of the valves is usually supposed to be impossible on account of their silicified nature. It is imagined, however, that the production of infinitely small individuals, which would obviously result from the continuance of such a method of division, is prevented by the interpolation of auxospore formation; by the latter means the diatom is given an opportunity of regaining its maximum size.

Mr George Murray, in a paper entitled "On the reproduction of some marine Diatoms" (*Proc. Roy. Soc., Edin.*, Dec. 1896), has, however, thrown new light on reproduction in this group. The author has observed in some marine forms a very interesting and totally new method of division. This form of reproduction was observed in *Coscinodiscus, Biddulphia, Choeboceros*, &c., but was followed out most fully in the first-mentioned genus. In *Coscinodiscus* the cell-contents divide by successive division into eight or sixteen portions, and these become rounded off and lie free in the mother cell like spores in a sporangium. Each of these portions becomes invested with valves showing the characteristic markings, and in fact becomes a young *Coscinodiscus*. These young forms ultimately escape from the parent cell, and are found floating free in the water as packets of eight or sixteen small individuals enclosed in a delicate membrane; later on the several individuals themselves become completely free. It is by this method of division that the enormous quantities of marine diatoms, occurring in many waters at certain seasons, are doubtless produced.

As mentioned above, diatoms are usually supposed to be incapable of superficial growth, owing to the silicified nature of their membrane; but, as Mr Murray points out, this view is hardly well founded. At all events, it raises the question of the nature of the membranes produced by the young diatoms inside the old valves. These diatoms must obviously be much smaller than the parent, and so, in the absence of growth, continued reproduction by this method would have as disastrous an effect on the size of the species as the method of simple division, and would similarly necessitate the production of auxospores. With this question in view Mr Murray investigated the nature of the membrane of the daughter forms, and his conclusion is that the valves are either not silicified or only incompletely so. There is thus nothing to prevent the further growth of these young diatoms to the full size of the species. That this later growth takes place is supported by observation, for the very small forms of certain species of *Coscinodiscus* which are abundant at one season, disappear later in the year.

Plants and the Weather

We have received an interesting little pamphlet on the effects of weather upon vegetation, the subject of a recent lecture to the Bradford Naturalists' Society.

The lecturer, Mr John Clayton, has made some simple but none the less instructive experiments, the results of which he put before his audience. The effects of sunshine are, as we should expect, very striking. Of twelve bean-plants, as like as possible in size and health, six were placed in the ground where they would catch all the sunshine of the day; the other six were sheltered by a boarding which effectually prevented any rays from falling upon them. When freshly gathered in October the weight of beans and pods grown in sunshine was more than three times as great as in the case of those grown in the shade (99 : 29), while the weight of the dry beans was in a similar proportion (16 : 5). The experiment was continued in succeeding years. Thus in 1892 the fresh weight of beans and pods grown from the sunshine-grown seed of 1891 was half as much again, as in the case of plants from shade-grown seeds—all being grown in sunshine and under precisely similar conditions in the second year. In the fourth year plants with an exclusively shady ancestry produced flowers, but failed to mature fruit.

A series of measurements illustrating the contraction of trees in frost was made in the winter of 1894-5. A comparison of the girth of tree-trunks in October, when growth had ceased but before the frost set in, with the girth at 9 a.m. on February 8th, at a temperature of 3° F., showed an appreciable contraction under frost. In the sycamore it was from two to three sixteenths of an inch, in the elm from three to five sixteenths, in the oak from five to six sixteenths. On March 2, at a temperature of 39° F., the trunks had expanded to their original measurements. To this contraction under frost is due the frequent splitting of our forest trees.

An interesting review is also given of the distribution of sunshine, rain, &c., in the different months of the year; and various improvements are suggested for individual months, which we recommend to the consideration of the clerk of the weather.

Plant Chemistry

The chemistry of some common plants is discussed by Dr P. Q. Keegan in a recent number of *The Naturalist*. The buttercup owes its brilliant golden hue to a pigment called carotin (from its presence in the carrot root), "which is amassed in discoidal bodies that nearly fill up the cells of the epidermis, especially towards the base of the petal; in other parts, especially when the flower is fully expanded, it seems diffused in oily droplets or amorphous granules." In either case the starch grains in the subjacent tissue act as a reflector, and contribute greatly to enhance the effect. The flower-heads contain a considerable amount of sugar, starch, calcium oxalate, and soluble phosphates, in these respects approaching more to the character of leaves than is usual. The stem and root of this buttercup (*Ranunculus bulbosus*) are remarkable for the presence of an acrid camphoraceous body easily decomposed into a volatile bitter principle (anemonin) and an acid, even during the drying of the plant, so that its original poisonous character disappears.

Bird's-foot trefoil (*Lotus corniculatus*), with the brilliant orange and crimson tints of its little papilionaceous flowers, is known to everyone. To produce this vividness and lustre the epidermal cells are swollen into papillae, and contain no less than three distinct pigments. There are the solid carotin corpuscles, and also two colouring-matters in solution in the cell-sap. One is a clear, yellow juice, the other is identical with the anthocyan of the rose. Where the latter predominates we get the deep red colour.

The tiny flowers of the cheerful little yellow bedstraw (*Galium verum*) contain carotin, much yellow resinous matter, and "a curious purplish substance (possibly purpurin) insoluble in cold alcohol or benzene after purification." The flowers also contain a species of ferment which, like rennet, has the power of coagulating boiling milk. A substance known as rubichloric acid is present, not only in the flowers but in the stem and root. It forms a colourless solution in water, but when boiled with a few drops of hydrochloric acid, suddenly produces a deep blue, then a green colour, and deposits a dense, dark green precipitate soluble in ammonia. The disc florets of the daisy are tinged with carotin granules; the crimson of the ray florets is due to a soluble pigment described as a tannin anhydride. The blue of the harebell and chicory is again a tannin derivative.

Two colouring matters are engaged in the decoration of the primrose. At the base of the petal-limbs, where the tint is more deeply orange, carotin granules are present; the rest of the corolla contains a pale yellow, soluble pigment. Although the tints are comparatively feeble, chemical analysis shows that the plant is capable of "an infinitely richer wealth of coloration" than it shows in our climate, since "it seems almost impossible to exhaust the flower heads of substances which yield vivid and powerful orange and yellow dyes."

On Willows

The March number of the *Botanical Gazette* opens with a useful paper by C. J. Chamberlain, entitled "Contribution to the life history of *Salix*." The author has worked out the development of the flower, and that of the microspores and macrospores, and their germination, the process of fertilisation, and the embryology in several species of willows growing in the northern United States.

In view of the results of the work of Treub, Nawaschin, and Miss Benson, on *Casuarina* and various genera of the so-called Amentiferae, considerable interest attaches to Mr Chamberlain's researches.

He finds that *Salix* shows none of those striking divergences from the usual course of events in the ovule, which have been demonstrated in the above-mentioned genera, and his results confirm previous views as to the primitive simplicity of the willow flower. A diligent search in buds, both of the male and female flowers, failed to reveal the slightest trace of a rudimentary perianth, such as might be expected were the floral simplicity the result of reduction. The path of the pollen-tube is perfectly normal, an entrance to the ovule being effected through the micropyle. The synergids have a strongly developed beak, and breaking through the embryo-sac, project into the micropyle and attract the pollen-tube.

Great difficulty was experienced in finding the antipodal cells in the embryo-sac; only in six cases out of several hundreds examined were they clearly seen. This leads the author to suggest that their reputed absence in *Casuarina* may be due to a similar difficulty. The course of cell-division, in the development of the embryo from the fertilised ovum, shows several differences in detail from that described by Hanstein for *Capsella*.

Some interesting sports were also noted. Besides the well-known mixed catkins, which were observed in considerable number on a vigorous plant of *S. glaucophylla* in three successive seasons, the writer describes and figures the growth of stamens inside the ovary, an occurrence hitherto unknown in *Salix*. These intra-ovarian stamens consisted generally of a one-celled stalked or sessile anther, borne on the wall of the carpel or on a placenta-like outgrowth. Occasionally these ambisporangiate ovaries had a perfectly natural appearance, but generally they were more or less deformed. The ovules were generally inverted (anatropous), their normal form; but occasionally upright (orthotropous) ovules were found, sometimes borne on a long stalk. Quite normal embryo-sacs and embryos were produced in these sporting pistils. Masters in his "Teratology" mentions only one authenticated case of the formation of stamens within the ovary cavity, namely in *Baeckea diosmifolia* (a member of the myrtle family). Certain ovaries of quite normal external appearance contained numerous perfect or sometimes imperfect stamens, but no trace of any ovules.

Some Basic Dyke-rocks from Southern India

A recent issue of the *Records of the Geological Survey of India* (vol. xxx. part i.) contains a paper by Mr T. H. Holland dealing with certain basic dykes widely distributed in Peninsular India. On both geological and petrographical grounds they are correlated with the Cuddapah lava-flows, and they shew none of the effects of dynamic metamorphism so general in the older series of dykes referred to in the Dharwar system. The author treats the rocks under three groups: olivine-norite, augite-norite, and augite-diorite. The first consists essentially of olivine, enstatite, augite, and a basic plagioclase, with subordinate biotite. The second lacks olivine, and has more augite relatively to enstatite. In the third group biotite and usually enstatite have disappeared, a comparatively late crystallization of augite gives a tendency to the ophitic structure, and interstitial micropegmatite (sometimes with potash-felspar) is invariably present. Considerable variations occur, including transitions in the first group to peridotites and in the second to pyroxene-rocks. To each group there are 'hemicrystalline' equivalents corresponding with the rocks of plutonic habitus, and some of these present types not hitherto described but comparable with the limburgites or magma-basalts. These contain phenocrysts of olivine, of enstatite, or of both minerals in a dark compact matrix, largely of glass. The author also points out resemblances between the dyke-rocks and the Cuddapah lavas of which they are the probable equivalents.

The rocks styled augite-diorites seem to be practically identical with some which in this country have been termed quartz-bearing gabbros, etc., and the author makes an interesting comparison between the Indian examples and those of Carrock Fell, St David's Head, and Carlingford, with the well-known rocks of Penmaenmawr and the Whin Sill. He finds no evidence to support Professor Sollas' suggestion that the micropegmatite in such rocks is the result of a later injection of a distinct acid magma into minute veins and druses. He regards it as the final product of crystallization of the rock formed under somewhat changed ('aqueo-igneous') conditions, consequent upon the concentration of the original water in the residual magma. He suggests that distinct veins of granophyre traversing the rock, as described by Sollas in the Carlingford district, may be 'contemporaneous veins' rather than later injections, and that even the considerable bodies of granophyre so often associated with gabbro in many districts may be due to the squeezing out of the residual magma under greater pressure. Such a hypothesis is certainly in accord with conclusions reached by quite different lines of petrological research, and has the advantage of bringing into relation a number of facts which are already well known. In this view differences of pressure due to more or less deep-seated consolidation are held to account for the association of the pyroxene-plagioclase-aggregate and micropegmatite, sometimes as contiguous and obviously cognate bodies of rock, sometimes as constituents of one and the same rock.

Taking Time by the Forelock

In these days when a determined effort is being made to settle once for all the dates of publication of generic and specific names, and when bibliographers like Mr Sherborn are devoting themselves to finding out the times of issue of classical works issued at irregular intervals and with no record of their appearance, one is horror-stricken to find a contemporary, held in high esteem by reason of the beauty of its illustrations and the scientific character of its letterpress, doing its best not merely to conceal the dates of the appearance of its fascicules, but actually endeavouring to give a false impression of punctuality.

The part of the *Journal de Conchyliologie* for October 1895, and dated '1st October 1895,' appeared in this country on 3rd Sept. 1896, and contained the descriptions of several new species.

In the issue for Jan. 1896, ostensibly published '1st Jan. 1896,' but received on 21st Dec. 1896, these are referred to in the following form: "[Genus, species, author], *Journal de Conchyliologie*, vol. xxxiii., 1895," thus giving the impression that they were published before they actually were, and laying traps for the unwary priority-student of the future. We have every sympathy with journals which try by prompt publication to give their contributors the fair reward of their labours, but—'fair play is a jewel.'

I.

On the Distribution of the Pelagic Foraminifera at the Surface and on the Floor of the Ocean.

THE pelagic Foraminifera play a most important rôle in the economy of the present ocean, as well as in the geological history of our planet. Living specimens of these pelagic Protozoa are distributed everywhere in the surface waters of the open ocean, about fifteen or twenty species being met with in the surface waters of the tropics, and one or two dwarfed species are captured among the floating icebergs of the Arctic and Antarctic regions. The dead shells of these Foraminifera make up by far the larger part of the carbonate of lime present in the deep-sea deposits known as Pteropod and Globigerina Oozes, which cover about 50,000,000 square miles of the ocean's bed. In addition, they make up the major part of the carbonate of lime present in the other deep-sea deposits, such as Diatom Ooze, Radiolarian Ooze, Red Clay, and the deeper terrigenous deposits which are laid down in close proximity to continents and oceanic islands. Indeed, it may be said that, taken as a whole, nine-tenths of the carbonate of lime in marine deposits from depths greater than one hundred fathoms is derived from the dead shells of the pelagic Foraminifera.

When the *Challenger* set out on her cruise around the world, all the naturalists of the expedition believed that the habitat of the *Globigerinae* was on the sea-bed in deep water. This opinion was held by Wallich,[1] Carpenter,[2] and Wyville Thomson.[3] Gwyn Jeffreys,[4] however, took another view; he regarded the *Globigerinae* as surface organisms, and the Globigerina Ooze as made up of dead shells which had fallen to the bottom from the surface waters. The fact that M'Donald[5] and Major Owen[6] had captured several species of these Foraminifera in tow-nets at the sea-surface appears to have

[1] The North Atlantic Sea-bed. London, 1862; also Deep-Sea Researches on the Biology of Globigerina. London, 1876.

[2] *Proc. Roy. Soc.*, vol. xxiii. p. 234. 1875.

[3] *Proc. Roy. Soc.*, vol. xxiii. p. 32. 1874.

[4] *Proc. Roy. Soc.*, vol. xviii. p. 443. 1869.

[5] See Huxley's Appendix to Dayman's Report on Deep-Sea Soundings in the North Atlantic made in H.M.S. *Cyclops* in June and July 1857. London, published by the Admiralty, 1858.

[6] *Journ. Linn. Soc. Lond.*, vol. ix. (Zool.), p. 147. 1866.

been overlooked or forgotten. Huxley[1] discussed this question, and, while not coming to any definite conclusion on the subject, he held that the balance of evidence was in favour of those who maintained that the *Globigerinae* lived on the bottom of the ocean.

During the first few months of the *Challenger* Expedition the attention of the naturalists was almost wholly taken up with the

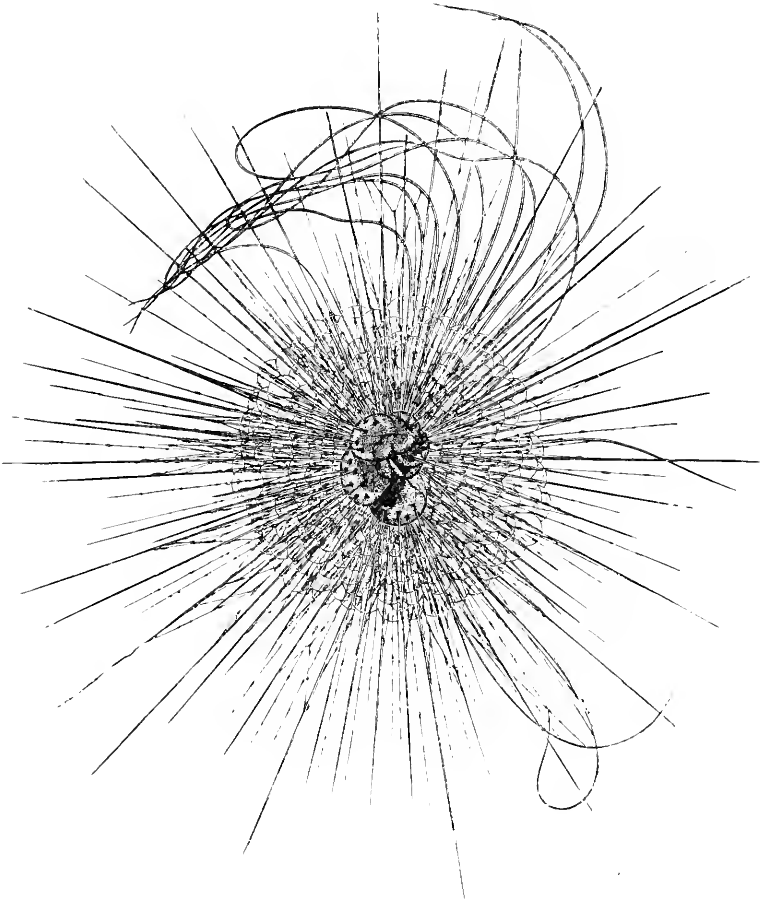


FIG. 1.—*Hastigerina pelagica* (d'Orbigny) [*murrayi*, Wyville Thomson] with floating apparatus and pseudopodia extended, as found floating on the surface.

examination of the deep-sea organisms obtained in the trawl and dredge, and with the larger animals procured at the surface. When, however, the expedition entered the tropics I frequently observed *Globigerinae, Orbulinae, Pulvinulinae,* and *Sphæroidinae* at the bottom of the glass globes into which the contents of the surface-nets were washed, and the attention of Wyville Thomson and the other

[1] Appendix to Dayman's Report, already cited.

naturalists was called to the fact. It was Wyville Thomson's opinion, however, that these shells really came from the deep-sea deposits. It was the custom to sift and wash large quantities of the ooze procured in the dredge on the deck of the ship, and it was believed that some of the shells from the deck being washed overboard were subsequently caught by the tow-nets dragging astern. But the appearance of the shells taken in the tow-nets was so different from that of those procured from the bottom that I could not accept the above explanation. When the weather permitted, the tow-nets were dragged, at considerable distances from the ship, from a rowing boat, and Foraminifera were procured in abundance. By using a water-glass I was sometimes able to dip up a single specimen in a glass beaker without in any way touching it. When this specimen was taken on board the ship, and placed under the microscope, the whole sarcode of the animal was to be seen expanded outside of the shell, as represented in Fig. 1. When our attention was once directed to the subject, the pelagic Foraminifera were observed in almost every haul of the tow-net. Many of the *Globigerinae*, the *Orbulinae*, and the *Hastigerinae* are furnished with long spines, and when the animal is expanded the sarcode rests between the spines. In the *Pulvinulinae*, the *Sphaeroidinae*, and *Pulleniae*, which have no spines, the shell is frequently so hidden in the expanded yellow-coloured sarcode that it may escape observation.

On the return of the *Challenger* Expedition, the late Mr H. B. Brady[1] and others pointed out that, if the *Globigerinae* were pelagic organisms, it was a most extraordinary circumstance that no naturalist had recorded them in any of the numerous tow-net gatherings about the British coasts. This, however, quite agreed with the experience of the *Challenger* naturalists. Whenever the ship entered a bay, an estuary, or, indeed, any coastal waters, the pelagic Foraminifera became very rare or entirely disappeared from the nets, although they may have been abundant fifty miles from the coast. I have never seen a single specimen in the tow-nets around the coasts of Scotland. In the *Triton* and *Knight Errant* Expeditions pelagic Foraminifera were found in abundance in the Gulf Stream waters which flow up the Faroe Channel, although not a single specimen was observed in the Minch or North Sea waters. The pelagic Foraminifera are truly oceanic creatures, even more so than the Pteropoda: they are most abundant in true oceanic currents; where these currents flow directly towards a coast they may be borne close to the shore, but usually they are only to be met with far out at sea.

From an examination of the large number of microscopic pre-

[1] *Quart. Journ. Micro. Sci.*, vol. xix., N.S., p. 292. 1879; see also *Zool. Chall. Exp.*, part xxii., pp. ix-xv. 1884.

parations and tow-net gatherings made during the *Challenger* Expedition, the following species of Foraminifera have been recognised as pelagic:—

Globigerina sacculifera, Brady.
,, *aequilateralis*, Brady.
,, *conglobata*, Brady.
,, *dubia*, Egger.
,, *rubra*, d'Orbigny.
,, *bulloides*, d'Orbigny.
,, *inflata*, d'Orbigny.
,, *digitata*, Brady.
,, *cretacea*, d'Orbigny.
,, *dutertrei*, Brady.
,, *pachyderma* (Ehrenberg).
,, *marginata* (Reuss).
,, *linnaeana* (d'Orbigny).
,, *helicina*, d'Orbigny.

Orbulina universa, d'Orbigny.
Hastigerina pelagica (d'Orbigny).
Pullenia obliquiloculata, Parker and Jones.
Sphaeroidina dehiscens, Parker and Jones.
Candeina nitida, d'Orbigny.
Cymbalopora (*Tretomphalus*) *bulloides* (d'Orbigny).
Pulvinulina menardii (d'Orbigny).
,, *tumida*, Brady.
,, *canariensis* (d'Orbigny).
,, *micheliniana* (d'Orbigny).
,, *crassa* (d'Orbigny).
,, *patagonica* (d'Orbigny).

Cymbalopora bulloides[1] (Fig. 3) can hardly be regarded as a true pelagic Foraminifer. It was only captured in the neighbourhood of coral reefs, and the curious thing about it is that not a single specimen was taken containing ordinary sarcode, similar to that observed in the other species of pelagic Foraminifera. In all the specimens the shells were filled with immense numbers of minute zoospores; these latter spread over the field of the microscope in a cloud-like swarm when a shell was broken under the cover-glass.

The usual colour of the sarcode of the pelagic Foraminifera is yellowish-brown. In *Hastigerina* it is bright red from the presence of red coloured oil-globules and pigment. This red colour enabled me to pick up this species with a beaker on the sea-surface more easily than other species. In *Globigerina bulloides* (*hirsuta*) and *aequilateralis* the yellow-orange colour of the sarcode is due to the presence of numerous oval-shaped xanthidiæ, or 'yellow cells,' similar to those found in the Radiolaria. When the sarcode with these 'yellow cells' flows out of the foramina, and mounts between the numerous spines outside the shell, the whole presents a very striking object under the microscope; the transparent sarcode can be seen running up and down the long silk-like spines, and the 'yellow cells,' seated at the base of these spines, quite obscure the body of the shell.

The majority of the species in the above list occur within the tropics, and the thick-shelled species occur only in warm water, such as *Sphaeroidina dehiscens*, *Pulvinulina menardii* (Fig. 4), *Pullenia obliquiloculata*, *Globigerina conglobata* and *sacculifera*. The number of species becomes less in the temperate regions, *Pulvinulina micheliniana* and *canariensis*, *Orbulina universa* (Fig. 2), *Globigerina bulloides* and *inflata* being the prevailing forms. In the Arctic and Antarctic

[1] See *Narr. Chall. Exp.*, vol. i., pp. 838-9, 1885.

regions *Globigerina dutertrei* and *pachyderma*, together with very minute specimens of *Globigerina bulloides*, appear to be the only forms present in the surface waters. The gradual disappearance of the tropical species, and their replacement by other species, as the colder water to the north and south of the equatorial regions is entered, has

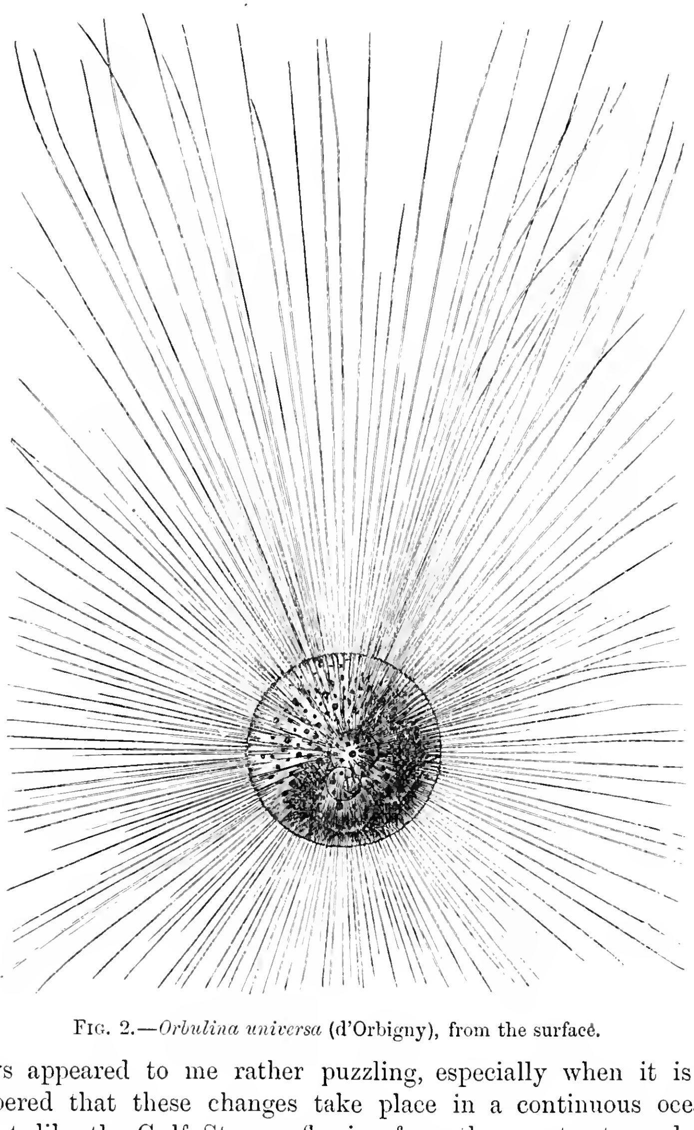


Fig. 2.—*Orbulina universa* (d'Orbigny), from the surface.

always appeared to me rather puzzling, especially when it is remembered that these changes take place in a continuous oceanic current, like the Gulf Stream, flowing from the equator towards the poles. It sometimes seemed as if the one form slowly passed into the other with the changed conditions of surface temperature.

The same species inhabit all the great oceans, but in the Indian and Pacific Oceans certain species appear to predominate, for instance, *Pullenia obliquiloculata* and *Globigerina aequilateralis;* on the other hand, *Pulvinulina menardii* and *Globigerina rubra* appear to be more abundant in the tropical Atlantic.

The species inhabiting the north and south temperate regions, and the species inhabiting the two polar regions, appear to be nearly if not quite identical.

The distribution of the dead shells of the pelagic Foraminifera on the floor of the ocean corresponds exactly with the distribution of the living specimens at the surface of the sea. It has sometimes been urged that the dead shells of tropical species might be carried a long way to the north or to the south by oceanic currents, but this does not seem in any way to be the case; the distribution of the dead shells on the bottom does not appear to be much if any

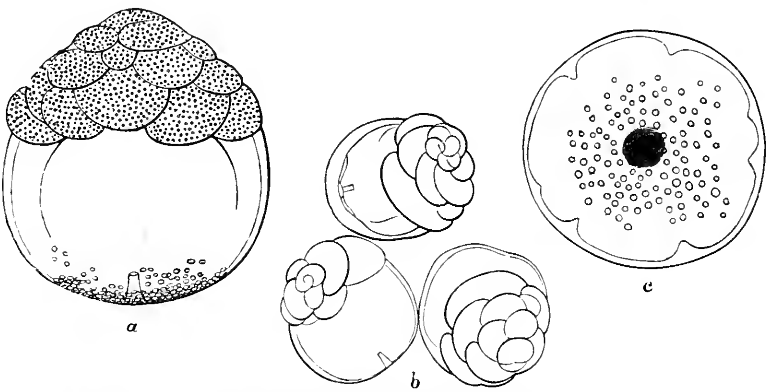


FIG. 3.—*Cymbalopora* (*Tretomphalus*) *bulloides* (d'Orbigny). *a*, large surface specimen; *b*, small (young) specimens from the same gathering; *c*, distal face of the balloon-like chamber, showing the entosolenian orifice, seated in a slight depression. All magnified 60 diam.

wider than that of the living specimens at the surface, and this shows that the dead shells must reach the bottom a very short time after the death of the organisms. The fact that the distribution of these shells at the bottom of the ocean is governed by the surface conditions is of itself almost conclusive proof that they live only at the surface, for otherwise their distribution would be similar to that of bottom-living, or benthos, species, which is wholly independent of the temperature conditions prevailing at the surface of the sea. Carpenter[1] and Brady[2] at one time held the view that young individuals lived at the surface and adult ones at the bottom; in addition to the fact that no living specimen has ever been obtained from the bottom, the above considerations with regard to distribution show that this view is not supported by any trustworthy observations. In the surface gatherings the young individuals are much

[1] *Op. cit.* [2] *Op. cit.*

more abundant than adult ones, still shells as heavy as any in the deposits are occasionally taken in the surface-nets. The young individuals are likewise more abundant at the surface than in the deposit, when compared with the adult shells present; this is especially the case in deposits from very deep water. This arises, as we shall see, from the more rapid solution of the young shells as they fall through the sea-water to the bottom.

When examining a deep-sea deposit it is always possible to say, from a study of the pelagic shells of the Foraminifera, whether the sample comes from the tropics, the temperate or the polar regions, but from the examination of these shells alone it would be extremely difficult to say whether the specimen was from the northern or southern hemisphere.

Off the Agulhas Bank at the Cape of Good Hope, off the east coasts of Australia and Japan, and off the east coasts of North and South America, oceanic currents from different sources meet and

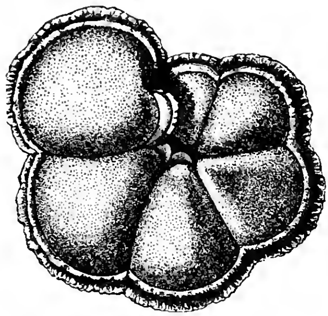


Fig. 4.—*Pulvinulina menardii* (d'Orbigny), from the tropical deposits.

mix, and there is a wide range of annual temperature at the surface. In these positions large numbers of pelagic Foraminifera (as well as other organisms) appear to be killed by the sudden changes of temperature, and consequently there are indications that the deposits, so far as due to these shells, are accumulating more rapidly in these areas than in other situations. It is a curious fact also that in these regions the deposits of glauconite and phosphatic nodules are more abundant than elsewhere.

In a certain sense the course of a surface oceanic current can be traced on the bottom by means of these dead pelagic shells; for instance, the axis of the Gulf Stream is marked out by deposits of Globigerina Ooze from the Strait of Florida to within the Arctic circle. No similar warm current enters the Antarctic region, and consequently no true Globigerina Ooze is found to the south of lat. 50° S. When the *Challenger* took her first deep-sea sounding after leaving Heard Island (in lat. 60° S.) there was much speculation as to what the nature of the deposit would be. I ventured to say that it would not be a Globigerina Ooze, founding that opinion on the fact that only one or two small Foraminifera had been observed in the tow-nets for several days. When a white-coloured deposit was brought on board from 1260 fathoms the laugh was rather against 'the philosophers,' for in external appearance it greatly resembled the calcareous oozes of the Atlantic. On examination, however, it was found to be a Diatom Ooze with only relatively few *Globigerina* shells.

The most striking peculiarity in connection with the distribution of these dead shells on the floor of the ocean is the fact that they are wholly absent from all the greater depths of the ocean, although at the surface their living representatives are as abundant over these deep areas as elsewhere. If we suppose a volcanic cone to rise from the greater depths of the ocean up to within 400 or 300 fathoms of the surface, it will be found that the summit of this cone is covered with a calcareous deposit for the most part made up

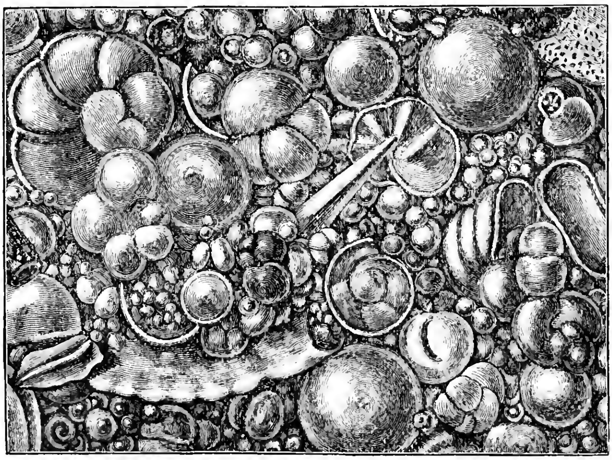


Fig. 5.—Globigerina Ooze, from 1900 fathoms in the Atlantic; magnified 25 diam.

of the dead shells of pelagic organisms, the deposit may contain 90 per cent. of carbonate of lime, and in it every species of shell met with in the surface waters of the region is represented. As we descend the sides of this cone into deeper water, the thinner and more delicate shells, like *Candeina* and *Hastigerina*, disappear first from the deposit (along with the Pteropod shells). In about 2000 fathoms the deposit consists chiefly of pelagic Foraminifera, and the proportion of young shells is much smaller than in the deposits at lesser depths. With increasing depth the whole of these calcareous shells gradually disappear, till at 4000 and 5000 fathoms probably not a vestige of them can be traced, and the deposit all round the cone in 3000 fathoms becomes a Red Clay with only traces of carbonate of lime in its composition. Again, if we suppose a basin-like depression on the floor of the ocean, the centre of which descends to 4000 or 5000 fathoms, while the rim of the basin has only a depth of 1000 or 2000 fathoms, then, on the rim deposits of Pteropod and Globigerina Oozes will be found with 70 or 80 per cent. of carbonate of lime, while the centre of the basin will be occupied by a Red Clay with probably not a trace of these carbonate of lime shells. The gradual disappearance of these calcareous shells with increasing depth is evidently due to the solvent action of sea-water, and especially of deep-sea water. In the lesser depths a very large proportion of these surface shells seem to reach the bottom before they are completely dissolved, and accumulation takes place. With increasing depth the more delicate shells are dissolved before reaching the bottom, and accumulation becomes slower and slower, the last traces of these shells observed in the deposits with increasing depth being broken fragments of large *Pulvinulinae* and *Sphaeroidinae*. The greater quantity of lime in solution which Dittmar found in the *Challenger* samples of deep-sea water is apparently a consequence of the solution of the pelagic shells here referred to.

During the early part of the *Challenger* Expedition, Wyville Thomson was much puzzled to account for the origin of the fine Red Clay which occupies the basin-like depressions of the sea-bed far from land, and he suggested that this was an *ash* [1] left behind after the solution of the carbonate of lime shells. He was led to this view by observing that when the shells were taken from the purest samples of Globigerina Ooze, and, after being carefully washed with pure water, were dissolved with dilute acid, a small clayey residue of a red colour remained behind. I was not satisfied with this experiment, for I observed that the colour of the residue varied in different samples, and it seemed to me that the fine clayey matter had infiltrated the shells after they had reached the bottom. I accordingly collected, in the course of several months, about 10 grammes of pelagic Foraminifera from the surface of the sea. When these shells were dissolved in dilute acid not a vestige of residue was observed. It was subsequently shown that the Red Clay came from a variety of sources, and that in the deep sea far from continents it was chiefly derived from the trituration and decomposition of floating pumice.[2]

During the past year or two I have carefully collected all the available temperatures of the surface waters of the ocean, and from these have constructed a map showing the annual range of tempera-

[1] *Proc. Roy. Soc.*, vol. xxiii., p. 45. 1874.

[2] See Murray, *Proc. Roy. Soc. Edin.*, vol. ix., p. 247. 1876; also Murray and Renard, *Deep-Sea Deposits Chall. Exp.*, p. 294. 1891.

ture in different regions of the ocean. This map shows that the surface of the sea may be grouped into five great zones, viz.:—(1) A nearly continuous equatorial zone, where the temperature is high and the range throughout the year does not exceed 10° Fahrenheit. This zone includes all the principal coral-reef regions. (2 and 3) Two polar zones, where the temperature is low and the annual range likewise does not exceed 10° F. In these zones there are relatively few lime-secreting organisms. (4 and 5) Two regions lying between the equatorial zone and the two polar zones, where a wide range of temperature occurs between the different seasons (the annual range amounting to as much as 52° F. in some places). In these temperate regions the secretion of carbonate of lime appears to be much more active in the warmer than in the colder months. It thus appears that the most favourable conditions for lime-secreting organisms are met with in the warm, equable tropical waters of the ocean, and here as a matter of fact we find the greatest development of corals, and the largest number of lime-secreting pelagic organisms. In the polar areas and in the cold water of the deep sea there is, as is well known, a feeble development of all carbonate of lime structures in marine organisms.

From experiments which have been carried out by Mr Irvine and myself at the Granton Marine Station, we have reason to believe that this distribution is dependent primarily on the physical or temperature conditions of the oceanic waters. When carbonate of lime is precipitated by alkaline solutions, such as carbonate of soda, carbonate of ammonia, or carbonate of methylamine, the effect of temperature is very marked, and it appears to be the case that the secretion of carbonate of lime by organisms is of the nature of a fine precipitation in the interior of the soft structures.[1] If we add sufficient carbonate of ammonia to sea-water at different temperatures to convert all the lime salts present into carbonate, we obtain a precipitate which varies both in its crystalline form, in amount, and in time of formation. At 32° F. the precipitate begins to form in about six hours as small but distinct crystals of calcite, the quantity in twenty hours amounting only to 0·2 gramme from a litre of water. At a temperature of about 47° F. a mixture of calcite and aragonite is precipitated; at 80° to 90° F. the quantity precipitated is about 0·6 gramme, the precipitate begins to form in from a half to one hour, and it appears to consist of minute crystals of aragonite. It thus seems evident that carbonate of lime would be more easily and more rapidly secreted in the high temperatures of the tropics by means of the effete products of the organism.

As is well known, carbonate of lime in any form is easily soluble in water containing carbonic acid, and the aragonite form is more

[1] Murray and Irvine, *Proc. Roy. Soc. Edin.*, vol. xvii., pp. 79-109. 1890.

quickly soluble than the calcite form in the proportion of about three to two. Both aragonite and calcite are apparently very partially soluble in sea-water which does not contain free or loosely-combined carbonic acid, but when these dead shells are in contact with decaying organic matter, giving off carbonic acid, they are rapidly dissolved. An experiment with Globigerina Ooze in a sea-water containing additional carbonic acid showed that the thin walls of the chambers of the shells were first dissolved, leaving rings of the thicker portions of the *Pulvinulina* shells, for example. Decaying organic matter has a powerful solvent action on carbonate of lime, due to two causes: (1) by the carbonic acid formed as one of the products of this decay, and (2) on account of the formation of sulphides and sulphuretted hydrogen, due to the reduction of the sulphides present in sea-water.[1]

JOHN MURRAY.

[1] See Murray and Irvine, *op. cit.*, table on p. 106.

II.

A Californian Marine Biological Station.

THE European zoologist who visited the Pacific states would be very apt to find his way to the old Spanish-Californian town of Monterey, and to the Marine Laboratory of the Leland Stanford Junior University. As this station, however, seems to the present writer surprisingly little known in proportion to its deserts, a brief account of its equipment and surroundings may prove of interest to the readers of *Natural Science.*

This at the present time is the only permanent biological station on the American side of the Pacific. Temporary stations have indeed been established within recent years. The University of California has several times carried on a seaside school of zoology, both at Pacific Grove near Monterey, and on the Santa Catalina Islands in the region of Santa Barbara. Further northward, in Puget Sound, Washington, a local society, that of the Young Naturalists of Seattle, has done excellent faunal work during its camping seasons; and in the same region during last summer Columbia University of New York established a laboratory at Port Townsend.

The Stanford, or the Hopkins Laboratory, as it is called, is both an annexe and an integral part of its university. It was, indeed, contemplated as early as the time of the building of the university, when it was decided that a portion of the studies in zoology and botany might be carried on during the summer, the students to be given the regular credit for their work as in the winter courses. It was, accordingly, with a summer laboratory in view, that in 1891 two of the Stanford professors, Drs O. P. Jenkins and C. H. Gilbert, visited the region of Monterey (which had indeed been known to Dr Gilbert previously during his studies on the fishes of the Pacific), and made a reconnaissance to determine the particular point of the bay which was best suited to the needs of the collector and investigator. The site they then determined upon was at Pacific Grove, a few miles westward of Monterey. Here, in the first place were found most favourable fields for collecting. The shores were unshifting, the coast was rugged, while huge rock masses and bluffs alternated with sheltered harbours and beaches, rich in forms of animals and seaweed life. The locality seemed also a particularly convenient one on account of its facilities for the lodging and living of students, a summer settlement of possibly five hundred people being in the immediate neighbourhood. It was here finally that land was obtained, a gift of the Pacific Grove Improvement Company, and the buildings were shortly put up and equipped, thanks to the generosity of Mr Timothy Hopkins, after whom the laboratory has been named.

The buildings are shown in the adjoining figure (Fig. 1), but

Fig. 1.—The Hopkins Seaside Laboratory, near Monterey, California. East view.*

the picture gives only a slight idea of their surroundings; thus they are seen to be built on a level field, and there is but a glimpse of the sea in the background. One needs, therefore, to imagine the laboratory site as a small treeless plateau, on the top of an abrupt rocky point which terminates about a hundred yards to the right in the picture. The sea surrounds the buildings, therefore, on three sides. In front there is a sheltered harbour and a small sandy beach, furnishing an admirable landing place for the boats; at the back the surf is breaking on the rocks thirty feet below—hardly far enough away as it has been proved, for in the winter storms the waves have threatened to overturn the buildings, and have rendered necessary the additional braces which one sees at the corners of the building. From its position the laboratory becomes a prominent feature of the entire neighbourhood. The visitor will not fail to notice it even at the incoming of his train, for he naturally will be looking seaward after his three hours' journey from San Francisco. He will just have passed through the hot and dusty valley country, but his interest revives as the train emerges on the sea-coast at Monterey, thence to skirt the shore of the bay during the few final minutes of the trip.

The bay of Monterey appears not unlike that of Naples. There is the same long curving beach, broken with rocky points, the clear

* The illustrations have been prepared by Mr Percy Buckman from photographs taken by the author.

blue water, and the same setting of half tropical vegetation, although the mountainous background is lacking. The climate is here less variable than at Naples; the temperature remains almost constant throughout the year, each day averaging about 60°F., and during six of the months outdoor life is not interrupted by rain. The railroad line terminates at Pacific Grove. Here on one side of the railroad are bluffs and the rocky point on which the laboratory is situated, while on the other a tidy little town, with well kept villas, bright shops, lines of tents for the summer campers, a good hotel, and a small park-like square, rich in the deep greens and light olives of Californian plants. All about are scattered forest trees—live oaks, tall pines, eucalyptus and palms. With these are numerous trees and hedges of the Monterey cypress (*C. macrocarpa*), whose very restricted range gives it an especial interest.

Point Aulon, the little promontory on which the laboratory is situated, juts out from the western end of the town. It has been

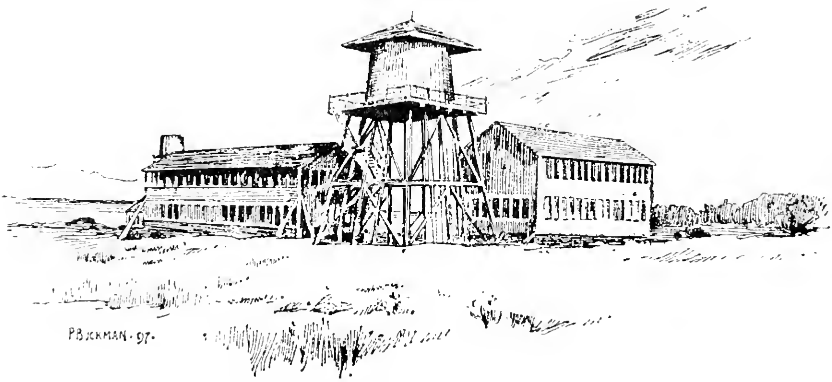


Fig. 2.—The Hopkins Laboratory. West view.

fenced off as a kind of marine park for the cottagers; and here at all times throughout the summer may be seen clusters of people, old and young, idling away their holidays, clambering about the rocks, or watching the ceaseless strings of cormorants, or the doings of the little school of boats huddled closely together off the point salmon catching, or the return of the little fleet of Chinese fishermen, whose curious town may be seen on a projecting coast point in the direction of Monterey. Such a thing as the sight of an occasional whale or sea-lion, and these will come surprisingly near the point, or even the loss of a straw hat, will cause a flutter of excitement among the summer visitors, diverting their attention, as a student will uncharitably believe, from their attempts to invade the penetralia of the laboratory.

Our second illustration (Fig. 2) gives a west view of the two buildings. The older, used during the first and second sessions of the summer school in 1892 and 1893, stands to the left, long, and with many windows. It measures sixty feet long by twenty feet wide. The newer, smaller, but more substantial building measures forty by twenty-six feet. From the figure one may also see the two large salt-water tanks, which have been so arranged that each can supply either building. The older building is now used mainly for the classes of elementary students. It has two laboratories on the ground floor, a small engine room, and a concreted workshop, which serves as a dissecting room for the larger marine beasts. Upstairs a long laboratory faces the east, and on the south side a series of small separate rooms have been arranged for investigators. In the newer building a laboratory occupies the rear end of the ground floor, used during last summer mainly for students in the botanical courses; and on the floor just above there is a room of the same size, with blackboards, cases, and portable tables, used both as a lecture room and laboratory. The front part of the house in both storeys is divided by partitions into a dozen rooms for investigators, and it has, in addition, a photographic dark room. Throughout both buildings the fittings are simple but adequate. There is an abundant supply of microscopes, reagents, glassware and the usual set of dredges, tangles, and nets, a small beam-trawl, and apparatus for sounding and temperature-taking. At present the boat facilities include only a rowing boat and a small sailing boat, the latter almost too small for dredging or trawling, except in comparatively shallow water. Hitherto, however, the laboratory seems scarcely to have needed collecting facilities for the deeper water—enough at least to warrant the support of a steam vessel. The shore fauna has been of the richest, and dredging in shallow water could well be done with the boat at hand. As a convenient means of collecting in the shallow rocky bays a water-glass has been found of great service, especially in securing conspicuous forms such as echinoderms and holothurians, and has to a certain degree served as a substitute for diving apparatus, which here, as at the French marine station at Banyuls, might well prove of the greatest value. The station has never found difficulty in securing an abundant supply of fish material, thanks to the Chinese fishermen of the neighbouring village.

A whole article might be written on this small Chinese village near Monterey. It is but a quarter of an hour's walk from the laboratory, approached along the ledge of the railroad on the seaside rim of the town—a daily walk for a number of the students, who have come to have the greatest faith in the fishing powers of the heathen. This walk is by no means an uninteresting one; the sea-birds are around, whitening the tall rocky ledges, and on every hand there are quantities of little ground squirrels—a species of spermophile—which sit up before the visitor like little prairie dogs.

The village itself looks as though it has been imported from China in its present condition, a huddled little town of unpainted shanties sprinkled closely along a crowded street, with a few shops, a joss-house, and a sky-line of picturesque scaffolding for fish-drying. There will be a crowd of mushroom-hatted fishermen, a din of chaffering, a mixture of nets, trawl lines and baskets, distinctly unpleasant odours, placards of crimson and tinsel spattered with Chinese characters. The people are Cantonese, many of whom have been living here for two generations. They are classed as a peculiar poor grade of Chinaman, and are, I am told, looked upon at home as mere barbarians, if for no better reasons, that they have lived in China only two or three centuries, and are unable to trace their descent for more than seven generations. To the stranger, however, they certainly appear very industrious, honest (except in bargaining), kindly and painstaking. They are excellent fishermen, and in

Fig. 3.—The Chinese Fishing Village at Monterey. A corner of the beach.

several instances very intelligent collectors. Their little fleet of boats is often out before sunrise; between seven and ten they have become scattered along the coast, and their trawl lines are put out, often six or more (each about five-eighths of a mile long) to a boat; about noon-time they return, their skiffs sometimes gunwale-deep with fish-rock cod, black bass, flounders, mackerel, with an occasional wolf-fish,[1] their little latteen sails making the picture a still more foreign one. In a few moments after landing, the fish are carried off in shoulder baskets, to be shipped to San Francisco, and the boats are drawn high up on the beach (Fig. 3). The little colony also carries on a very successful squid-catching industry, so that at night there is often as much life and excitement in Chinatown as during the day. The amount of a catch will often be measured by tons.

[1] *Sebastodes* (*nebulosus*) and *melanops*, *Scomber* (*colias*), *Platichthys*, *Anarrhichthys*.

The boats go out with nets and red pine fires, which are hung cresset-wise over the sides of the boats to lure the squid. Some of these are intended to be cleaned and dried on latticed trays as a staple article of diet in Chinese markets. The bulk of the catch is, however, spread over the fields for drying, then to be packed in matting bags for export to China, as a rich fertilizer for the rice fields. Another phase of their industry is that of collecting abalones, *Haliotis*, these also to be dried for export. The people have their usual Oriental thrift,—they are infamous at a bargain, but make up this deficiency by the skill with which they separate the fertile or unfertile eggs of sharks or *Bdellostoma*, and recognise what they refer to as the 'hen' or 'rooster' sharks or rat-fish (*Chimaera*).

There is also another little imported village in this neighbourhood, nearer Monterey, namely, a settlement of Portuguese, who, like the Chinese, have retained minutely their foreign ways. Their boats are precisely those one would see in the Tagus, and, judging from the writer's experience in Portugal, he believes that the immigrants have not improved in the way of zoological collectors.

The laboratory has now completed its fifth season, and the work of last year seems to have been carried on very much in the lines of former years. There is a class in the dissection of types, and in the study of methods, limited to twenty or thirty students, each paying a stated fee for a term of six weeks. A second class includes advanced students in zoology, mainly from Palo Alto. The investigators, finally to be mentioned, occupy the private rooms in both buildings. These are afforded their quarters, reagents, and collecting facilities gratuitously. Class instruction is carried on by the professors of the Stanford University, during the present year by Doctors Jenkins, Shaw, and Wilbur. Among the investigators of the past summer were W. R. Shaw, working on the development of conifers, E. P. Wheeler, on the embryology of *Dicyema* and on diptera, D. A. Saunders, on the brown seaweeds, H. Heath, on the anatomy and development of *Chiton*, O. P. Jenkins, on contractility of muscles and conductivity of nerve-tissues in invertebrates, H. P. Johnson, on the annelids, and W. E. Ritter, on the ascidians, W. A. Setchell, on (laminarian) seaweeds, and the present writer, on the development of *Chimaera* and *Bdellostoma*. Many of these investigators have previously spent summers at the laboratory. Among the workers of former seasons might be mentioned H. Ayers, whose lecture on *Bdellostoma*, published in the volume of zoological lectures of Wood's Holl Laboratory (1893), has merited wide attention. Dr C. H. Gilbert, as the director of the station jointly with Dr Jenkins, has also been a constant visitor, and has here prepared no little part of his studies on the ichthyology of the Pacific. Dr E. C. Price, also one of the zoological staff at Palo Alto, was the first of the several workers at the laboratory to secure embryos of *Bdellostoma;* while on the botanical side F. M. M'Farland and L. H. Campbell have been in frequent attendance. President Jordan has also taken the warmest personal interest in the work of the station, and although his researches have hardly been carried on at Pacific Grove, he has, nevertheless, been a constant visitor.

It is evident, perhaps, from the foregoing pages that the zoological station of the Stanford University has neither the equipment nor the subsidies of the Stazione at Naples, or even, as yet, of the laboratory at Wood's Holl; but the zoologist will certainly find there all of the facilities for his work which can reasonably be needed. The warm interest which Dr. Jenkins has shewn in the welfare of each worker of the station will not be forgotten; and it is doubtless due in no small degree to this care that the visitor

FIG. 4.—The Coast of the Pacific at Cypress Point, near Monterey.

takes away with him the kindliest recollections of Stanford's hospitality. The richness of the fauna and flora of this little nook in the Pacific cannot fail to leave the strongest impression upon the visitor's mind. He will remember the rugged shore line, with its stunted and twisted cypresses (Fig. 4), the sunken rocks bristling with the largest sea-urchins, the bright-coloured star-fishes, the orange-red *Cucumaria*, a yard in length. So too the tidal rocks covered with *Pollicipes*, the clumps of palm-tree-shaped *Postelsia*, the tangled masses of bull-kelp (*Nereocystis luteana*), whose stems are often many fathoms in length; the field-like areas of *Macrocystis* (*M. pyrifera*); the rich molluscan fauna, including the red shelled *Haliotis*, to be found even at the base of the laboratory rocks, *Cryptochiton* (*C. stelleri*) seven inches long, and abundant nudibranchs. There is a wealth of ascidians, annelids and hydroids. Nor does this neighbourhood represent the vertebrates in a less interesting way. Mr Leverett M. Loomis, curator of the California Academy of Sciences, tells the writer that the coast line at Monterey is particularly prolific ornithologically; it includes among its common birds, cormorants, pelican, auklets, murres, and albatross. Among fishes there are several forms of especial interest. A species of *Chimaera* (*Hydrolagus colliei*) is plentiful in deeper water, twenty or more being a not unusual catch by a single boat. The hag-fish (*Bdellostoma stouti*) is one of the most common forms of the bay; and in some localities it is so abundant that it becomes a serious nuisance to the fishermen.

BASHFORD DEAN.

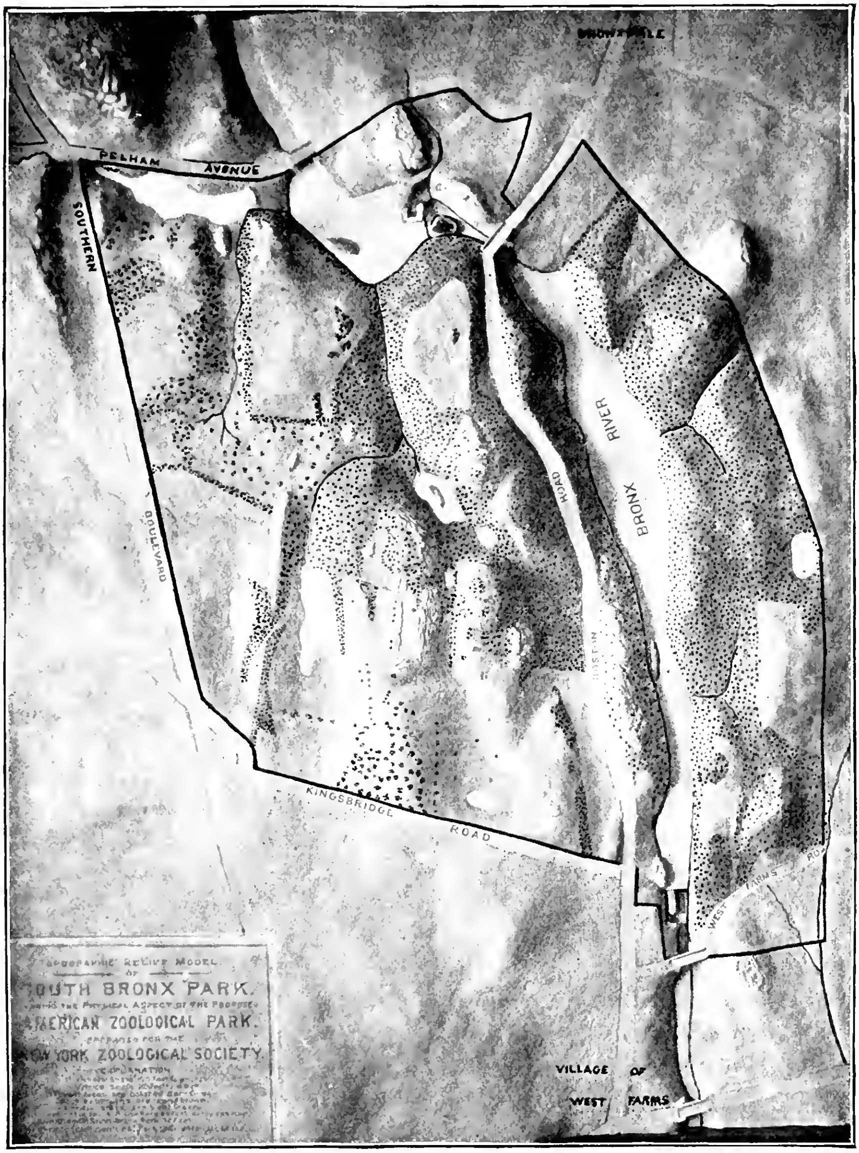
COLUMBIA UNIVERSITY, NEW YORK CITY,
April 1, 1897.

III

The Proposed Zoological Park of New York

IN the great city of New York, at the present time, the only place where living animals are kept for the edification and amusement of the public is what is called the 'Central Park Menagerie,' under the control, we believe, of the Commissioners of the Public Parks, which, though well kept and arranged, so far as it goes, is utterly unworthy of one of the largest cities in the world, which is adorned by so many and so various institutions. Several attempts have been made at different periods to start a zoological garden on a proper scale in New York, but it is only within the last few years that the subject has been taken up seriously, and in a manner which seems likely to produce definite results. So far, however, has the matter now progressed, that we have before us the first Annual Report of the New York Zoological Society, which received its charter in 1885, with the worthy objects of "establishing a free zoological park to contain collections of native and exotic animals," of "preserving the native animals" of the U.S. from further destruction, and generally of promoting the interests of zoological science.

Let us now see what the first Annual Report of the New York Zoological Society tells us about its progress and future prospects. Its Executive Committee appear to be mostly business men of New York, who are interested in the subject, but amongst them we note the name (as chairman) of Mr Henry F. Osborn, who is well known to all the scientific world as one of our leading authorities on mammals, and amongst the 'Scientific Council' we recognize the names of Prof. Allen, Mr F. M. Chapman, Dr T. H. Bean, and other gentlemen well known in scientific zoology. In Mr W. T. Hornaday the Council have made a choice as director for their Park, of another gentleman also well known in Europe, who possesses many excellent qualifications for the post. One of the first steps taken by the Committee—and a very wise one—was to send Mr Hornaday off to Europe for the purpose of inspecting and studying in detail the best zoological gardens of England and the Continent. Mr Hornaday's account of his tour of inspection, in the course of which he visited fifteen zoological gardens, will be read with interest, and we think we may say not without profit, by all those who wish



BIRD'S-EYE VIEW OF THE PROPOSED ZOOLOGICAL PARK AT NEW YORK

Photographed from the Society's Topographic Model

to make acquaintance with this subject. Five of the gardens visited, namely, those of London, Antwerp, Amsterdam, Hamburg and Berlin, are classed by Mr Hornaday as being of 'first rank,' and receive due praise for their success in various particulars. Warm thanks are also offered to the officials of these and the other gardens for the great help they have afforded to Mr Hornaday in his investigations. The principal criticism made upon these gardens by Mr Hornaday is that of want of space, many of them being so overcrowded with buildings and yards, that little attempt can be made to imitate the natural haunts of the creatures exhibited in them.

In European gardens, Mr Hornaday truly observes, "the large game—animals, such as the various species of deer, elk, bison, buffaloes, etc., are kept in small pens, because ample park-space is not available. Living trees are never utilized as homes for arboreal mammals. Ledges of natural rock are entirely absent, but hills of artificial rock, and small masses of stone, are quite common. With the exception of the great 'flying' cages of London, Rotterdam and Paris, the homes provided for birds are always of the most conventional and artificial character. The 'flying' cages, however, just mentioned are so very large, and contain so much of nature in the form of living trees, shrubs, plants and water, that the birds within them seem to be as much at home as if they were really in a state of nature, in a leafy wilderness."

We shall see presently that in the proposed new garden in New York, it has been wisely arranged that much more ample space shall be provided than is to be found in the existing establishments in the Old World.

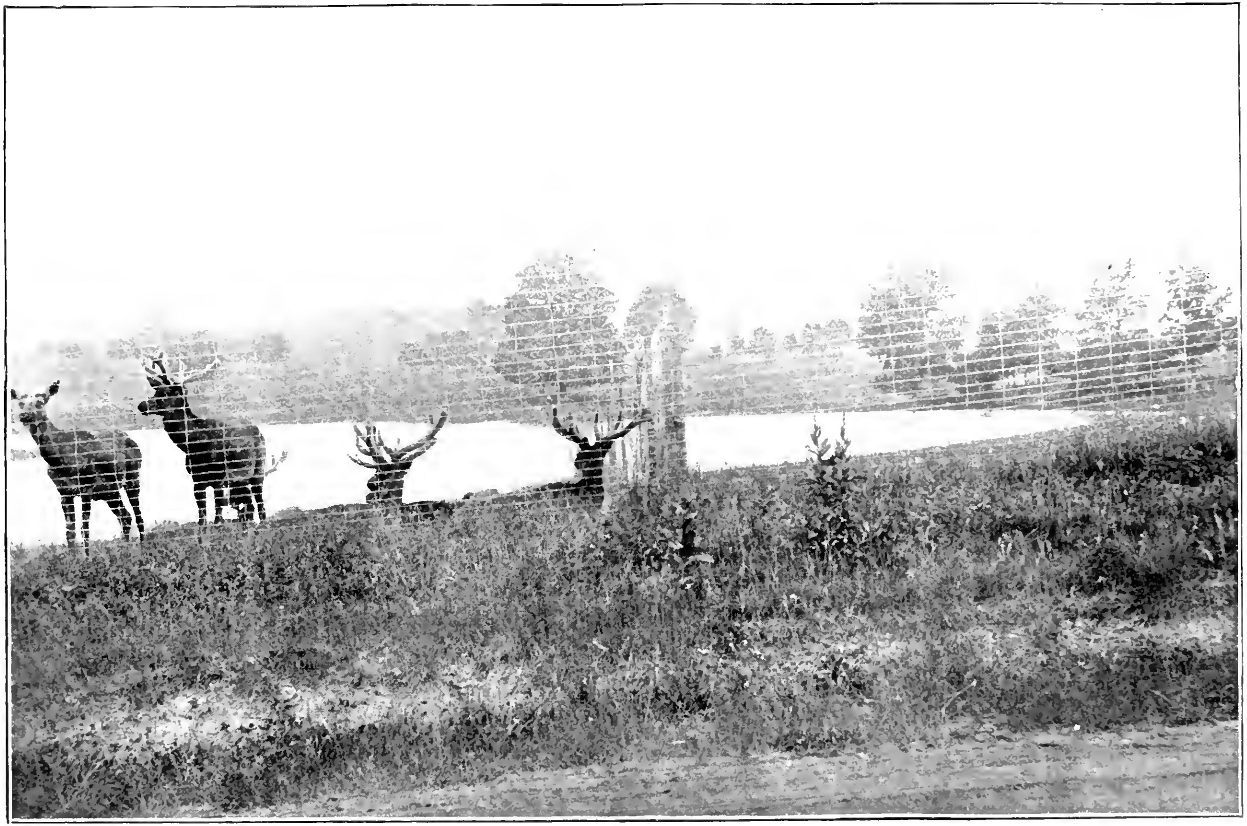
Mr Hornaday also speaks of the attempts made in some of the European gardens to provide oriental buildings for oriental animals, and buildings of an elaborate architectural design. It is his opinion, and we quite agree with him, that conformity to a plain and uniform style of architecture is more desirable in such matters than a "succession of startling contrasts."

But although we have derived much instruction from Mr Hornaday's Report, and from the appositeness of some of his remarks, it is time now to turn to what our enterprising American friends propose to do in order to found in New York a Zoological Garden, certainly better provided with space, and, if possible, better ordered in every other respect than those of Europe. In selecting for the site of the proposed new garden South Bronx Park, a tract of "261 acres of forest, meadow-land, and water," in the northern environs of the city, in what is called the "annexed district," the Executive Council appear to have made a wise choice. As will be seen by the bird's-eye view of the surface of this piece of ground, taken from a relief model of which by the kind favour of that Council we are enabled to give a copy herewith (Plate II.), the ground is sufficiently varied, well supplied with water, and provided with plenty of trees for shade. It also seems to be conveniently accessible by several lines of railway, electric and otherwise, although perhaps rather far distant from the great centres of New York population. But considering its large dimensions it could hardly be expected to be nearer. If, however, the proposed buildings are to be scattered about over so wide an area, it will become necessary, we think, ultimately, if not immediately, to provide some mode of locomotion from one building to another within the gardens.

The great extent of space available in South Bronx Park will of course be highly advantageous to the larger Ruminants, and we may expect that herds of deer, antelopes and other bovine animals will hereafter form one of its marked features. The manner in which the Executive Committee propose to deal with animals of this character will be seen by a second illustration which our friends have likewise provided for us (Plate III.), and which represents "Elk at sunrise, photographed from life." The following passages extracted from the Report inform us generally of the views of the Society regarding its proposed collection of living animals, in the wisdom of which we must all fully concur.

"As may fairly be expected, the first duty of the Society in the formation of collections, will be to bring together a liberal number of fine examples of the more noteworthy and interesting species of the animals of North America, particularly of those species that are threatened with extinction. No reasonable effort will be spared to show each species of the larger mammals under conditions of liberal space and surroundings which will at least suggest its natural haunts, which will promote the comfort and longevity of the captives, and render their contemplation by visitors a pleasure. Next to the mammals, birds and reptiles of North America, the fauna of South America will receive attention; but the Society's collections must of necessity include a sufficient number of the living creatures of the Old World to furnish the student and the general public with good examples of the principal orders, families and sub-families of the higher land-vertebrates of the world.

"It follows that, in the formation of the numerous living collections, which will find homes in the Zoological Park, the first to be gathered will be the representatives of the 'great game animals' of North America—the buffalo, elk, moose, mountain-sheep, antelope, black-tailed deer, Virginia deer, and caribou, and also the mountain goat, if it can be induced to survive in this climate. The enclosures planned for these species vary in area from three to twenty acres each. All will be abundantly provided with shade,



HERD OF ELK AT SUNRISE

Photographed from life at Adrian, Michigan

water and shelter, and such cliff-dwellers as the mountain-sheep will be located on rugged masses of natural rock. It is proposed that the buffalo herd shall contain about twenty-five carefully selected animals, living in a twenty-acre range, and be in every way worthy to represent this important species."

In conclusion, we are sure that all persons interested in Natural History will wish full success to the Zoological Society of New York, and will hope that the sum necessarily required to start such a large undertaking may be speedily raised. From the well-known munificence of the wealthy Americans in all matters of this sort it cannot be doubted that these expectations will be abundantly realised.

P. L. SCLATER.

IV

Primeval Refuse Heaps at Hastings

WHEN we reflect upon the abundance of refuse heaps left by early man on the eastern coast of the North Sea, as compared with similar accumulations on our own shores, we are tempted to seek a reason for the remarkable difference. In the former region these relics of man exist for many miles upon the shores as enormous heaps, hundreds of yards long and hundreds of feet in width, usually varying from three to six feet, but sometimes attaining as much as ten or twelve feet in thickness; while in England they are practically unknown.

There are several facts in connection with these old refuse piles which may assist us in our search for them in the British Isles. In the first place they are the relics of a people who lived upon fish, supplemented by such animal food as they were fortunate or clever enough to obtain. Darwin, in his "Journal" (p. 234), draws a picture of the shellfish-eating Tierra del Fuegians which, though a very dismal one, would doubtless equally well describe what might have been seen on the shores of the Baltic in the period under consideration:—"The inhabitants living chiefly upon shell-fish are obliged constantly to change their place of residence, but they return at intervals to the same spot, as is evident from the pile of old shells." Obviously, the refuse and rubbish of these people would be confined to the coast, for had they penetrated into mid-country their relics would necessarily be in the main of a different nature. It is also evident that the comparative stability of a coast line is essential to the existence of the "Kitchen-Middens"; and this idea is supported in Denmark itself, where these accumulations are found far more plentifully around the more permanent and protected fjords. than upon the ever-varying sea-board. On the eastern side of England the cliffs have been wasting practically ever since the incursion of what we call the North Sea, and the breach by the Straits of Dover. It thus becomes obvious, that searching for these deposits in these or similarly circumstanced localities would be hopeless. If, on the other hand, we can get a firm unwasting coast line, furnished with convenient rock-ledges, which at once offer shelter while in use and immunity from destruction of deposits formed upon them, we have all the necessary conditions for the existence of Kitchen-Middens, and there they will doubtless be found all round the coast.

There is, however, no reason why the people who left these mounds of refuse should not have proceeded inland, and there formed settlements; but under these conditions the relics left behind would necessarily be of a very different nature. Certainly they would not consist largely of shells as does the Midden material near the sea-shore. If we found a similar set of animal remains in the Middens and in inland settlements, it would be something to indicate that both deposits might have been geologically contemporaneous. But this evidence would be by no means conclusive, as the people inland, in the absence of a plentiful supply of fish, might have been driven to the chase, and thus have captured a large number of animals which eluded the skill of the fisherman, remains of which would be present among the relics of the former but not among the latter. Then again, the landsmen would naturally be first to practise any kind of domestication of animals for the purpose of food and clothing, and would doubtless adopt this custom long before the fishermen annexed it to supplement their method of obtaining sustenance. It will thus be seen that, although a given set of animal remains might enable us to fix approximately the geological 'age' of the deposits, their presence or absence would not be sufficient evidence to enable us to correlate deposits found upon the sea-shore with those occurring mid-land. Unfortunately the appearance and disappearance of animals associated with man are not so clearly indicated by our time charts, as at present constructed, as they ought to be; so that the fossils of man's mental evolution over a given locality —namely, the exhibitions of skill in the production and fabrication of articles to obtain the necessaries of life, and make it more endurable, or gratify his desires—must serve as the figures upon our chronograph. If, for example, all implements found in these deposits were of the well-known Palæolithic types, we should not hesitate in classing the Middens as Pleistocene. If there were a profusion of beautifully polished axes and barbed arrow heads, we should assign them to the ordinary Neolithic men; and if only a single bronze implement were found, we should just as readily relegate them to the age of metal. So also if we find a certain set of relics differing from anything else previously recognised, although they may tell us little of the actual age of the deposit, yet they may prove invaluable in correlating the age of identical objects found over a large extent of country, and justify our assigning them to one race of people: and if from each of the localities in which these are found we obtain supplementary evidence, we may at last obtain a most comprehensive and reliable account of the heretofore unknown people.

Judged by this standard, I do not at present see anything to connect the people who left the Hastings "Kitchen Middens" with those of the Baltic; still a great deal of work remains undone on both sides of the North Sea. The flora and fauna of the two countries are too dissimilar for exact comparison; but the implements in the two cases are practically quite unlike. Many of the habits of both peoples were, and are, the common property of all savages, or semi-savages, living under similar conditions; but beyond this I do not feel we can go safely, and I am therefore, from other evidence, disposed to regard the Hastings Midden men as quite a different race from those of the Baltic, and recognise their closer relationship with the race who made the identical curious little implements in the Valley of the Meuse and other places.

We will next take a survey of some of the features presented by the Hastings "Kitchen Middens," and note some of the points raised by them, and the contained materials. For fuller description of these on some points, to prevent repetition, the reader is referred to the paper in *The Journal of the Anthropological Institute*, vol. xxv., plates x.-xiii.

Hastings has always been celebrated for its picturesque rock-ledges, caves, and fissures, which exist on both sides of the old town; but few, if any, ever realize the antiquity of some of these, dating back as they do to the very earliest history of the weald, and will, doubtless, some day reveal to us our lost Miocene World. There can be no doubt that the celebrated St Clement's Caves or fissures originated in the great Earth movements above referred to: that they were inhabited in later Palæolithic times, and were enlarged by ambidextrous Stone Age man, and still more extensively excavated by right-handed Iron Age man, as shown by the pick marks still extant. I have found fragments of Neolithic pottery in them, and although much of their contents, dating back to the earlier occupations, may have been cleared away by Iron Age man, I still think they would well repay a thorough investigation. To what extent they were used by the Midden men it is difficult to say at present; but, so far as research has gone, it appears that the fissures nearer the sea were used more by way of shelter, and it is the rock-ledges outside these upon which the relics of the life of the time have been preserved. Plate V. shows the general appearance of these fissured and cavernous rocks; the accumulation of Midden material upon one of the ledges is seen in the front, as shown during the excavations. These projecting surfaces exist at all altitudes up to, say, 120 ft.; here were enacted all the dramas of domestic life, hither were brought the trophy of the chase, the captures from the sea, and the gatherings from the shore; to this resort were taken the pebbles from the beach, and here they were worked into the various forms and implements that man required for his increasing needs:

here he lit his fire, heated his pot or roasted the pony or deer he had captured in the chase; here he changed his broken flint tip snapped in the chase, replaced it by a new one, and threw the broken butt end upon the Midden. Here he also sat and split his marrow bones, and feasted right royally upon the contained luscious grease; and here he piled up his refuse heaps of everything with which he for the time being had done.

It is difficult to say which are the most interesting, the fauna of the period, the relics of which have been stored up in these old heaps, or the fossils of man's civilization in the form of flint and bone implements, pottery, etc. We will take the latter first, as belonging to the highest of the mammals represented.

THE WORKED FLINTS.

Being away from the chalk, the supply of flint had to be drawn from the pebbles on the sea shore. These were taken up to the settlement, and in several places heaps of these were found: they were sometimes 'quartered' when flat flakes were required, at others they were flaked into long narrow flakes, which required but little, if anything, done to them to make them fit for use. The almost absence of the ordinary more or less circular-edged skin-scraper—the commonest of all Neolithic implements—is very remarkable; but some spatulate forms were found, several of which were tanged for hafting. There were no large axes or adzes found, nor even the small triangular form, which exist in such profusion in the Danish Kitchen Middens, and no flint bore the slightest trace of polishing. One of the most remarkable and interesting features of the flints was the variety of forms of the chisels, gouges, and gravers, the cutting edges being always well worked, and either rounded or rectangular, turning now to the left and now to the right. A peculiar feature which strikes one in connection with these is, that they differ entirely from the majority of flint tools one sees in museums, in which is shown the expenditure of extensive trouble and work to bring the implement into an orthodox shape, although the cutting or operating edge is confined to an exceedingly small spot, and is entirely independent of the elaborate work of the other part of the implement. In these, on the other hand, it is just a simple flake, which is usually untouched, except at the point, where it is worked in a straight or oblique inward or outward curve or line; occasionally they are large, but not very often. With such a variety of carving tools in the possession of these old people, one is disposed to feel disappointed at finding so little evidence of the practice of carving. Still, it must be borne in mind that the Kitchen Middens after all were only waste heaps, and nothing of any value would be likely to be found in them, except an occasionally lost article. That bone was largely used we shall presently see, and doubtless great care would be taken with these finely-carved articles, and they may be found when the innermost recesses of the fissures have been penetrated. That a large portion of flint flakes were intended for arrow and fish-spear tips is certain from the fact that many are secondarily worked into bilateral symmetry. Disregarding mere chips the long-pointed flakes monopolise 75 per cent. of the flints found; they vary in size from 4 in. by $\frac{3}{4}$ in. down to $\frac{1}{2}$ in. and by $\frac{1}{8}$ in. They were doubtless used for lance and arrow heads, fish-spears and fish-hooks; no sign of a barb of any kind was seen. A point of special interest in regard to these tips was that very many of them had well-worked butts, sometimes showing the removal of ten or a dozen small flakes in the formation of the well-rounded, nicely-bevelled, square or circular-ended base. There was one feature about these which puzzled me for a long time, and that was that the present periphery of the flake-face of the flint usually truncated these secondary flakings. It, therefore, became evident that the secondary base was worked round a single or double ridge while the lance-tip was yet upon the core, from which it was dislodged by the last skilful blow. In experimenting I found this practicable; and ultimately I found a block of flint worked into ridges in the process of manufacturing these tools in the way suggested, with one base worked ready for the next blow to produce a perfect implement. This specimen is shown in Plate VI., at the bottom right corner, and the arrow marks the point at which the last blow would have been struck, that would have dislodged the finished tip, such as is seen just at the left of it. That the tips were used and broken in the chase or in fishing is certain from the immense numbers of broken butt-ends that were found in the Midden; as it is only reasonable to suppose that, if these were broken in fishing or in hunting, the broken butts would have been taken off the line or shaft when the user returned to *chez lui* and thrown upon the refuse heap, and replaced by new ones. Large quantities were used as knives, and were hard worn.

The next most plentiful tool was the needle maker, or small hollow-scraper; these varied in size, say, from an inch or more, down to a tool that would turn out a bone duplicate of a good-sized thread needle.

There was an immense number of cooking stones indicative of the method of applying heat. Charcoal was fairly plentiful; and many of the bones of horse, pig, sheep, &c., were burned in the process of roasting upon the old hearths; the latter still remained in places. That the pots were put upon the fire, however, is certain from the fact that many had a deposit of soot upon them.

W. J. Lewis Abbott.

(*To be continued next month, with the illustrations.*)

V

The Taxonomic Position of the Pteraspidae, Cephalaspidae, and Asterolepidae*

IN his notice of the life and work of the late Professor Cope, published in the June number of *Natural Science*, Mr Arthur Smith Woodward writes:—"It is mainly due to his initiative that we now regard the strange Pteraspidae, Cephalaspidae, and Asterolepidae (Ostracodermi or Ostracophori as Cope termed them) of the Upper Silurian and Devonian rocks as the armoured extinct allies of the modern lampreys (Marsipobranchii)."

No one, I am convinced, entertains a more sincere admiration for the palæontological work of Professor Cope than I do. His extraordinary insight (one might almost say 'instinct') in dealing with many problems of vertebrate morphology placed him in a very high position amongst modern comparative anatomists. But his genius was all the more astonishing owing to the fact that it was accompanied by equally extraordinary mental deficiencies—namely, a frequent failure of the power of correct conscious reasoning and a love of what Mr Smith Woodward calls "wild guesses" and other persons might term "baseless conclusions."

Mr Smith Woodward, in the passage quoted, cites with approval one of Cope's most empty taxonomic vagaries concerning the class of fishes; he solemnly states that it is due to Cope's "initiative" that "we" now regard the Ostracodermi as Marsipobranchii. Zoologists are of course aware of the fact that Mr Smith Woodward has given the weight of his reputation as an ichthyologist to this remarkable proposition. It seems to be a proper occasion, now that he tells us that there are others besides himself who have been impelled by Cope's initiative into so astonishing a point of view, to ask Mr Smith Woodward to state the reasons which induce him deliberately to assert that *Pteraspis* and *Cephalaspis* are armoured Marsipobranchii.

I think it will be generally admitted that it is not a sufficient justification for upholding such a taxonomic novelty as the one in question to point out that no one has shown any other position to

* For a general illustrated account of these extinct organisms see *Natural Science*, vol. i. (1892), pp. 596-602.

be the proper one for the doubtful group; we may not, for that reason alone, boldly assert that they are Marsipobranchs. Nor again will it be held to be sufficient in the opinion of most zoologists to say "Mr X. was a very gifted man, and he used to say that the so-and-so's are really such-and-such." It really is time for Mr Smith Woodward to give us more solid reasons than such as these for classing Ostracodermi with Marsipobranchii.

I shall be surprised if he can do so; for I have turned the matter over carefully and happen to have a rather intimate acquaintance with both the Ostracodermi and the Marsipobranchii. I am unable to find a single fact which can be considered as positive evidence of affinity between the two groups.

It is true that we do not know of the existence of paired fins in the Ostracodermi—nor in Marsipobranchii—but though the supposed pectorals of *Cephalaspis* are probably not to be regarded as pectorals, we are not in a position to assert that *Pteraspis* had no lateral fins, nor that the 'flippers' of *Pterichthys* do not represent such organs.

Before the proposition favoured by Mr Smith Woodward could be seriously discussed in the terms in which it is stated, it would be necessary to show that the Ostracodermi are a natural group, and no one can pretend that this is the case. The Cephalaspids, the Pteraspids, and the Pterichthyids were originally associated as 'Ostracodermi' for purposes of mere temporary convenience. It is a question whether in these days of an avowed genealogical implication in our classifications, such 'lumber-rooms' as 'Ostracodermi' are permissible. I think not. There is absolutely no reason for regarding *Cephalaspis* as allied to *Pteraspis* beyond that the two genera occur in the same rocks, and still less for concluding that either has any connection with *Pterichthys*.

If, in view of this fact, we consider in a more detailed way the suggestion of Cope, acclaimed by Smith Woodward, we find that it amounts to the assertion, that there are evidences of the close genetic relationship of the Marsipobranchii with *either* the Cephalaspids, *or* the Pteraspids, *or* the Pterichthyids—or possibly with all of them.

So far as I am aware the only satisfactory evidence of marsipobranch affinities which one could expect to be offered by fossil remains of palæozoic date, is the presence of a single median nasal aperture. The characteristic monorrhine structure of the marsipobranchs *might* have been recorded in the preserved remains of an armoured marsipobranch, had such a creature ever existed. Is there any evidence of such a single nostril in Pteraspids, or Cephalaspids, or Pterichthyids? Most assuredly there is not. There is no aperture in the cephalic shields of any of these forms which can be assigned to the nostril. If these fishes possessed a nostril, single or double (as presumably they did), it seems that it was placed in such a position as to avoid perforating the bony shields of the head. To assume—in the total absence of evidence *pro* or *con*—that these fishes were monorrhine, is surely illegitimate and arbitrary. Further, there are in *Eukeraspis* and some other head-shields indications of lateral chambers, remotely suggesting lateral branchial chambers; but the form and position of these would be equally consistent with elasmobranch as with marsipobranch affinities.

It seems to me that even Huxley's cautious statement as to the affinities of *Pteraspis* and *Cephalaspis* goes too far. He says (*Quart. Journ. Geol. Soc.*, vol. xiv., p. 279), "A careful consideration of the facts, then, seems to me to prove only the necessity of suspending one's judgment." So far I entirely agree with him. He proceeds, "That *Cephalaspis* and *Pteraspis* are either ganoids or teleosteans appears certain, but to which of these orders they belong there is no evidence to show." That was written a long time ago. It seems to me that whilst there is abundant evidence to shew that Pteraspids and Cephalaspids, and also Pterichthyids were craniate vertebrates, there is nothing to show conclusively that they are referable to any known group of fishes, rather than that they are to be regarded as representatives of isolated extinct lines of descent. Their possession of paired orbits, fish-like scales, and fish-like median fins, renders it the course involving least assumption concerning matters of which we are ignorant, to treat them as detached groups of primitive fish-like forms, concerning the closer relationships of which judgment must be suspended.

The palaeontologist is, we must admit, entitled to make suggestions and guesses as to the affinities of the organisms which have left behind them the fragments with which he has to deal. On the other hand, if he aspires to be a zoological taxonomist he must accept the common point of view of zoologists. He does injury rather than benefit to zoological science when (as is, unfortunately, sometimes the case) he endeavours to impose an unwarranted "guess" upon zoological taxonomy as though it were a soberly worked-out conclusion, or reconstructs classifications hitherto based upon the consideration of comprehensive anatomical data, in order to give unnatural prominence to such characteristics alone as are furnished by the hard parts of organisms—parts with which a study of fossil forms renders him specially familiar.

E. RAY LANKESTER.

VI

Note on the Warm Undercurrent in the Arctic Ocean between Greenland and Spitzbergen

[In our review of Nansen's "Farthest North" (*Natural Science*, vol. x. p. 270, April 1897) we took occasion to remark that the author's recognition of comparatively warm water beneath the cold surface of the Arctic Ocean was no 'surprising' discovery, but merely confirmed "the observations of Scoresby, Markham, and Maury, and the views of Lyell, Croll, and other people not unknown." We now have the pleasure of further illustrating this point by publishing the following observations, with which we have been favoured by Mr Leigh Smith. They were written in 1875, but have not hitherto been printed.—EDIT. *Nat. Sci.*]

THE following table of deep-sea temperatures observed (with a Miller-Casella thermometer) on board the *Samson* in 1871-72, seems to me to establish Maury's theory of a warm undercurrent running into the Arctic Basin between Greenland and Spitzbergen. I cannot find an account of the temperatures taken by me on board the *Diana* in 1873, but they confirmed previous results.

TABLE OF DEEP-SEA TEMPERATURES TAKEN IN 1871-72 ON BOARD THE *SAMSON*.

Lat.	Long.	Surface temp.	Depth, fathoms.	Temp.
81°·20′	18°·0′E	33	300	42·5
80·10	6·55E	34·5	600	39
80·1	6·36E	34	50	37
,,	,,	,,	200	40
78·34	8·8 E	37	600*	33·5
77·16	4·38E	34·5	25	32
,,	,,	,,	250	39·5
76·36	2·14E	31	150	39·5
76·21	0·21E	36	150	39·5
76·20	0·21E	31	200	39·5
76·20	0·54E	33	50	40
,,	,,	,,	200	48·5
75·50	12·55E	40·5	100	34·5
,,	,,	,,	250	33·5
75·0	13·15E	41·5	100	34
,,	,,	,,	250	42·5
74·39	26·16E	32·5	30	34
,,	,,	,,	100	35·5
73·27	20·21E	38	100†	35
,,	,,	,,	230	44

* After this sounding was taken the thermometer ought to have been lowered to 250 fathoms, as it is probable that there might have been warm water that the thermometer passed through too quickly to register.

† South of Bear Island. Warm current going east.

This warm undercurrent, after passing Hakluyt's Headland, goes to the N.-E., and some of it may come to the surface in the shallow seas on the western shores of lands lying in that direction. This would account for the fact that Payer found open water and a warmer climate to the north.

Dr Carpenter, in a paper read at the Royal Institution of Great Britain on March 20, 1874, refers to his theory as "A general oceanic circulation sustained by a difference of temperature *alone*." In a paper read before the Royal Geographical Society, June 1, 1874, he also states his theory in the same terms. It is true that he afterwards admits that difference of salinity will produce a circulation, but then his theory simply becomes Maury's theory.

Independent of all theory the heaviest water will go to the bottom; therefore, if there is an oceanic circulation, whatever may be its cause, a warm stream of heavy salt water will flow beneath a cold stream of lighter brackish or fresh water.

Down the east coast of Greenland there is an Arctic current * about 200 miles broad, bearing on its surface a mighty floating glacier, which extends to Cape Farewell, a distance of 1400 miles. The rate of this current is variously estimated from 5 to 15 miles a day.†

How is the water and the salt so carried out of the Polar Basin replaced? Must it not be by an undercurrent of greater specific gravity running into the Polar Basin? B. Leigh Smith.

* See Scoresby's "Arctic Regions," Drift of the Hansa.

† I am anxious to establish the existence of this current, as two great authorities have lately doubted it. I therefore append the following note, dated Feb. 16, 1897, from Mr R. Kinnes, manager of several whalers cruising during the summer season along the east coast of Greenland:—"The drift of the ice down the east coast of Greenland varies from 10 to 12 miles per day, and I think this may be taken as a fair average. If the wind is N.-E. it goes much faster, but with a S.-W. wind it travels eastwards. The pack may vary from 150 to 200 miles in breadth, according to the season. The current travels a little faster than the ice, and to the southward, near Cape Dan, it becomes stronger." I may also add that in 1874 Captain David Gray was up the east coast on the *Eclipse*, and he sent me the following extract from his log:—"July 24. Found by to-day's observations that we have driven 43 miles S. by W. ½ W. true in the past three days, and that in the face of fresh winds from S.-W."—B.L.S., May 1897.

VII

Karl Pearson on Evolution *

THE belief that the fundamental problems of organic evolution are essentially statistical problems, which require numerical treatment before they can be adequately solved, is constantly gaining ground; and with the growth of this belief the need for finer methods of statistical inquiry has grown also. With the exception of Mr Francis Galton, who worked for years almost alone in this direction, no one has done so much as Professor Pearson to make the systematic investigation of animal statistics possible. His development of the theory of Chance enables us now to find fairly simple mathematical expressions, by which masses of statistics, hitherto incapable of arrangement in such a form that the mind could grasp their meaning, may be easily and accurately represented. Professor Pearson devotes four of the essays in his recently-published volumes to a popular account of some of his results.

The first essay ("The Chances of Death") begins by showing how regularly "chance" is seen to operate, when a large series of fortuitous events can be observed. The regular character of such events, and the accuracy with which they can be predicted in the long run, is illustrated by records of experiments of the usual kind with coins, dice, and cards; and it is then shown that a law of the same form as that used to express the result of a long series of games of chance may be used to express, with the same degree of accuracy, the frequency with which given magnitudes of a cephalic index occur in a race of men, or the frequency of patients of given age among a large group of typhoid fever cases. Finally, the frequency of incidence of death at various ages among every thousand people born at the same time is exhibited as the resultant of five series of fortuitous events, each series producing its maximum death-rate at a particular period of life.

These examples are admirably fitted to show how such apparently irregular phenomena as death, or attacks of fever, or variation in the dimensions of a particular organ, may be easily and accurately represented so that the mind can grasp the effect upon the population as a whole, grouping the series of isolated instances

* The Chances of Death, and other Studies in Evolution. By Karl Pearson. 2 vols. 8vo, pp. xii. 388, and iv. 460. London: Edward Arnold, 1897. Price, 25s.

simply and naturally under one general law. The reader who is acquainted with the theory of Chance, as it is propounded in the ordinary text-books, will appreciate the great extension of the theory which is necessary in order to treat successfully such statistical results as those expressed by the remarkable "curve of infantile mortality," or even those relating to the incidence of scarlet fever, enteric fever, and diphtheria, at particular periods of life.

These illustrations being given in the first essay, the second is devoted to the results of roulette, as played at Monte Carlo, this being chosen as an example of results which are so little capable of prediction by the theory of Probability as to justify the belief that some constant influence other than chance is at work. The point of general interest is the smallness of the discrepancy between the observed result and that given by the theory of Chance which can be used as evidence of some constant disturbing factor.

The third essay, on "Reproductive Selection," contains a most interesting study of the importance to be ascribed to variation in fertility. The material used consists of two tables, showing the number of children arising from each of a large series of marriages. The first series contains 4390 marriages, which are spoken of as "Anglo-Saxon"; they are for the most part English and Americans of the well-to-do classes, with some from the Almanach de Gotha. The second series includes over 34,000 Danish marriages. The result is so important that a rough outline at least must be given. The following table shows the number of "Anglo-Saxon" marriages which produced any given number of children:—

No. of Children,	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17
No. of Marriages,	546	656	682	628	496	383	336	228	172	118	63	47	22	8	2	1	2

The series contains no record of barren marriages; but for reasons fully discussed in the essay, 320 is assigned as the number of barren marriages likely to have existed in a population with the observed number of fertile unions. The above table may therefore be regarded as representing the offspring of 4710 marriages.

The total number of children produced is 19,833, giving an average of about four and a half per marriage; but the striking feature about the table is the demonstration that half of this entire number of children is produced by little more than a quarter (25·8 per cent.) of the total number of marriages, so that half the second generation are the offspring of a quarter of the first. Now, suppose this character of excessive fertility to be completely inherited, Professor Pearson shows that (in the absence of an enormous selective death-rate) ninety-nine per cent of the sixth generation would be descended from the "superfertile" quarter of the original generation. The effect of selection in checking this result among human beings is examined, and the conclusion is reached that it is not sufficient to make any great difference—half the adolescent members of the second generation being produced by less than 29 per cent. of the first.

It is evident that any character, possessed by this original 29 per cent. of "superfertile" couples, will quickly spread among successive generations, unless it be extremely disadvantageous to its possessors; and as an example of the complicated problem presented by this result, it may be mentioned that Professor Pearson found, on examining 206 families, that the mean height of 133 men, fathers of less than five children each, was half an inch greater than the mean height of 73 men, each of whom was father of more than five children; but these 73 men produced between them 561 children, the 133 less fertile producing only 394 children. On the other hand, the more fertile mothers were sensibly taller than the less fertile. Perhaps no example could show more clearly than this the complex nature of the phenomena with which the student of animal evolution has to deal, and the absurdity of trying to deal with them otherwise than by the patient numerical evaluation of each factor separately.

The last essay to which attention can here be called is that on "Variation in Man and Woman." The object is to support the contention that women are on the whole more variable than men; and an immense series of measurements has been collected, in many of which—such, for example, as the cephalic index, the stature at particular ages, and others—this is undoubtedly the case. But it must be borne in mind that Professor Pearson refuses to consider "secondary sexual characters," and that he proposes a peculiar measure of variability. What exactly the rejection of secondary sexual characters means, it is difficult to understand; surely any character, other than the structure of the reproductive organs, may be called a secondary sexual character, if the two sexes differ with respect to it; and if this definition be adopted, Professor Pearson's position becomes unintelligible; if, on the other hand, the term be limited to those characters which are directly affected by sexual selection, then Professor Pearson should not permit himself to discuss the relative variability of a particular organ in men and in women without first showing that marriages occur at random so far as that organ is concerned. No definition of a secondary sexual character is offered in the essay, which is apparently directed against some rash persons who have asserted that every character is more variable in men than in women, and who attempt to deduce social and practical consequences from this proposition.

A more important point is the assumption "that the only useful sense in which we can study relative variability is by endeavouring to answer the problem : Is one sex closer to its mean, more conservative to its type, than the other? and that the only scientific answer to this lies in the magnitude of the per centage variations of the two sexes for corresponding organs."

The meaning of this may be made clear by an example. Suppose a number of sticks, about a yard long, to be chosen by some rough process of measurement ; and suppose that more careful examination showed the average length of these sticks to be 3 feet, while half the sticks were between 2 feet 11 inches and 3 feet 1 inch in length. Suppose, further, a second group of sticks, whose average length is 6 feet, while half the sticks lie between 5 feet 10 inches and 6 feet 2 inches. Now, since one inch is the same fraction of three feet that two inches is of six feet, Professor Pearson asserts that the only scientific view of the variability of the two sets of sticks is that which treats the two bundles as equally variable ; and he accordingly defines the "coefficient of variation," or measure of variability, as the ratio of the "Standard Deviation" or "Error of Mean Square" to the Mean.

Now, it may at once be freely admitted that the coefficient of variation, as above defined, is an exact measure, and probably the best available measure, of the degree to which a group of animals is "close to its type" ; that is to say, it is a measure of the extent and frequency of the mistakes a man would make, if he should simplify a discussion of these animals by using, instead of the individual animals, a series of perfectly average "types." It is precisely the measure of accuracy of the customary morphological definition of a species or variety. But the student of evolution may have to concern himself with another measure of variability, when he asks not "how close is the race to its type," but "how much material for Selection is afforded by the variability of the race?"

The functional importance of a variation of known magnitude, and the effect of such a variation upon the selective death-rate, seem legitimate, if difficult, subjects of scientific inquiry ; and if it can be shown that an organ in one sex gives more scope for the selective formation of varieties or races than does the corresponding organ in the other sex, it is surely legitimate (neglecting the possible complications due to peculiarities of heredity) to say that one sex is more variable than the other. For example, it is certainly possible, in the case of the common fowl, to produce races of cocks which differ more from each other in the length of their tail-feathers than do any hens yet produced ; and the statement, that the tail of cocks in general is more variable than the tail of hens in general seems thereby justified, whether the "coefficient of variation" in the cocks of any one race be greater than that of the corresponding hens or not.

No one acquainted with the facts of animal variation—certainly not Professor Pearson himself—will assert that the "coefficient of variation" is always a measure of the importance of variations; no one will believe that in any animal a deviation of ten per cent. in excess of the mean of one organ has of necessity the same importance as a deviation of ten per cent. in excess of the mean in another organ. A finger nail of double the normal length, or a hair of double the normal thickness, will hardly produce so much inconvenience as a leg of double the normal length. It is even certain that in closely allied species the same per centage deviation of corresponding characters may produce widely different effects.

There is no doubt that the "coefficient of variation" is for certain purposes a valuable measure of variability; and Professor Pearson has shown, in some of his more technical papers, that it is of great use in establishing important propositions in the theory of Chance. At the same time, students of evolution, paying attention specially to the functional importance of variation, may need units proportional to this importance; and such units may well be different in different cases. The violent assertion that there is only one "scientific" measure of variability is therefore to be regretted.

Whether one agrees with Professor Pearson on this single question or not, one cannot but be grateful to him for the four essays here referred to, as well as for the more elaborate memoirs on which they are based. The picture they present of the orderly treatment to which animal statistics can be subjected, so that hitherto unwieldy and perplexing masses of figures can be made to yield simple and intelligible results, should do much to make the study of Probability, in its application to the problems of animal evolution, more popular than it is, and to enable biologists at last to put before themselves an adequate numerical estimate of those phenomena which it is the business of their lives to formulate and to explain.

W. F. R. Weldon.

SOME NEW BOOKS

Our Dead Volcanoes

The Ancient Volcanoes of Great Britain. By Sir Archibald Geikie, F.R.S. 8vo. 2 vols., pp. xxiv. and 478, and pp. xvi. and 492. With 7 folding maps and 383 illustrations. London: Macmillan & Co., 1897. Price, 36s net.

This work is an expansion of the two addresses given by its author to the Geological Society of London in 1891 and 1892. Those of us who have often marvelled at the amount of detail, largely new, that was brought into the compass of the addresses, may have foreseen the solid and permanent character of the two large volumes now before us. In earlier years, Sir Archibald Geikie spent his leisure in volcanic areas—travelling, observing, correlating, and storing up the wealth of information which is now made orderly and accessible. The 270 pages—more than a quarter of the whole work—devoted to the Devonian, Carboniferous, and Permian volcanoes of Scotland will be welcomed on these historic grounds. And in later years, with the resources of the Geological Survey at his command, the author has been able to extend his area, so as to examine the whole range of volcanic phenomena in our islands. Ireland is not mentioned in the strict title of the work, but is in reality dealt with in a manner that atones for a great many omissions on the part of British text-books. We have here, in fact, a basis which must be consulted before work is begun on any igneous rocks of the British Isles, for Sir Archibald frequently, and very properly, treats of holocrystalline and deep-seated masses in addition to the volcanic relics associated with them.

So far as we can judge, the passages compiled from previous writers have been drawn up with admirable accuracy, even to the use of the rock-names employed in the original papers. These names, in fact, would sometimes be the better for a little comment or revision, as in the case of the French rock-term 'labradorite,' imported on p. 29. The book is, however, written for geologists, and largely for those engaged in actual investigation; it has, at any rate, the merit of offering no encouragement to young persons 'reading' for examinations.

While book i. is of a general character, there is much in it that is admirable and suggestive to the worker in the field. We may note, for instance, the diagram of "the gradual emergence of buried volcanic cones through the influence of prolonged denudation" (p. 75). The characteristic structures of igneous rocks are described, and are illustrated by photographs from actual specimens. The polished surface of 'Napoleonite' on p. 22, and the fluidal Antrim rhyolite on p. 23, are perfect examples of their kind.

It is surely, however, an undue extension of terms to call the structure of 'Napoleonite' variolitic; and we could wish that variolitic and orbicular structure had not been separated from spherulitic in the text. The ophitic structure, again, is referred to, as in so many works, from its superficial or microscopic aspect, the enveloping crystals of pyroxene being described as 'large plates.' As all field-workers know, they are often so uniform in length, breadth, and thickness, as to produce the appearance of nodules on the weathered surfaces of a rock.

In dealing with columnar structure, it is remarked that in one type, of which the rock at Fingal's Cave is an example, "the columns pass with regularity and parallelism from the top to the bottom of a bed." The other type is of the irregular character. "At Staffa the regularly columnar bed is immediately overlain with one of the starch-like character." Though we are told that the two types may even pass into one another, we should have liked an opinion as to their continuity in the case of Staffa. In vol. ii., p. 210, the photograph of that island is used to show the 'bedded' structure of the basalt; and the reader might easily regard the mass as formed of two successive sheets. Some reference would be useful to Scrope's observations in

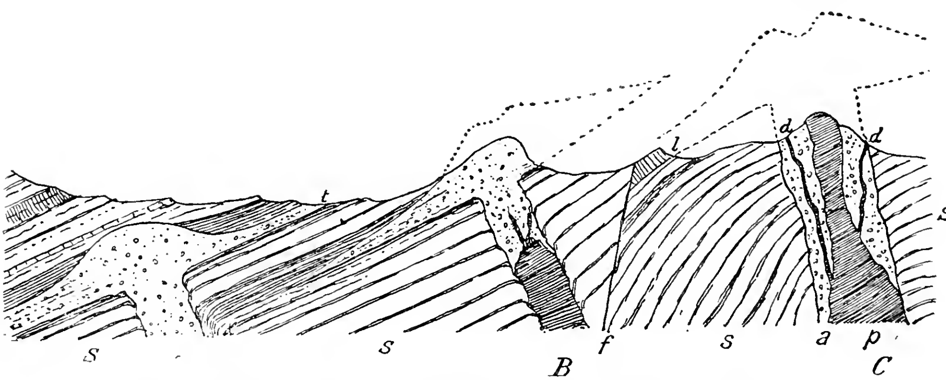


Diagram illustrating the gradual emergence of buried volcanic cones through the influence of prolonged denudation.

the Vivarais ("Considerations on Volcanoes," 1825, p. 141), already noticed in Prof. Judd's 'Volcanoes.'

Another case in which the opinion of Sir Archibald Geikie would have been of service is in regard to the relation of laccolites to earth-movement. How far does the pressure that propels the lava forward enable it to lift up the dome of strata and to form a cistern for itself? In any case, we are not left with the vague idea that some expansive force within the molten mass itself enables it to upheave the earth above it (p. 87, and vol. ii. p. 361); but might not greater emphasis have been laid upon the view stated on p. 98, where the cavities occupied by the lava are themselves referred to earth-movement? Mr Gilbert, however, was undoubtedly of opinion in 1877 that the pressure on the fluid mass was sufficient to produce the arching above the laccolite.

A good summary of the theory of differentiation in plutonic masses, and of recent results on 'bosses,' is given on pp. 88-99. The variation of an intrusive magma by absorption of surrounding rocks is also very fairly dealt with.

Chapter vii. gives scope for some of those admirable descriptions of landscape, which recur afterwards delightfully through the work, and which constantly remind us of the open-air culture of the author.

Page after page attests the energetic field-work on which each series of results is based, though now and then acquaintance with the ground is required, before we can detect how much is due to a grasp of the features of the scene itself, and how much is culled from the drier field of published memoirs.

We must not attempt, chapter by chapter, to touch on the numerous new suggestions, or the revisions of previous work, contained in the description of each special area. On p. 145, we note that the Cambrian is stated to pass down conformably into the Pebidian of St David's, which is included with it, the Arvonian and Dimetian being intrusive; and no pre-Cambrian rocks are tolerated here. But in other places the earlier work of the survey is candidly set aside, where the personal investigation of the author has led to a change of view. Here and there, work remaining to be done is indicated, as in the Malvern range (p. 170), and in the important Borrowdale series (p. 227).

In the latter case, the discussion given in the present work does much to fill the gap, and is a fine example of the caution with which such altered masses must now-a-days be approached. We have done with the broad structural diagrams, accompanied by 'theories of the earth,' which had to serve as a basis for future observations, at a time when the labourers were few; and Sir Archibald Geikie writes of the map of the lake district, "so rapid has the progress of certain branches of geology been since these sheets were published, that the map is even now susceptible of considerable improvement."

A strikingly new chapter, embodying results hitherto unpublished, or, perhaps, only hinted at in the annual reports of the Geological Survey, deals with "the Silurian Volcanoes of Ireland" (pp. 239-256). We note than an Arenig age is suggested for the crushed tuffs of eastern Tyrone, formerly regarded as 'Dalradian'; but "no recognisable radiolaria have yet been detected" in the associated cherts.

An interesting point in the description of the Old Red Sandstone eruptions is the occurrence of infillings of sandstone in the cavernous hollows of lavas poured out under water (pp. 283 and 333). In some cases, these sandstones are even stratified, reminding one of the material deposited in the interstices of a coral-reef.

We are glad to note, on p. 346, the decided attitude taken up with regard to the correlation of the Dingle Beds. Irish geologists have, more or less, played with this important stratified series, usually referring it to the Silurian, although it clearly caps the Ludlow beds. Since Jukes led an attack upon the Devonian system as a whole, an attempt has been made to do without that system in Ireland, the Upper Old Red Sandstone being carried bodily up into the Carboniferous, and the lower thrust down into the Silurian. No man living can speak with better authority on this point than Sir Archibald Geikie.

The first volume closes with a superb series of full-page photographs of the Carboniferous volcanic phenomena of Scotland. While these recall the work done by the Geological Survey of the United States, we can only regret that in our own islands their publication, on this ample scale, has been reserved for private enterprise. We cannot resist mentioning by name the view of the agglomerate of the Binn of Burntisland (p. 431), which equals the fine Cainozoic sections in the neighbourhood of Budapest, and the spheroidal weathering of dolerite at North Queensferry, given on p. 455.

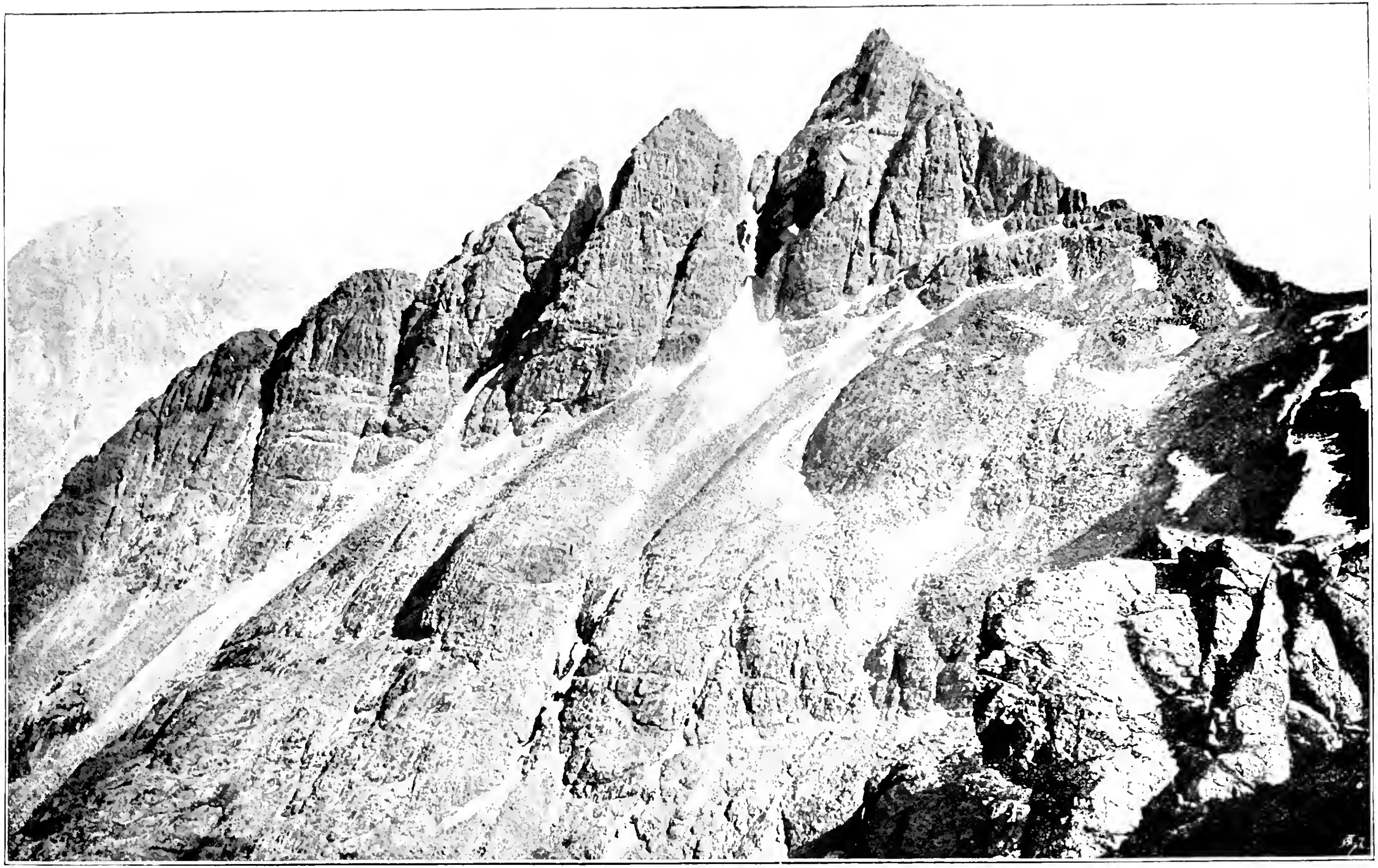
The second volume opens with the Great Whin Sill, which is provisionally referred to the Carboniferous period. A considerable series of volcanoes is then included as Permian, or possibly Triassic, on the ground that "there is usually ample proof that the strata in question [associated with them] are much later than the Coal-measures, while their geological position and lithological characters link them with the undoubted Permian series of the north-east of England." The best known mass placed in this group is the upper portion of Arthur's Seat, Edinburgh, which, the author maintains, is independent of the older and true Carboniferous series. He thus supports Maclaren's view of 1839, in opposition to its subsequent revision by its author, and to the well-known paper by Prof. Judd.

Some exceedingly pretty mapping of the fragmental materials in a vent at Elie Harbour, Fife, is seen in Fig. 217; and the volcanic sections of this district are again finely illustrated by full-page photographs. The capping of Titterstone Clee, in Shropshire, is referred also to the Permian period.

Then comes the great gap in our volcanic history, book viii. opening with Cainozoic times. The plateau-basalts are attributed to fissure-eruptions rather than to repeated flows from central vents; and a very valuable chapter (pp. 260-269) supplies an account of such eruptions in recent times in Iceland. It is, however, pointed out that the tabular sheets are composed of the union of successive flows, one overlapping on the edges of another (p. 193); and the fact that cones are formed along the lines of fissure, each sending out its separate flows, is clearly brought out on pp. 264 and 265. The absence of clear evidence of the connexion between our dykes and the superficial flows is very candidly stated (p. 268). The recognisable vents, such as Slemish in Antrim, and other well-known necks in the same county, indicate more normal conditions of eruption.

While there is this difficulty in correlating the intrusive basalts with the lavas penetrated by them, we must also face the similar difficulty arising from the lack of continuity between the gabbro masses of Mull and Skye and the surrounding plateau-basalts. If the dykes form the orifices for the extrusion of the latter, why may not the heart of Skye have served equally as a great volcanic centre? In both cases, the intrusive rock presented to us is that which last consolidated in the vent; its earlier and basal volcanic products may, however, remain to us in the surrounding lavas. Surely the great core of Vesuvius is at this moment intrusive in the earlier lavas of its flanks. The author recognises some such argument on p. 348, but will not allow the site of the Cuillin Hills to have any direct relation to the existing surface-products (p. 362). Granting that a number of small vents suits the conditions recognised in Antrim, why may we not have traces of more centralised activity in Mull and Skye? The difference of opinion between the author and Prof. Judd on this point appears less fundamental, less to be insisted on, the more clearly we have laid before us the relations of the dykes and plateau-basalts from the fissure-eruption point of view.

It can hardly be expected that Ireland should receive more detailed



SGURR NA GILLEAN, SKYE, SHOWING THE CHARACTERISTIC FORMS OF WEATHERED GABBRO

treatment, in regard to its Cainozoic volcanic areas, than is accorded to it in the present volume, which is far and away more generous than any predecessor. While Ireland cannot at any point equal the crags of Scuir na Gillean (p. 335), so finely set forth by Mr Abraham, yet we should have liked some recognition of the strikingly scenic aspect of the Mournes, one of the most 'self-contained' and solitary mountain-groups in the British Isles. One page deals with this district, while as many as seven are devoted to the Limerick Basin, fourteen to the toadstones of Derbyshire, and thirteen to St Kilda. To ask for more, however, when we close these handsome volumes, is only a well-merited compliment to their author. G. A. J. Cole.

A Live Naturalist

Round the Year: A Series of Short Nature Studies. By Professor L. C. Miall. 8vo, pp. viii., 296. With illustrations chiefly by A. R. Hammond. London: Macmillan & Co. 1896. Price, 5s.

"Live Natural History!" The phrase is our author's, and no better example of it could be found than this book. It is, we may imagine, just such a book as might have been written by Gilbert White had he lived in these days, and had the benefit of a thorough scientific training. Against the 'dry, marrowless, useless,' 'melancholy' and 'stodgy' catalogue-type of natural history, the author raises a just protest. There is also a style of natural history writing that consists largely of phrases without knowledge and imagination unallied to observation. Professor Miall gives us the attempted literary charm of the latter with the accuracy and wealth of knowledge of the former. He takes us out into the fields and over the mountains, but does not forget that there is a well-stocked library at home. It is indeed a feature that we would fain see more of in so-called popular 'Natural Histories'—this constant reference to fuller accounts and original authorities. So many writers treat their readers as sheer dyspeptics, unable to digest aught beyond pap.

As examples of the subjects so fascinatingly and suggestively dealt with, we may mention: snow-flakes, birds in mid-winter, cat and dog, the moon, spring crocuses, catkins, the oil-beetle, the botany of a railway station, hay-time, cabbages and turnips, weeds, the love of mountains, the reversed spiral, the structure of a feather, the shortest day of the year.

It is now July, and we find our author treating of duckweed. How many of us know its flower? Now is the time to see it. Let the field naturalist take some duckweed from the water, and, with Professor Miall, let him examine, describe, and draw it. The reasons for its peculiar shape may then be guessed at, and the guesses checked by experiments with models. Thus he is led to understand more about the relations of this common water-plant to its environment, and the ways in which it may spread from one pool to another. How widespread it is he must learn from books, such as Hegelmaier's "Die Lemnaceen."

A word of praise is due to the illustrations, the fresh pen-and-ink drawings by Mr Hammond being specially clear and artistic.

SCRAPS FROM SERIALS.

THE June number of the *Westminster Review* contains the conclusion of an article by J. F. Hewitt on the cave deposits of the Ardennes, which he thinks contain evidence to show that the palaeolithic dwellers in those caves had a regular totemistic system.

There is no magazine in New Zealand, but the *Press* of Christchurch attempts to supply the want by publishing, every Monday, signed articles on literary, social, and philosophical questions. The issue for April 26 contains an interesting account by Prof. Arthur Dendy of the land-planarians, nemertines, and the *Peripatus* of New Zealand.

The Photogram is publishing a useful series of articles on applied photography. Application No. 5, which appears in the June number is 'in Zoology,' by Dr R. W. Shufeldt, and is illustrated by some admirable examples. A cognate subject is "Stalking with the Camera," by R. B. Lodge, of which part 3 appeared in the May number. This appears to be quite as exciting as stalking with a gun, and the results are both permanent and valuable.

Geologists who propose attending the International Geological Congress, and who may be so fortunate as to have obtained tickets for the excursion to the Caucasus, should study a sketch of the geology of that region, published by V. Dingelstedt in the *Nauchnoe Obozryenie.* Or in case this is beyond them, they will find an excellent summary in the May number of the *Scottish Geographical Magazine.*

The *Journal of School Geography*, edited by R. E. Dodge, New York, maintains its bright and useful character. The April number contains a short article on the geographical distribution of plants, by Prof. Conway Macmillan, who contends that the study of plant distribution in a limited area, such as a pond and its shores, or a hill and its slopes, would be of more value in schools than 'the ordinary herbalism.'

In the numbers of the *Revue Générale de Botanique* for the present year some useful reviews of recent work done in certain departments of the science are in progress. One by A. Prunet deals with results in plant anatomy, published in the years 1892 to 1894. The first four numbers treat of the anatomy of the cell under the headings of *Nucleus, Eléments figurés* (including Crato's *physode*, and certain chlorolenciles), and *Membrane.* The review is illustrated, and references are given to original papers. The second, by Géneau de Lamarlière, relates to descriptive work on mosses, published between January 1889 and January 1895.

NEW SERIALS

THE *Revista Italiana di Sociologia*, Rome, intends to be a thoroughly scientific and independent review. Since it numbers among its editors such men as Salvatore Cognetti de Martiis, Augusto Bosco, G. Cavaglieri, G. Sergi, V. Tangorra, and Enrico Tedeschi, it is probable that the promise will be fulfilled.

We have received No. 2 of the *Aeronautical Journal*, which to non-members costs 2s., though it contains only 20 pages. It is thoroughly practical, and brings together all that is taking place in the aeronautical world, whether in the form of experiments, lectures, publications, magazine articles, inventions, or patents.

La Revista Nueva is an illustrated monthly, which commenced publication at San José, Costa Rica, in September 1896. The directors are R. Fernández Guardia and Alberto Masterrer, and they have the moral support of the Government. The price is 60 cents a number, 1 dol. 50 cents a quarter. For the most part the *Revista* is devoted to general and literary topics; but we notice that the earlier numbers contain a series of articles by Anastasio Alfaro on the antiquities and the mammals of Costa Rica. Among the coloured plates are representations of figured jars in the museum of Costa Rica.

We have received the first two fascicules of *Revista Quindicinale di Psicologia, Psichiatria, Neuropatologia ad uso dei Medici e dei Giuristi*, which is the expressive though cumbersome title of a fortnightly magazine, blessed with two directors, seven editors, and twenty-nine collaborators. The chief editor is Dr Sante de Sanctis, R. Clinica Psichiatrica, Via Penitenzieri, 13, Rome. The publishers are Fratelli Capaccini, Via Sistina 22, Rome. Each fascicule, which consists of 16 pages, contains two original articles and several reviews. A year's subscription (from May 1 in each year) is 6 lire. The *Revista* should do much to produce healthy co-operation in Italy between the students of pure psychology and their fellow-workers in criminal and pathological psychology; success seems guaranteed by the eminence of the contributors.

Further Literature Received.

First Stage Mechanics of Fluids, G. H. Bryan and F. Rosenberg. First Stage Sound. Light and Heat, J. Don: Clive. Text-book of Geology, W. J. Harrison: Blackie. Hand-book to the Order Lepidoptera, W. F. Kirby: Allen. Catalogue of the Fossil Cephalopoda in the British Museum, Part III., A. H. Foord and G. C. Crick. Catalogue of Tertiary Mollusca in the British Museum, Part I., G. F. Harris: Trustees Brit. Mus. The Concise Knowledge Natural History, ed. by P. Myles: Hutchinson. Field Geology in South Westmoreland, H. G. Foster-Barham: Atkinson, Kendal. Investigations into Applied Nature, W. Wilson: Simpkin, Marshall. The Aeronautical Annual, 1897: Wesley. The Psychology of the Emotions, Th. Ribot: Scott. Annual Report of the Geological Survey of the United Kingdom, 1896, A. Geikie: Science and Art Dept. Memoirs of the Field Columbian Museum, Chicago, Vol. I., No 2, Ornithological Series, No. 3, Botanical Series. Farmers' Bulletin, No. 4: U.S. Dept. Agriculture.

Poissons provenant des campagnes du yacht l'Hirondelle (1885-88), R. Collett. On the Origin of the European Fauna, R. F. Scharff: *Proc. Roy. Irish Acad.* On the Brains of two Sub-Fossil Malagasy Lemuroids, C. J. F. Major: *Abstr. Roy. Soc.* On Nereids commensal with Hermit Crabs, N. R. Harrington: *Trans. New York Acad. Sci.* Edward D. Cope, H. F. Osborn: *Science.* Comparison of the Carboniferous and Permian Formations of Nebraska and Kansas, C. S. Prosser: *Journ. Geol.* Some Observations on certain Species of *Arion*, W. E. Collinge: *Journ. Malacol.* On Ctenoplana, A. Willey: *Quart. Journ. Micro. Sci.*

Jersey Times, June 7; Tunbridge Wells Gazette, May 26; Amer. Geol., June; Amer. Journ. Sci., June; Amer. Nat., June; l'Anthropologie, March-April; Botan. Gazette, May; Feuille des jeunes Nat., June; Irish Nat., June; Knowledge, June; Literary Digest, May 15, 22, 29, June 5; Naturae Novit., May; La Naturaleza (Madrid), May 14, 28, June 8; Naturalist, June; Nature, June 3, 10, 17; Naturen, May; Photogram, June; Review of Reviews, May, June; Revue Scient., May 22, 29, June 12; Science, May 14, 28, June 4; Scient. Amer., May 15, 22, 29, June 5; Scot. Geogr. Mag., June; Scot. Med. and Surg. Journ., June; Victorian Nat., May; Proc. Biol. Soc. Washington, Vol. XI., May 13; Chivers' New Book List, June; Halifax Naturalist, Vol. II. No. 8, June; Journ. School Geogr., May.

OBITUARIES

AUGUSTUS WOLLASTON FRANKS

Born 1826. Died May 21, 1897

Sir Augustus Wollaston Franks, K.C.B., D.C.L., F.R.S., Pres. S.A., F.G.S., Trustee of the British Museum, late keeper of British and Mediaeval Antiquities and of Ethnography at the British Museum, was born at Geneva in 1826, and educated at Eton and Trinity College, Cambridge, taking his M.A. degree in 1852. His taste for the beautiful in works of art, and his appreciation of the niceties, peculiarities, and fantasies of artists, whether the results were produced with the inspiration of genius, or by handicraft and labour, led him to collect largely in each department of artistic work, and fortunately his ample means enabled him to do so. With munificent liberality he gave many valuable collections to the National Museum at Bloomsbury. It was thus that, not only theoretically, but practically and personally, he was acquainted with the extensive and many-sided groups of antiquities and ethnographic exhibits under his keepership. He was not a mere official custodian, but a cultured connoisseur, and a high-class authority on all points connected with the scientific and historical aspects of the materials or collections in his charge. Necessarily his study of medieval things kept him in touch with those of prehistoric age in the British Collection which was under his care; and, indeed, of these there are many objects of human workmanship dating from extremely early times. Contemporary with these were similar productions in European and other countries. These are largely represented in the British Museum by the "Christy Collection," which Sir Wollaston Franks augmented by successive gifts of similar well-sorted examples from many localities. Indeed, this notable department in the museum well deserves now to be called the "Christy-Franks Collection."

In March 1864, Mr Henry Christy invited a party of his friends, interested as antiquaries and geologists, to examine some of the bone-caves on the Vezère, Dordogne district, in the south of France, which, with his friend Edouard Lartet, he had for some time been investigating with great care, and at considerable expense. The party comprised Mr W. J. Hamilton (President of the Geological Society), Prof. Rupert Jones (Assistant Secretary of the Geological Society), Capt. (since Sir) Douglas Galton, Mr (since Sir) John Lubbock, Mr (since Sir) John Evans, and Mr (since Sir) A. W. Franks.* Not only had the last-named already interested himself in Henry Christy's researches in the ethnographic relationship of various textile fabrics, which had led him to Mexico, and in that country to the observation of stone implements; but A. W. Franks heartily joined Christy in the study of stone implements, and of those who

* *Quart. Journ. Geol. Soc.*, vol. xx., 1864, p. 444; and "Reliquiae Aquitanicae," Part xii., 1873, p. 161.

made and used them, whether ancient peoples, recent savages, or living workmen in some modern trades and manufactures. Together with Dr Hugh Falconer, he aided H. Christy and E. Lartet in planning their great work, "Reliquiae Aquitanicae," which was intended to comprise descriptions of all the Dordogne caves and their contents. Unfortunately the death of both Christy and Lartet circumscribed the work within smaller limits, for no more caves were worked out by them, and but few plates were subsequently added to the eighty or more already lithographed for its illustration. It is noticed, we see, in the preface of the book that, "In bringing together and arranging the varied materials supplied by friends at home and abroad desirous of making the "Reliquiae Aquitanicae" useful in archaeology and anthropology, the directing counsels of Mr A. W. Franks, F.R.S., have been constant and efficient, like his courtesy and knowledge."

He took charge of the "Christy Collection," at 123 Victoria Street, S.W., for some time before it was transferred to the British Museum; and he individualised the specimens with accurate drawings by his talented assistants—first, T. K. Gay, and subsequently Charles Read. The latter worthily succeeded Sir Wollaston on his resignation, in 1894, as keeper in the British Museum.

It was with great caution that Sir Wollaston exercised his judgment as to the authenticity of implements of stone and their relative age. He was not an enthusiast in the subject of the great antiquity of the Human Race. Possibly, had he been induced to give more leisure to the study of the geological aspect and details of the subject, he might have become cognisant of the value of Sir Joseph Prestwich's researches in the geological age of some of the older groups of flint implements, especially of those collected with earnest and intelligent care at Ightham, in Kent, by Mr Benjamin Harrison (see *Natural Science*, Vol. V., p. 269, Oct. 1894).

Sir A. Wollaston Franks contributed largely to scientific literature, especially by memoirs and notices in the Transactions and Proceedings of learned societies. He has greatly enlarged the knowledge of antiquities and their real relationships, not only by original research, but by his willing advice and ready information to enquirers, whether in London or the provinces. He has bequeathed his collections to the British Museum. He was elected to the Royal Society in 1854. For many years an active and valued Fellow of the Society of Antiquaries, he was elected Director of the Society in 1858. Subsequently for some years he was Vice-President, and eventually he became President in 1892. T. R. J.

JULIUS VON SACHS

BORN OCTOBER 2, 1832. DIED JUNE 1897

WE regret to announce the death of Professor Julius von Sachs, the botanist, who perhaps more than any other is responsible for the present position of his science—be it good or bad—at any rate from the point of view of the university or college curriculum. His "Lehrbuch der Botanik," or its English translation, was for more than twenty years the text-book for advanced students; and even now it holds an honoured place in all botanical libraries.

Born at Breslau sixty-five years ago, Sachs studied in the German University of Prague, and in 1851 became assistant to Purkinje. In 1856 he was appointed Privatdocent for Plant Physiology in the same University. In 1861 he was called to the Chair of Botany in the Agricultural Academy of Poppelsdorf; six years later he removed to Freiburg; and finally in 1868 he obtained the Professorship of Botany at Würzburg, which he held until his death.

Sachs was a hard worker and a voluminous author. The Royal Society's catalogue enumerates 92 papers up to 1883 only. The first, on the crayfish, appeared in *Ziva*, a periodical printed in Bohemian, and published at Prague. If we turn over the numbers of *Ziva* for a few years from 1853 onward, the great energy of the man and the bent of his mind towards the morphology and physiology of plants is evident. Among the excellent figures which accompany the text and which alone appeal to most of us, we see the originals of many which have since become classical. Besides his Text-book of Botany, the Clarendon Press has put two other of Sachs' useful works within the reach of all English-speaking students—the "Lectures on the Physiology of Plants," translated by Prof. Marshall Ward, and the "History of Botany." Some idea of the amount of his work may be gained from the size of the collected contributions to plant physiology, published in 1892-3, which form a book of more than 1200 pages, large octavo. The relation of temperature and light to the living plants, chlorophyll and assimilation, the measurement of water through the tissues, and the transport of food-material, are the very wide headings under which his work in this branch is grouped. Besides his numerous papers in the *Botanische Zeitung*, *Flora*, and many other German periodicals, Sachs founded and edited the *Arbeiten des Botanischen Instituts in Würzburg*, the first volume of which appeared in 1874, and the third and last in 1888. They represent mainly his own work or that of his pupils, many of whom have since become well known as investigators and teachers.

FRITZ MUELLER

Born 1822. Died May 21, 1897

This eminent helminthologist, carcinologist, and field-naturalist died last month at his residence in Blumenau, Santa Catarina, Brazil. His earliest contributions to science appeared in Wiegmann's *Archiv für Naturgeschichte* in 1844, and were written under the Christian name of Friedrich. Later on he appeared as Fritz, again as Friedrich, and in more recent publications as Frederico—a series of changes which have confused not a few librarians. So far as his contributions to periodical literature are concerned, the list in the Royal Society's catalogue is correct, and in the tenth volume of that work we read that Müller's full name was Johann Friedrich Theodor Müller. The latter, no doubt, was information received from himself, but Müller does not mention this fact in a sketch of his life in his own hand that lies before us. He was a voluminous and steady worker, but his chief claim to remembrance is his book, "Für Darwin," which was a first-class contribution to the subject of Natural Selection, and was translated into English as "Facts and Arguments for Darwin," by the late W. S. Dallas. C. D. S.

BARON OSCAR DICKSON, who died at his estate Almnäs, near Gothenburg, on June 5, aged 73, used his opportunities as one of the wealthiest men in Sweden to succour all scientific and educational enterprise, and especially geographical explorations, most notable among which has been the voyage of the Vega through the north-east passage under Baron Nordenskjöld. Some years ago Baron Dickson offered to contribute largely to an Antarctic expedition under the command of Nordenskjöld, if the Australian colonies would help, but the scheme fell through.

There are also announced the deaths of:—MARTIN L. LINELL, assistant in the Department of Insects in the U.S. National Museum, aged 47; C. A. L. ROBERTSON, one of the editors of the *Journal of Mental Science*, and well known through his work in medical physiology; H. V. CARTER, for many years Professor of Anatomy and Physiology in the Grant Medical College at Bombay; E. RUSSOW, ex-Professor of Botany at Dorpat, on April 23, aged 56; CH. SCHOLZ, Professor of Geodesy in the Polytechnicum at Delft; TRAILL GREEN, first President of the American Academy of Medicine, and author of the "Floral and Zoological Distribution of the United States"; Dr DEROUBAIX, Professor of Medicine at Brussels University and assistant Court Physician, on May 22, aged 84; ANTOINE T. D'ABBADIE, formerly President of the Academy of Sciences at Paris, and the author of many valuable works on geographical exploration and geodesy; JOSEPH F. JAMES, at Hingham, Mass., on March 29 (of pneumonia), teacher of botany at the Cincinnati College of Pharmacy, Miami University, and Maryland Agricultural College, and formerly connected with the Division of Vegetable Physiology and the U.S. Geological Survey; EMILY L. GREGORY, professor of botany at Barnard College, U.S.A.; JAKOB BREITENLOHNER, professor of meteorology and climatology in the College of Agriculture, Vienna; SINKU SAKAKI, professor of psychiatry in the University of Tokyo; PETER D. KEYSER, formerly professor of ophthalmology at the Medical Chirurgical College, Philadelphia, and surgeon to the Wills Eye Hospital; LUDWIG HOLLAENDER, who wrote on dental anatomy; DR FEULARD, a well-known dermatologist, in the fire at the Paris charity bazaar; LUCIEN BIART, a French physician in Mexico, who had sent thence botanical and ornithological collections to the Paris Museum; L. JURANYI, professor of botany at the R. University of Hungary, and director of its botanic garden, on Feb. 27, at Abbazia, aged 59; EDSON SEWELL BASTIN, professor of materia medica and botany at the Philadelphia College of Pharmacy, and author of an "Elements of Botany," aged 54; the entomologist, C. J. J. M. BUGNION, on Jan. 19, at Lausanne, aged 86; the coleopterologist, J. HAMILTON, of Alleghany city, on Feb. 12, in Florida, aged 69; WILHELM HORN, director of the forestry research station in Brunswick, on April 4, aged 68; Mrs ALICE BODINGTON, a well-known and accurate populariser of science, at New Westminster, B.C.; the coleopterologists, H. D'ACHON in Orleans, and V. MAURICE TEINTURIER in Clayeures, France; at the beginning of April, the professor of geology and palaeontology at the Neufchatel Academy, LEON DU PASQUIER, aged 33; VICTOR LEMOINE, of Reims, who investigated the vertebrate fossils of the Lower Tertiary deposits near that city; EDMUND NEMINAR, formerly assistant Professor of Mineralogy and Petrography at Innsbruck University, on April 10, in Vienna; KARL KOLBEL, curator at the State Natural History Museum in Vienna, and specialist in Arachnida, Myriopoda, and Crustacea; on Ponape, one of the Caroline Islands, J. S. KUBARY, who had a wide acquaintance with the fauna and flora of the South Seas; on Feb. 7, in Lyons, the botanist, ALEXIS JORDAN, author of "Icones ad Floram Europae," aged 83; on Feb. 17, at Ashton-on-Ribble, the entomologist, J. B. HODGKINSON, aged 73; FRIEDRICH WILHELM KLATT, teacher of botany in Hamburg, on March 3; GEORGE W. TRAILL, the marine algologist; on Feb. 7, in Moscow, the curator of the Zoological Museum, ALEXANDER N. KORTSCHAGIN, carcinologist; on Feb. 27, at Luebo on the Kassai, Congo State, the Belgian botanist, ALFRED DEWÈVRE; on Feb. 28, at Grange-over-Sands, the Rev. JOHN EDWARD CROSS, author of a paper on the geology of N.-W. Lincolnshire, aged 73; on March 18, in Cassel, the ichthyologist, FRIEDRICH SEELIG, aged 69; Prof. HERMANN FRIEDRICH KESSLER, student of Aphides in Cassel; HEINRICH WANKEL, anthropologist, in Olmutz, aged 76; EMILE MAGITOT, President of the Société d'Anthropologie of Paris, and an eminent odontologist; A. STOCQUART, Professor of Vertebrate Anatomy at Brussels, aged 40; LEOPOLD MANEN, correspondent of the Paris Academy of Sciences in the section of Geography and Navigation, in May last; on Jan. 23, in Baltimore, Md., JOSEPH EWING MACFARLAND, who was connected with the U.S. Geological Survey, and had been doing field work in Tennessee; HUGH NEVILL, of the Ceylon Civil Service, at Hyères, on April 10, formerly editor and publisher of the *Tabropanian*, and a successful collector of zoological specimens, as well as of Ceylonese antiquities; Madame JEAN DOLLFUS, who for many years conducted *La Feuille des jeunes Naturalistes*, founded by her son, E. Dollfus.

NEWS

THE following appointments are announced :—

W. Garstang, of Lincoln College, Oxford, to be Naturalist to the Marine Biological Association of the United Kingdom; Dr Charles W. Dabney, jun., to be special agent in charge of Scientific and Statistical Investigations in the U.S. Department of Agriculture; Dr J. L. Prevost, to be full Professor of Physiology at Geneva; Dr E. Kaufmann, privat-docent in Anatomy at Breslau, to be Professor; Dr Max Wolters, privat-docent in Anatomy at Bonn, to be Professor; A. J. Moses, to be Professor of Mineralogy, and H. M. Howe, to be Professor of Metallurgy, in Columbia University; W. H. Lang, to be Lecturer, and Miss D. Clark, to be Demonstrator in Botany, and Miss M. Maclean, to be Demonstrator in Anatomy, at Queen Margaret College, Glasgow; Dr Fritz Frech, to be Professor of Geology and Paleontology, at Breslau; Dr Walter Kruse, to be Professor of Hygiene, at Bonn; D. W. Ule, to be Professor of Geography, at Halle; Dr Raphael Slidell, Freiherr von Erlanger, and Dr Paul Samassa, privat-docents in Zoology, at Heidelberg, to be Assistant-Professors; Dr J. Thomayer, to be Professor of Pathology, at the Bohemian University, Prague; Dr E. B. Copeland, to be Assistant-Professor of Botany in the University of Indiana, as successor to Dr G. J. Peirce, who becomes Professor of Plant Physiology in Leland Stanford Junior University; Dr Ivan V. Muschketoff, to be full Professor of Geology, at the Mining Institute, St Petersburg, in place of A. P. Karpinski, resigned; Dr A. Stoss, Prosector at the Veterinary College, Munich, to be Assistant-Professor in place of Prof. Johannes Ruckert, who goes to the University; at the Geologischer Reichsanstalt, Vienna, A. Bittner, as Chief Geologist, G. Geyer, as Assistant-Geologist, G. v. Bukowski and A. Rosiwal, as Adjuncts; Dr Tschirwinski, of Moscow, to be Professor of Pharmacology, at Dorpat.

THE widow of Prof. G. vom Rath has presented his library to the University of Bonn.

MR F. D. GODMAN has been elected president of the British Ornithologists' Union.

MR H. H. W. PEARSON has received from Cambridge University a grant of £100 for botanical research in Ceylon.

THE National Herbarium, U.S.A., is sending an expedition under Mr E. P. Sheldon, late of Minnesota University, to explore the Blue Mountains, Oregon.

THE memorial to the African explorer, Joseph Thomson, at his native place, Thornhill near Dumfries, was unveiled on June 8, by Sir Clements Markham.

THE *Windward* has taken out special stores for Mr Andrée, in case he meets with any accident and should be obliged to seek safety on Franz Josef Land.

A HISTORY of the Berlin Academy of Sciences is to be prepared by Prof. Harnack, and to be published on the 200th anniversary of the Academy's foundation.

THE city of New York is raising a loan of 500,000 dollars for the erection of a further wing to the American Museum of Natural History.

THROUGH the influence of President David Starr Jordan, says *Science*, arrangements have been made for the establishment of zoological gardens in San Francisco.

CAPTAIN ABNEY, who delivered the sixth Robert Boyle Lecture before the Oxford University Junior Scientific Club on June 1, took for his subject "The scientific requirements of colour photography."

THE first award of the Tilanus gold medal, to be decided every five years by the University of Amsterdam, has been made to Dr Zwaardemaker of Utrecht, for his work on the physiology of smell.

WE much regret to hear that there is talk of withdrawing the Government subsidy to the fresh-water biological station at Plön, where, under Dr Otto Zacharias, so much valuable work has been accomplished.

THE Russian Geographical Society and Academy of Sciences are sending an expedition to study the geography and natural history of the khanates of Roshan, Shignan, and Darwaz.

Mr R. C. L. Perkins, who has been investigating the zoology of the Sandwich Islands for a committee of the British Association and Royal Society has returned to England.

The following numbers of students at the Imperial College of Science, Tokyo, were recently given by *Engineering*:—Mathematics, 11; Astronomy, 2; Physics, 30; Chemistry, 15; Zoology and Botany, 12; Geology, 14. In all there are 89 students.

Dr Louis Gréhant, Professor of Physiology at the Musée d'Histoire Naturelle, Paris, has been awarded 1000 francs by the French Government to assist his researches on the hygienic applications of physiology.

An important change has just been made at the Spanish universities and other educational institutions under State control. Foreigners are now allowed to study there and to enter for the examinations, and to take degrees at the universities.

The Shute Scholarship in Animal Morphology, recently founded at Oxford University, has an annual value of £50, and is attached to no college. The examination takes place this July, and is open to all who may be in need of assistance at the university, and who have not been members of the university for more than eight terms.

The late Prof. Newberry, having left funds for the encouragement of scientific research, it has been decided to apply the grant successively to geology and palaeontology, zoology, and botany. A sum of 50,000 dollars will be awarded in the first subject, on July 15, to competitors from among the Scientific Alliance of New York City.

Cambridge University has made a grant of £300 to Prof. A. C. Haddon, to enable him to make an expedition to the Torres Straits to continue his researches on the anthropology of that region. He will be accompanied by other anthropologists from Cambridge, and by an expert in Melanesian languages.

The International Congress of Medicine and Surgery meets this year at Moscow during August. The Russian Government has not only contributed some £8000 towards the expenses, but has arranged for a two weeks' excursion to the Caucasus, in the course of which the mineral springs of Kislovodsk will be visited.

A laboratory for the study of cavernicolous animals has been started by Mr Armand Viré in some subterranean passages, recently rediscovered beneath the Jardin des Plantes and the Boulevard St Marcel, Paris. Water is supplied from springs by means of pipes.

It is expected that the *Belgica*, the ship of the Belgian Antarctic Expedition, will arrive at Antwerp early in July, and that the expedition will start in August. The expedition will stay from October to March on the eastern shore of Graham's Land. The following year, after re-coaling and provisioning at Melbourne, it will visit Victoria Land.

A survey of the Pribyloff islands is now being carried out by the U.S. Coast and Geodetic Survey.

The Danes are charting the northern part of the east coast of Greenland, with the help of some £1000 contributed from the Carlsberg Fund.

The Rev. Prof. Thomas Wiltshire, of Trinity College, Cambridge, for many years treasurer of the Geological Society of London, has presented his library of scientific works to the Woodwardian Museum, Cambridge. The donation comprises about 600 volumes and 900 pamphlets.

The Belgian Government has convened a second International Bibliographic Conference at Brussels, on August 2-4. Those who do not already subscribe to the Institut International de Bibliographie may become members for a subscription of 20 francs, on application to the Institut, 1 Place du Musée, Bruxelles. Among other subjects for discussion is the state of bibliography of the different sciences.

The Geological Department of the British Museum (Natural History) has just obtained an interesting series of teeth and jaws of the dwarf elephant discovered a few years ago by Dr Hans Pohlig in the cavern of Carini, near Palermo, Sicily. The species seems to be *Elephas mnaidriensis*, the largest form met with in the bone-caves of Malta. It may be merely a dwarfed race of the existing African elephant, which was stranded and gradually became extinct on the islands of Malta and Sicily when the land barrier, which once existed in that region between Africa and Europe, became destroyed.

A statement has lately appeared in many scientific journals that there exists on the Pamirs a dwarf tribe, with dwarf domestic animals. This appears to have originated from a journalist of St Petersburg. Lieut. Olofsen explains that the reference must simply have been to the Wakhanis, who are of true Aryan type and by no means dwarfs, although, owing to their mixture with Mongolians, they are not tall. Their domestic animals are half-starved but not dwarfed. Neither do the Wakhanis worship fire, as has been reported.

LIEUT. PEARY, having obtained five years' leave of absence, will start about July 10 for Whale Sound on the N.-W. coast of Greenland, leaving scientific parties on the coast of Labrador, Baffin Land, and Greenland. In July of next year, Lieut. Peary, accompanied by a surgeon and six families of Esquimaux, will push up the coast from Whale Sound to Osborne Fjord (81°N.), where he will establish a base of supplies in charge of some of the Esquimaux. About March of 1899 he will start for the north limit of Greenland, wherever that may be, and for the Pole.

THE Botanical Society of America will meet in Toronto, on August 17 and 18, immediately before the meeting of the British Association, under the presidency of Prof. J. M. Coulter. Dr C. E. Bessey, retiring president, will deliver his address on Tuesday at 8 P.M. All foreign botanists, of whom many are likely to be in Toronto, are invited to be associates of the society and to read papers.

THE following bequests of the late E. D. Cope are mentioned by *Science*: His scientific books, osteological collection, and collection of fresh-water molluscs, to the School of Biology of the University of Pennsylvania; his collection of minerals to the university; duplicates of fresh-water mollusca to the Cincinnati Society of Natural History and the American Museum of Natural History; spirit-specimens and skins to the Philadelphia Academy of Natural Sciences. The palaeontological collections are to be sold in three lots, viz. (1) the North American, (2) the South American, from the Pampean formation and West Indies and Mexico, (3) European collections, chiefly from the Neogene of Allier, France. After the payment of private bequests, the money arising from this is to found a professorship or curatorship in vertebrate palaeontology at the Philadelphia Academy of Natural Sciences.

AN important and urgent work is the collection of anthropological data from races that are disappearing or losing their old customs. For this purpose Mr Morris K. Jessup, president of the American Museum of Natural History, is fitting out an expedition under the leadership of Prof. F. W. Putnam, assisted by Dr Franz Boas. They will proceed up the north-west coast of North America, cross Behring Strait, and so pass down through eastern Siberia into China, and thence along the Indian Ocean to Egypt. The expedition will be away six years, and is expected to cost over 60,000 dollars. The pecial problem to be studied is the relation of the American races to those of Asia and Africa.

HENRY G. BRYANT, of Philadelphia, accompanied by S. J. Entrikin and E. B. Latham, has started for Alaska for the purpose of climbing Mount St Elias and making explorations in the adjacent region. Mr Bryant, says *Science*, has had experience of exploration in Labrador, and has made summer trips to Greenland. Mr Entrikin was with Peary in Greenland and made an expedition over the inland ice. Mr Latham is a member of the U.S. Coast Survey, and goes equipped for geographical work. The party, having established a base camp on the west shore of Yakatat Bay early in June, will cross the Malaspina glacier to the Samovar Hills; from there ascend the Agassiz glacier, and thence up the Newton glacier to the divide between Mount Newton and Mount St Elias. A camp will be established on the divide, elevation about 13,000 feet, from which the ascent to the summit of Mount St Elias will be made. On returning to the Samovar Hills the explorations will be continued westward through an entirely unknown region until a pass is discovered which will enable the explorers to cross the St Elias Mountains and gain one of the branches of Copper River. The return to the coast will be by way of Copper River. The party is well equipped, and has every prospect of success.

A PHOTOGRAPH of the new South African Museum at Cape Town, which, as we have stated, was recently opened, is given in *Nature* for May 13. The building is at the upper end of the Municipal Gardens, and consists of two floors, the upper of which contains the principal exhibition rooms. A room 63 by 41½ feet contains the birds, reptiles, and fishes of S. Africa, recent and fossil, while a room of equal size holds the general collection of vertebrates. The S. African mammals are in a smaller room, and another contains the anthropological collections both S. African and general. On the ground floor are four exhibition rooms, the two larger containing the invertebrates and the general geological collection; the two smaller, the collection illustrating S. African geology and mining and the local antiquities. Other rooms on this floor contain the library, study collections, and offices. The taxidermist's shop and store-room are in a separate building. All the cases are made of glass and iron (see Dr Meyer's letter in *Natural Science*, vol. ix., p. 142, Aug. 1896).

The Auckland Institute has decided to add a new hall, 50 feet square, to its museum, on the east side of the ethnographical hall. It is intended to receive the statuary presented by Mr T. Russell, C.M.G., which has hitherto found an incongruous home among stuffed vertebrates. The space thus gained will be occupied by groups of the larger mammals, and £100 offered by Mr Russell will be used to procure a group of the larger carnivores.

Little Barrier Island, on which an attempt is being made to preserve the indigenous fauna and flora of New Zealand, has been placed under the control of the Institute, with a grant of £200 for the first year's expenses. Mr R. H. Shakespear has been appointed curator, and it is hoped that he may be able to stop the depredations of collectors.

Mr H. C. Chadwick has been helping Mr J. J. Ogle at the Bootle Museum, and has rearranged much of the zoological series with elucidatory diagrams. Many new exhibition cases have been acquired, and in one of these the birds are to be rearranged, after consultation with a specialist. The museum lends specimens to teachers for the illustration of object-lessons, and the curator himself gives lectures, illustrated by the lantern, which appear to be much appreciated by young people.

According to the *Halifax Naturalist*, the Natural History Museum of Halifax, which was handed over to the County Borough Council about eighteen months ago by the Literary and Philosophical Society, has now found a permanent home in the old mansion named Belle Vue. The geological and botanical collections are very extensive and valuable, but zoology is as yet very imperfectly represented. The herbarium has lately been much enriched by the fine Gibson collection of British plants, the gift of Lady Trevelyan. The Curator, Mr Arthur Crabtree, is making a praiseworthy attempt to render the Museum of general educational value by adequate labelling, and we sympathise with him in his aspirations for a competent committee of management to direct and second his efforts. As he remarks, the ordinary municipal committees may be very admirable bodies, but they are not able to comprehend the requirements of a Museum of Natural History. He needs a committee of naturalists, of which there is no lack in Halifax, and his proposals deserve to be carried out immediately.

We learn from *L'Anthropologie* that the Museum of Moscow University has recently received from the Commission of the Archives of Riazan a collection of skulls, mostly prehistoric, of which the locality and conditions of finding are known in each case.

The same journal informs us that, near Elissavetpol, in the Caucasus, there have recently been found by seekers after copper, at a depth of 3 metres, a massive bronze bracelet and a copper spear-head of quadrangular shape and 35 cm. in length. In the same locality, on the banks of the Tchovdar, are traces of prehistoric mining.

Some three thousand prehistoric objects in bronze, iron, bone, and pottery, have been found on the site of ancient places of sacrifice of the Tchoudes at Gliadenevo, on the left bank of the Kama, near Perm.

At Chita in Trans-Baikal, the local branch of the Russian Geographical Society has founded a museum, which already contains valuable natural history, archaeological, and Buddhistic collections.

On September 15, 1896, the National Museum of Costa Rica was definitely installed in its new quarters, a two-storeyed building. A view of it is given in *La Revista Nueva* for October 1896.

Referring to this museum in his always valuable "Current Notes on Anthropology" (*Science*, March 19), Prof. D. G. Brinton says that few localities in America offer better specimens of aboriginal pottery and stone-work. The most abundant remains were left by the Guetares, a tribe of whose language and affinities we are still ignorant.

Plenty of attempt has been made to introduce some study of common objects or of nature into our elementary schools; but the obstacle generally lies in the teachers. A similar difficulty is felt in America, and to meet it the college of agriculture of Cornell University has undertaken to help, free of expense, all teachers who may wish to be put in the right way.

The same subject has been fully studied by a committee of sixty, appointed in May 1896, by the Chicago Institute of Education, and this committee has appointed sub-committees to prepare maps of the neighbourhood, to prepare printed outlines and suggestions for teachers, to look after appropriate books in the Chicago libraries, to supervise the work of instruction and keep touch with the individual teachers, and also to establish an exhibit of appliances, to arrange cheap means of transport, and to see to finance.

Those who are advocating the extension of the study of experimental psychology in this country will be glad to hear that it is proposed to establish a lectureship in that subject, including the physiology of the senses, at Cambridge University. They will not be so glad to hear that the salary is fixed at £50 per annum. Lecturing in an experimental science is not much use; one wants experiments, and for those one wants apparatus and a laboratory.

The books bearing on lichens and the dried specimens belonging to the late Dr J. Müller, generally known as Müller-Argau, became at his death the property of the Boissier Herbarium, at Chambésy near Geneva. Following the example of the trustees of another important lichenological library, the "Tuckerman Memorial Library" at Amherst College, Massachusetts, the directors of the Boissier herbarium have instituted the "Foundation Müller-Argau," and the curator, M. Eugène Autran, now appeals to botanists generally for copies of publications bearing on lichens which have appeared since Müller's death, or may hereafter appear. Also that specimens of new or rare species, or "materials for morphological and biological research" may be deposited in the Lichenotheca Universalis Müller-Argau, in which is included the herbarium of the Bernese F. Schaerer (1785-1853). Gifts will be acknowledged in the *Bulletin* of the Boissier herbarium.

In connection with the Autumn Meeting of the Iron and Steel Institute at Cardiff, on August 3rd to 6th, Professor Herman Wedding of the Berlin School of Mines has issued a circular letter asking for subscriptions towards establishing a Central Laboratory for the Testing of Iron and Steel. Such a Laboratory would be founded at Zurich under the auspices of the International Society for the Unification of the Methods of Testing Materials of Construction, which was formed in 1895, and the proceedings at which were reported in the *Journal of the Iron and Steel Institute* for 1895. The Secretary of the Institute, Bennet H. Brough, has consented to receive the names of those who are interested.

The Radcliffe Library at Oxford is known to scientific men for its wealth in their peculiar literature, and to librarians for the excellence of its arrangements. Such libraries grow rapidly nowadays. Want of room long felt has urged the Drapers' Company to offer to erect a new building, from plans by Mr T. G. Jackson, at a cost of £15,000. The offer has been gratefully accepted by the University, which proposes to transfer the space thus gained in the museum to the medical school, and especially to start a library of pathology.

The new museum at Winchester College, built as a memorial of the quingentenary of the school's foundation, was formally opened on June 16, 1897. There was an interesting exhibit of Wykehamical antiquities, and the art department made a fine show, but the arrangement of the natural history collections has hardly begun. When these last are more advanced, we shall hope to give a detailed account of the building and its contents in our series: Museums of Public Schools.

The first annual meeting of the South-Eastern Union of Scientific Societies was held at Tunbridge Wells at the end of May. The Rev. T. R. R. Stebbing presided, and delivered the inaugural address. He dealt with the changes in the attitude of the public mind towards scientific research within modern times. Mr W. Cole contributed the first paper on the objects and methods of local museums, referring especially to the new Epping Forest museum. Prof. Boulger next discussed the duties of the committees of Field Clubs. Prof. Seeley described a geological section in the New Athletic Ground at Tunbridge Wells, showing current-bedding in clay. "The Search for Coal in the South-East of England," the subject of a paper by Mr H. E. Turner, led to a long and interesting discussion. The Mayor of Tunbridge Wells extended his hospitality to the assembled delegates, and an interesting geological excursion terminated the proceedings.

CORRESPONDENCE

OCEANIC ICHTHYOLOGY

The reviewer of Goode and Bean's "Oceanic Ichthyology" (*Natural Science*, vol. x., pp. 338-340) has made a gratuitous assumption, which devolves on me unmerited credit and responsibility.

The reviewer says "it is to be noted that the literary part of the work bears signs of having been intrusted to a third author not under proper control of the two responsible authors." The "third author" is evidently myself. I feel compelled to deny either credit or responsibility for all that is not specifically accredited to me. Dr Goode devoted much time and thought to the keys, and I had nothing to do with the original compilation.

The specific information that certain fishes have and others have not a mesocoracoid, so far as the italicised portions credited to me are concerned, I am responsible for, but not entirely for their application.

The critic remarks, "as a matter of fact, the vast majority of the fishes placed here have not been examined with reference to this point." True; but enough have been examined to authorise the deduction formulated. The same argument might be adduced against other generalisations. . . .

As to the definition of Pterothrissidae, it will be evident from the context that the mistake is due to unintentional repetition instead of the requisite antithesis. The family Alepocephalidae is defined as having the "dorsal fin similar and opposite anal," and the Pterothrissidae should have been contrasted as having the dorsal fin unlike, and longer than anal. Undoubtedly carelessness and oversight are manifest, and I am willing to assume that I glanced over the proof and therefore to share the blame, but I had nothing to do with the original manuscript. Drs Goode and Bean, as well as myself, had much on hand, and doubtless the proof sheets were often read in a perfunctory manner. But in this respect, we have good company. . . .

Such mistakes unfortunately are too common and are generally designated as "slips of the pen" or "lapsus calami." I do not defend them, but it does not become the guilty to animadvert on them too strongly. . . .

What are or what are not family characters, is a question about which Dr Günther and I have long differed, and I wish neither to defend my view nor to attack those of others on this occasion. All the many American naturalists, at least, agree with me on such points. Other strictures I leave to the surviving of the authors, if he should deem them worthy of attention.

Some notice of the remarkable novelties obtained since the publication of the results of the *Challenger* Expedition might have been given by the reviewer, but I shall not trespass further on your space to do so. Theo. Gill.

[We regret that Dr Gill should assume our review of Goode and Bean's "Oceanic Ichthyology" to have been written or inspired by Dr Günther, who had no share whatever in its preparation. Like many unsigned reviews in *Natural Science*, it was the work of more than one author, and expressed the views of the editorial management of this journal, not those of any one individual. We have therefore omitted from Dr Gill's lettter some remarks on Dr Günther's works.—Ed. *Nat. Sci.*]

Slugs

I have recently received from an American malacologist a communication, in which he says that in a paper,* written in conjunction with Lieut.-Col. H. H. Godwin-Austen, I have "wrongly applied the term slug by using it for such genera as *Parmarion*, *Microparmarion*, &c."

It is very largely a question of individual opinion as to what genera should be included under this term, but I fail to see any reason why *Parmarion* and its allies should not be termed slugs.

The slugs are not a group by themselves which can be separated into distinct families apart from the rest of the Pulmonata. On the other hand, very many genera are closely related to genera in which there is a conspicuous shell. In the above-mentioned paper the same opinion was expressed as follows :—"We think that future

* Proc. Zool. Soc., 1895, p. 249.

research will clearly show that many of the slugs cannot rightly be placed in families by themselves, but will find their true position before or after the genera they have descended from or developed into."

I would, therefore, include amongst the slugs all forms of Pulmonate molluscs in which the shell is absent, or where, when present, it is incapable of containing the whole of the animal.—Walter E. Collinge.

BUFFALO *v.* BISON

Mr G. H. Carpenter doubtless does well to be shocked; but the *Scientific American* you failed to correct has a fellow-sinner in another American, whose claim to the title scientific not even Mr Carpenter would deride. Dr C. Hart Merriam, in *Science* for May 14 last, writes: "The familiar story of the vanishing buffalo is only one of many." It is to be feared that the influence of Fennimore Cooper is still strong even with the purest of the pure scientific writers.—Buffalo Bill.

We have received an interesting note from Professor A. S. Packard, in which he refers to our recent articles on the Arthropoda. In his well-known memoir on the brain of *Limulus* (Mem. Nat. Acad. Sciences, vol. vi., 1893) he has already pointed out that there are four lines of descent among these animals. Hence he considers the group to be polyphyletic. He now writes:—"I do not believe the Crustacea and Trilobites had a common ancestry. I think they evolved from separate vermian ancestors."

Our remarks on the so-called shooting of the golden eagle in Yorkshire have also brought us communications from Mr Ernest Bell and Mr G. W. Murdoch. The former urges the necessity of more stringent laws for the preservation of the rare British birds; the latter takes a more hopeful view of the case. It appears that Mr F. Boyes, of Beverley, a thoroughly competent ornithologist, has personally investigated the incident referred to in Mr Joseph Collinson's letter last May (*Nat. Science*, vol. x., p. 303), and finds that the bird in question was not an eagle of any kind, but a young rough-legged buzzard. Mr Murdoch adds:—"I can bear out Mr Southwell's statement (*Nat. Science*, vol. x., p. 432) that the golden eagle is in no danger of extermination in this country."

NOTICE

To Contributors.—All Communications to be addressed to the Editor of Natural Science, at 67 St James' Street, London, S.W. Correspondence and Notes intended for any particular month should be sent in not later than the 10th of the preceding month.

To the Trade.—Natural Science is published on the 25th of each month; all advertisements should be in the Publishers' hands not later than the 20th.

To our Subscribers and Others.—There are now published Ten Volumes of Natural Science. Nos. 1, 8, 11, 12, 13, 20, 23, 24 being out of print, can only be supplied in the set of first Four Volumes. All other Nos. can still be supplied at One Shilling each.

Price of Set of Vols. I., II., III., IV.	£2	10	0
,, ,, ,, V., VI., VII., VIII.	1	4	0
,, ,, ,, I.—VIII.	3	10	0

One Shilling each Number of any Bookseller.

Annual Subscription, payable in advance to J. M. Dent & Co., 67 St James's Street, London, S.W., Thirteen Shillings ($3·50), post free.

NATURAL SCIENCE

A Monthly Review of Scientific Progress

No. 66—Vol. XI—AUGUST 1897

NOTES AND COMMENTS

Co-operation among Naturalists

We had barely space in our last number to chronicle the second annual congress of the South-Eastern Union of Scientific Societies. We have now received *The Transactions* of the Union for 1897, price one shilling. The Union already includes twenty-seven affiliated societies, and in carrying out its motto, "Co-operation not uniformity," is doing a really useful work. The papers and discussions at the congress were for the most part thoroughly practical. It is recognised, as we have so often pointed out, that there is a vast amount of labour wasted every year by enthusiastic naturalists, whose misfortune it is to have no friendly and enlightened guidance. The aim of such bodies as this Union is to co-ordinate scattered and wasted effort, and to direct it into profitable yet no less fascinating paths. Thus it is suggested that the Union shall form research committees to deal with special branches of scientific observation. These committees would be similar to those of the British Association, but they would confine themselves to local natural history. Like the Midland Union or the New Zealand Institute, such an union may become the publisher for all its affiliated societies, and thus exercise a much-needed editorial discretion. It can also organise lectures and lecture-apparatus, making a collection of lantern-slides to be borrowed from by any society; this is already being done by the S.E. Union. Again, there are many legal questions affecting naturalists and local societies, and these can best be dealt with by a strong corporate body. The present congress discussed one such question, namely, "How can the Technical Education Grant assist local societies?" It appears that it is out of the question to ask for direct pecuniary assistance; but there seems no reason in justice or equity why local societies, engaged as they are in the education of the public, should not be allowed the use of a room in buildings erected with public money for purposes of technical education, and we trust that on this matter the Union may be able to enter into cordial relations with the County Councils.

Government and Provincial Museums

The meeting of the Museums Association, held this year at Oxford, July 6-9, was not largely attended, and did not produce a plentiful crop of papers. Even those that were submitted were not all read, owing to the necessity this Association always feels itself under of curtailing within narrow limits the time devoted to their reading and discussion.

The chief discussion took place on Prof. Flinders Petrie's suggestion of a federal staff for museums; by which he means that small curators should be abolished, their place being supplied by caretakers, and their work being done by peripatetic specialists. The proposal was thought impracticable; but there is no doubt that more might be done to encourage co-operation. There are two schemes that suggest themselves as the kind of ideals towards which we might strive. One is that each curator of a small provincial museum should endeavour to become thoroughly competent in some one branch of his work, and that for two or three months in each year he should change places with his fellow-curator from another museum—equally competent, but in another subject. Thus the museum, while paying one curator, would as years passed obtain the experience of a dozen. The alternative plan that occurs to us is that the staffs of the notoriously under-manned government museums should be increased, and that it should be part of the official duty of each specialist-curator to work for two or three months of each year at provincial museums. Government would, of course, have to levy some tribute from the provincial centres, to be applied to the salaries of the government officials; but apart from this there would be a gain to the specialist, to the head museum, and to the country, by the co-ordination, investigation, and effective utilisation of all our obscured scientific and artistic material, as well as by the increased sympathies, knowledge, and experience of the specialist.

At present government officials seem to hold somewhat aloof from the provincial museums, and from the Museums Association. Whether it be that the hard-worked civil servant can ill spare days from his holiday merely to talk shop, whether he thinks he will learn nothing from these meetings, or whether he really takes no interest in his life-work beyond the drawing of his salary, we do not know. At any rate the Museums Association recognises that it, for its part, has much to learn from the keepers and assistants in our larger museums, and it wishes that government officials could be given facilities for attending the yearly meeting not less than those accorded to nearly all provincial curators by the much-abused town and county councils.

The Protection of our Fauna and Flora

We have heard a good deal lately, both from naturalists and those whom the world in its rude way calls faddists, about the extermination of many of our native plants and animals. There is, unfortunately, little room for doubt that, however ill-advised may be the action of certain enthusiasts, their fears are on the whole well founded. "Naturam expellas furca, tamen usque recurrit," is only true up to a point: and when 'furca' has to be translated 'bricklayer's trowel,' that point has been passed. Those who wish to preserve at least a sample of what was once English country should read the level-headed paper sent by the Rev. J. J. Scargill of Bromley to the Congress of the South-Eastern Union. Here is a suggestion of his: "There are, perhaps, a dozen animals, furred or feathered, that are habitually killed by keepers. Let a man devote himself to investigating the habits (of course, in its wild state) of one of these—hawk, owl or magpie, stoat or weasel—noting all that it feeds on, and recording his observations day by day. In a few years, and with a sufficient number of observers, a fair estimate of the truth might be arrived at. It would be no easy task, but it would be good 'naturalists' work' worthy the attention of any follower of Darwin."

The want of thought that works so much ill can only be checked by the creation of a public conscience. "There are," says Mr Scargill, "several obvious means—1st, County Councils should be active in exercising their powers under the Act of last year, and prohibit altogether the taking or killing of such birds as those just mentioned; 2nd, the editors of natural history magazines should never let an issue pass without a few words on the subject; 3rd, instruction on the duty and the reasons for it should be periodically given in every school."

As for the naturalists themselves, especially those whom Mr Scargill describes as "the camp-followers of science, eager for the loot, but inclined to shirk the discipline," they may remedy matters in two ways. First, let them collect only for their local museum, and themselves pay more attention to the habits or the structure of the animals and plants they meet with; secondly, let them leave the butterflies and the petaliferous plants alone for a time: they will find the flies, the grasses, the mosses, the marine invertebrates quite as interesting and far more profitable. Let us add that there is never any harm in collecting fossils, for they are dead already; it is only necessary to keep careful account of locality and horizon, and to spare the fossiliferous stones of ancient buildings. There is plenty of sport in fossil-hunting, and the merest of mere collectors may provide the most philosophical of palaeontologists with valuable material, and in this way increase the value of his own collection.

Frou-frou and Feathers

All moralists have assured us that "when lovely woman stoops to folly," she stoops very low indeed. And so when women attempt to emulate the glories of a Choctaw chief or a South Sea islander, it is not considerations of art or humanity or self-respect that will stop them. Consequently it is not likely that the insensate votaries of fashion, who disfigure their heads with baskets of artificial flowers (irrespective of the season), virulently dyed scraps of ribbon, twists of steel, and unnaturally clipped or coloured bird-feathers, will pay any attention to a paragraph in a scientific journal. But we are willing to leave the irresponsible half of creation all their *chiffons* (which mean 'rags' or 'women's dress' as you please), their coal-tar dyes, and their scrap-iron, if only they will leave us our birds. The rate at which some of the rarest and most beautiful birds on our planet are being destroyed to gratify this extraordinary taste can hardly be realised. On the 13th of April last nearly half-a-million birds were sold at an auction in London, and the details of the consignment were thus given by Mrs Edward Phillips at the annual meeting of the Selborne Society:—

Osprey plumes,	11,352	ounces
Vulture plumes,	186¾	pounds
Peacock feathers,	215,051	bundles
Birds of Paradise,	2,362	
Indian parrots,	228,289	
Bronze pigeons, including the goura,	1,677	
Tannagers and sundry birds,	38,198	
Humming birds,	116,490	
Jays and kingfishers,	48,759	
Impeyan and other pheasant and jungle fowl,	4,952	
Owls and hawks,	7,163	

A similar sale took place in February, and others were to follow in July and October.

It is small consolation to us to think that in a few years the price of these luxuries will be prohibitive, or that, unless fashion changes in the direction of sea-weeds or turnip-tops, there will soon be no more birds to destroy. Nor can we overlook the terrible suffering involved by this enormous slaughter: the young osprey bereft of its parents left to die in hundreds, the heron with the plumes torn from its back, writhing into death. But Frou-frou cares for these things no more than she does for the squalor of East-end sweating-dens. Dear delightful doll that she is, she actually attends a meeting of the Selborne Society with aigrettes in her bonnet.

What can we do? Frou-frou does not read *Natural Science*. But at all events each of our many thousand readers must enjoy the acquaintance of many ladies. He can at least use his influence in a quiet way in the home-circle, if not beyond it. If each of us will make sure of a few facts, and keep pegging away, perhaps we may even make converts, and so widen the small circle of our influence.

National Geology

The annual report of the Geological Survey of the United Kingdom for 1896 reaches us in its handy separate form, and each year's issue contains a wealth of information about our islands. Sir A. Geikie's far-seeing policy of attaching to the Survey men already qualified by original research must tend to increase still further the scientific character of its publications. Though the results may never appear in so handsome and truly national a form as do those of the United States, yet this annual summary shows strikingly the character of the work in hand. Teachers can now, for the sum of sixpence, keep abreast of the advances made by the Survey in England, Scotland, and Ireland; and, as all practical workers know, these advances often concern even the broader boundaries on the map. The classification of results in this year's report under the several geological systems makes reference easy through its hundred closely-printed pages. We would especially direct attention to the progress of knowledge with regard to the pre-Cambrian and older Palaeozoic groups. The occurrence of widely-spread diabasic lavas with 'pillow-structure' (p. 37), and of two abnormal short-lived volcanoes in Raasay (p. 74), may be cited as among the interesting igneous problems dealt with. One of the most important stratigraphical questions is the relation of the 'Upper Greensand' to the Upper Gault, referred to on p. 72.

It is obviously impossible to continually re-edit the engraved maps of the Survey so as to embody current progress. If our Parliamentary legislators, however, were more frequently trained in scientific schools, they would find much to be proud of in these annual reports, and would congratulate the State and themselves on that zeal for discovery which raises the work of a public department into one of international distinction.

Microscopy in Manchester

The Manchester Microscopical Society has recently issued its "Transactions and Annual Report" for 1896, and an excellent little publication it is. To our way of thinking it is almost a model of what such a publication should be. Without claiming to contain the results of elaborate original research, the papers are nevertheless of a very useful and suggestive nature. Prof. Weiss, of Owens College, in his presidential address gives a very good account of the main facts known of the biology of those, from some points of view, exceedingly familiar organisms, the diatoms. We believe that the type of microscopist known a few years back as the 'Diatomaniac' is wellnigh extinct, but if a few individuals of the species still exist we feel sure a perusal of Prof. Weiss's paper would do much to broaden their views of things in general and of diatoms in particular. We heartily endorse the sentiment of Prof. Weiss when he says, "I should like to plead for the union of two branches of study, the systematic and the physiological or biological, the severance of which is greatly to be regretted, and has proved wherever it occurs to be a hindrance to the real progress of Natural Science." Other papers in the *Transactions* deal with "The Method of Reproduction in Plants," more especially the microscopical forms, "The Structure and Development of the Hydrozoa," "The Lace-work Sponge," "The Defensive Devices of Lepidopterous Larvae," "The Entomology of the Oak," and "The Distribution of the Fresh-water Fauna." The latter is by Prof. S. J. Hickson, and contains many most interesting facts and suggestions. Prof. Hickson considers that the facts of distribution teach us that fresh-water animals may be divided into three groups: (1) the Cosmopolitan group, including the large majority of fresh-water species; (2) the Archaic group, represented by such forms as *Apus* and *Limnocodium*; and (3) the Recent group, comprising species which have only recently migrated into fresh water, such as *Cordylophora* and some of the prawns. The problems to which this paper draws our attention increase our regret that this country is still without any prospect of a fresh-water biological station.

Altogether, judging from the papers and report, the Manchester Microscopical Society seems to be in a very vigorous condition, which is something to be thankful for in these latter days when local societies are too often more asleep than awake.

The Apparent Disappearance of the British Phyllopods

In the course of the paper alluded to above Prof. Hickson makes the very positive statement that the phyllopod *Apus* does not occur at all in the British Isles. This may be true, in fact we are almost forced to believe that it is true; but it is also certain that this animal used to live in this country, for Dr Baird records it as having been taken by three different observers, although he does not appear to have found it himself. Since the publication of the "Natural History of the British Entomostraca" in 1850, *Apus cancriformis* has never again been recorded, so far as we are aware, and it does seem almost certain that it has totally disappeared from our fauna. The same fate also seems to have overtaken the brine-shrimp, *Artemia salina.* In Baird's time this form was to be found in the 'salt-pans' at Lymington, and probably other places, but at the present day one may search the old 'salterns' in vain for any trace of the creature. With regard to the beautiful *Chirocephalus diaphanus*, which Baird mentions from a large number of localities, the facts scarcely warrant our regarding it as totally extinct. It has certainly been seen several times since 1850. Prof. G. S. Brady mentions it from Yorkshire, and more recently it has been taken near Birmingham; nevertheless for all practical purposes it now appears to have disappeared. We should, however, be exceedingly glad to hear if any of our readers have taken this form, say, within the last ten years.

Prof. Hickson attributes this dying out of Phyllopoda—he refers to *Apus* more particularly—to the very limited means of dispersal which these creatures have at command. They are comparatively large forms, and cannot therefore be transported, attached to birds' legs, &c., so readily as the smaller and commoner Entomostraca. In addition to this they do not produce specially protected eggs like many of the Daphnias, &c. It seems probable, therefore, that, not being provided with the means of transport found in the cosmopolitan fresh-water forms, the phyllopods have not been able to extend their geographical distribution, while owing to the drying up of old lakes, and other changes, the localities in which they occur are becoming fewer and fewer.

The Bibliography of Science

Of course we are glad to find that Mr Arctowski's article on the Genealogy of the Sciences, which appeared in *Natural Science* for June 1897, should so have pleased the editors of the *Bulletin de l'Institut International de Bibliographie* that they should have published a French version of it in their number just received by us.

None the less we are surprised that a journal with so high-sounding a title should so ignore the objects of its existence and the responsibilities that it has assumed, as entirely to omit all reference to the original place of publication of the article. We may also point out the absence of an exact date of publication from the wrapper, the pages, and the included catalogue-slips of this *Bulletin*. To parody an old saying, we must really cry, "Bibliographer! bibliograph thyself."

The Belgian bibliographers seem to have found Mr Arctowski's article as unpractical as interesting. One thing is certain, we are not going to wait—not even the Royal Society Committee—for someone to write us a phylogenetic history of science. Therefore the impossibilities of the suggested classification do not greatly matter. At the present moment work is being done in the bibliography of science on a definite and uniform plan, which may be ridiculous, incorrect, confusing, but which is workable and being worked. There are no doubt plenty of beautiful, symmetrical schemes, as clear as daylight, but they are not in use. The following bibliographies are announced by the *Institut International* in a catalogue of its publications:—Bibliographia Philosophica, B. Sociologica, B. Astronomica, B. Zoologica, B. Medica Italica, B. Anatomica, B. Physiologica, B. Ostetrica e Ginecologica Italiana, while there are in preparation a Bibliographica Geologica, B. Physica, B. Medica Belgica, B. Agronomica Italica, and others.

Some of these bibliographies represent the adhesion to the uniform plan of periodicals or societies hitherto working on other lines, such as the *Zoologischer Anzeiger*, *Anatomischer Anzeiger*, and *Il Policlinico*. We notice too that the Biological Society of Paris accompanies its 1896 volume with an analytical index to the articles, arranged on the principles of the decimal classification. All the subjects dealt with in over 300 articles are thus referred to in two pages. These and numerous other facts, which it would be wearisome to detail, show that the system is gaining ground, whereat many will marvel.

A Biological Record

Yet another form of scientific bibliography comes to us in *L'Année Biologique*, further described as "comptes rendus annuels des travaux de biologie générale publiés sous la direction de Yves Delage, professeur a la Sorbonne avec la collaboration d'un Comité de Rédacteurs." The secretary to the editors is Dr Georges Poirault. The work is published by Schleicher Frères, 15 Rue des Saints-Pères, Paris, at a price of 20 francs. The first volume, just received by us, deals with the literature of the year 1895. We may describe it by saying that it treats of those papers that are noted by the zeal of Mr J. Arthur Thomson in the first section of our own *Zoological Record*, and that the plan of the work is like that of the *Zoologischer Jahresbericht* and the *Neues Jahrbuch für Mineralogie* combined. There are 53 collaborators, mostly French, so that the task of abstracting is pretty sure to fall into competent hands. The want of correlation to which this leads is compensated by the several introductions as well as by special articles on general subjects—*e.g.*, on grafting, by L. Daniel; experimental knowledge of the correlation of animal functions, by E. Gley; on polyzoism, by J. P. Durand.

As to what is meant by biology, there is always a quarrel simmering. It is not long since we received an elaborate discussion of the subject from Mr Henry de Varigny, extracted from the "Dictionnaire de Physiologie." He defined it as "the science of the relations of organisms to the environment and to other organisms, present and past." Professor Delage, in his Preface to the present work, does not waste much time in discussing what is or is not biology; for practical purposes, as a criterion of what shall be included in *L'Année Biologique*, he accepts every paper that professes to give an explanation of biological phenomena (*i.e.*, of the phenomena of living beings). It is easy for an analyser or recorder to see whether an author professes to explain. But Prof. Delage has opened a loop-hole for complaint, since he also promises to record facts that may be connected with some future explanation, or even those which "belong to general biology, and are not of the same nature as others already known." Who is to decide what facts will ultimately be of value in the explanation of our ever-varying problems? Each day has its own burning question, casting others into the shade; and what the riddle of to-morrow may be we know not. Facts that were passed over a few years ago are all-important now. What facts shall we be collecting twenty years hence? But, apart from this difficulty, only to be overcome by a prophet, there is the certainty that hundreds of facts undoubtedly worthy of record from Prof. Delage's point of view, will be overlooked by himself and his collaborators. It does not take us five minutes to discover a score of such facts, published during 1895, often with full knowledge of their import, but nowhere alluded to in this volume. We do not blame their omission, for we cannot think that anything else is to be expected on the present system of compiling bibliographies.

Taking this work for what it really is, and not for its unattainable ideal, we recognise that it is relatively complete; that it is well arranged and well executed, profiting by the experience of predecessors. It is an aid that should be neglected by none with a soul above species-mongering and section-cutting; in other words, it will be welcome to all readers of *Natural Science.*

NATURAL SCIENCE IN JAPAN

THE historian of science in Japan is too apt to restrict his view to the influence of European science, to the introduction of Newtonian and Darwinian philosophy and of the Linnean system, forgetful that there were philosophies and systems in the Far East centuries before, or else thinking wrongly that these were of small account. In an admirable Introduction to the first number of *Annotationes Zoologicae Japonenses,* Prof. K. Mitsukuri corrects this error. Early in the eighth century of the Christian era there was already established in Japan an Imperial University with 400 students, devoted to Ethics, History, Jurisprudence, and Mathematics. There was also an office for Astronomy, Astrology, Calendar-compilation, and Meteorology, as well as a Medical College with professors of Medicine, Surgery, Acupuncture, Necromancy (the art of healing by charms), and Pharmacology. In connection with the last-named branch of study, much botanical information was acquired. Towards the end of the ninth century the large Imperial library contained numerous medical works, among others, on the diseases of women and the diseases of the horse. In later times, under the Tokugawa Shoguns, natural history, especially botany, was extensively studied, and elaborate works were published, of which Prof. Mitsukuri instances the 'Shobutsu Ruisan' issued early in the eighteenth century, and the "Honzō Kōmoku Keimō" by the celebrated teacher of natural history, Ono Ranzan, published in 1803. *Honzō,* which strictly means botany, seems to have come to include general natural history, for many of these works deal also with stones, metals, and all kinds of animals. In Rosny's "Catalogue de la Bibliothèque Japonaise de Nordenskiöld" we even find a note on fossil shells, which appeared so early as 1725. The naturalists also held meetings at which they exhibited their treasures to one another and to the public. The Garden of Medicinal plants at Tokyo was established in 1681.

It was during the eighteenth century that western science first came into contact with the Japanese, through the medium of the Dutch language. The story of this and of the gradual development of modern science in Japan has already been told in our pages by Mr F. A. Bather (vol. iv., Jan., Feb., and March 1894); but many details are added by Prof. Mitsukuri. Zoology, he notices, had developed but slightly before the restoration of the Mikado in 1868; it was not till the appointment of Prof. E. S. Morse to the Chair of Zoology at Tokyo University, in 1877, that it made any progress

The indefatigable American popularised the science, secured a band of earnest students, established a museum, and organized the Tokyo Biological Society, now the Zoological Society. He was succeeded by C. O. Whitman, who introduced modern technical methods.

Since 1881 the development of zoology in Japan has been entirely in native hands, and does not seem to have suffered from that cause. All its main branches, including its practical applications, are now fairly represented. The Marine Station at Misaki has been outgrown, and a larger one is being opened two miles north of the present building. The teaching of zoology in the various schools over the country is a recognised thing. Further, the addition of Formosa to the territory of Japan has already been taken advantage of by Japanese zoologists. One thing is wanted, and that is literature. Prof. Mitsukuri appeals to the naturalists of other countries to send their publications to the Imperial University, where they are sure to be warmly appreciated.

Growth-Changes in the Spicules of Sea-Cucumbers

The sea-cucumber, trepang, beche-de-mer, or holothurian, is well known to be a favourite article of food in the Far East; especially is this the case with the common *namako* of Japan. For the protection and cultivation of this animal, Prof. Mitsukuri some time ago began an inquiry, at the instance of the Ministry of Agriculture and Commerce. One of the first questions to be answered was the number of species, if there were, as was supposed, more than one. The species of holothurians are often determined largely by differences in the form of the minute calcareous spicules found in the skin. Now it so happened that those who had examined this Japanese holothurian—namely, Selenka, Von Marenzeller, Lampert, and Théel—had failed to find the same appearances in the spicules, and had founded two species, *Stichopus armatus* and *S. japonicus*, together with a variety of the latter, called *typicus*. The shape of the spicules is that of a minute one-legged table made of open fretwork; but some individuals contain no tables at all, only smaller spicules something like round buttons with four or five holes in the middle. Prof. Mitsukuri's investigations, now published in *Annotationes Zoologicae Japonenses* (i. pp. 31-42), show that all these forms belong to *Stichopus japonicus*, and that in this species the form of the spicules changes with advancing age. The youngest individuals have an extremely large number of most perfectly formed large-sized tables, and nothing but these. With the growth of the animal, perfectly formed tables decrease both in number and size, and tables in various stages of arrested development are found mixed with them. This process continues with age, until in fully grown individuals there are found nothing but the small plates abovementioned. These represent a small part of the original table top, and are comparatively thinly scattered in the skin.

In the youngest stages the calcareous deposits are the most complete, and have almost the character of a coat of armour, like that of a star-fish or sea-urchin. This may be for the greater protection of the young, in which the skin and muscle-layers are very thin and pliable; but it may signify the descent of the species from a more richly plated ancestor. These differences are not entirely signs of age, but, in conjunction with others, distinguish geographical races. Thus forms with spicules in the shape of buttons are more common in the north of Japan, and are also characterised by numerous long-pointed papillae set in four rows along the back and sides, with many smaller papillae between them. As one passes southwards along the coast one comes gradually to forms that have only a row of low papillae along the sides, and a few scattered over the back. Habitat, however, has its influence no less than latitude. Those that live among rocks have a larger number of tall papillae, and are of a mottled brown colour, while those that live on sandy ground, probably among sea-weeds, have lower and fewer papillae, and are of a dark-green colour.

This interesting and doubly important paper makes one doubt afresh the validity of the many species of holothurians that have been based on the examination of the spicules of a few individuals; it shows the necessity for the examination of many specimens in various stages of growth from different localities; and it affords one more demonstration of the value of the study of all growth-changes and not merely of those that occur in the embryo.

How a Brittle-Star Lives in Japan

While the Japanese zoologists, K. Mitsukuri and T. Hara, were on a collecting tour last year, they came, on April 1, to a sandy shoal in the Bay of Kagoshima. Wading into the water, they were soon struck by very curious objects. "Numerous slender stalks a few millimeters in diameter and 10-15 centimeters high were standing up from the bottom, looking like the stems of so many weeds. Along one side of each stalk there was, however, a row of white papillae-like structures. These stalks were mostly by twos, although sometimes only one was standing by itself. We do not remember having seen three making a group. As we dug, to learn more about these curious objects, we were greatly surprised to find that they were the arms of ophiurans, and that the papilla-like structures were, therefore, no doubt, tubefeet. So far as we could see there was no difference between the five arms of the animal, and why only one or two of them should be thus thrust upwards into the water, and kept upright there, was a mystery. It seemed probable to us that it was done to secure respiration. The sand of the shoal was literally packed with these animals, and there must have been hundreds of thousands or, perhaps, millions in the whole shallow." The species was near or in the genus *Ophiopsila*. The account is extracted from the miscellaneous notes in the first number of *Annotationes Zoologicae Japonenses.*

A Botanical Discovery from Japan

Botanists became greatly excited when, several years ago, Treub published an account of his discovery of Chalazogamy in *Casuarina*. By this term, as our readers may remember (see *Natural Science*, vol. i., p. 132) he described a method of pollination, in which the pollen-tube entered the ovule through the chalaza instead of at the micropyle. Treub was so much impressed with the importance of this and other deviations from the normal course of events in *Casuarina* that he separated it from the rest of the seed-plants under the name *Chalazogamae*, the latter, in which presumably pollination was effected through the micropyle, forming the *Porogamae*. More recent work has shown this revision of our classification to be unnecessary, and that *Casuarina*, though certainly presenting remarkable anomalies, must still be retained among Dicotyledonous Angiosperms.

There has recently come from the far East news, and confirmation of the news, of a yet more startling discovery. S. Ikeno and S. Hirase, working at Tokyo in Japan, have found that in the process of fertilisation in *Cycas* and *Gingko* the male element (generative nucleus) is converted before its escape from the pollen-tube, into a motile spermatozoid. This swims through a quantity of sap occurring in these genera between the embryo sac and the top of the nucleus which forms a thin papery covering for the contents of the ovule, and impregnation of the oosphere is therefore effected in the same manner as in the Vascular Cryptogams. The spermatozoids are much larger than hitherto known among the Cryptogams, and that of *Cycas* is larger than that of *Gingko*. The shape is oval. The head consists of three spiral windings in *Gingko*, and of four in *Cycas*, and bears numerous motile cilia. The great importance of the discovery of the Japanese botanists lies in the fact that it strengthens our present system of classification. Hofmeister showed the near relation subsisting between Gymnosperms and Vascular Cryptogams working chiefly from the development of the female spore (embryosac) and the structures resulting therefrom. Now from the male side comes a striking confirmation of his conclusions,—a confirmation which, as Messrs Ikeno and Hirase point out, Hofmeister had suggested would be forthcoming. It is of much interest that Conifers represented by *Gingko*, as well as Cycads, show this relation; as we have always been wont to consider the latter so much the more ancient group both on palaeontological and morphological grounds. And *Gingko*, the maiden-hair tree, which with its strange fern-like foliage and non-conelike inflorescence, has always attracted us, will become still more fascinating. The confirmation of the news to which we have referred was supplied by Dr Scott, who at the last meeting of the Linnean Society showed actual microscopic preparations which he had received from Japan. A few more details will be found in a note communicated by the discoverers to the June number of the *Annals of Botany*.

Fungi and their Hosts

It is generally understood that a fungus, when parasitic, preys upon one and the same host during the whole period of its life-history. Hitherto only a single exception to this rule has been recorded, namely, that of certain 'rusts' (Uredineae), whose heteroecism (as change of host is technically termed) was first demonstrated satisfactorily by De Bary in 1864. Now, however, the Russian botanists Woronin and Nawaschin (*Zeitschr. für Pflanzenkrankenheiten*, vol. vi., 1896, pp. 129, 199) have discovered an interesting case of the same exceptional phenomenon, namely, in a new species of the Ascomycetes which they have described and named *Sclerotinia heteroica*. The resting-stage (or Sclerotium) giving rise to the *Peziza*-form grows in the capsules of *Ledum palustre;* the other (or conidial) form they found as a destructive parasite on the leaves of *Vaccinium uliginosum*. The fruit of *Ledum palustre* is attacked at an early stage of its growth, and is gradually replaced by the sclerotium. The diseased capsules, which do not differ much in appearance from the healthy fruits, remain attached to the parent plant during the winter, and fall to the ground in spring when the stalked cup-shaped ascus-fruits are developed. The ascus spores, scattered by the wind, light on the buds and young leaves of *Vaccinium*, where they germinate and spread through the cells of the plant. The conidial fructification, upright stalks with branched chains of conidia, appear on the petiole and veins of the leaves, which turn brown and gradually die. The authors by repeated experiments established without doubt the relation between the two forms; but it is rather remarkable that they were able to cultivate the conidial form from the ascus spores on a decoction of plums; and this fact, as pointed out by Fischer, interferes between the parallel with the above case and that of the Uredineae. The latter are obligative parasites, and no culture medium has been substituted at any stage for the living host plants.

There are a number of parasites among the 'Fungi imperfecti' which may prove to be heteroecious forms of parasitic ascomycetes, and it would be well worth while to carry out further experiments on the subject.

A Bacterium Living in Alcohol

During the last year much of the rum manufactured in Demerara has been found to be 'faulty,' and, the cause having been sought for in vain, great loss has resulted to the colony. Mr and Mrs Victor H. Veley, of Oxford, have recently discovered a micro-organism in some samples of faulty rum sent them for examination. The bacterium belongs to the group *Coccaceae*, adopting Zopf's classification, and is probably a new species. Mr and Mrs Veley have already obtained several stages in the life-history, by cultivation, and hope shortly to publish an account of its development and the chemical changes which it produces in the liquid. The fact of any micro-organisms existing and multiplying in spirit correctly assessed at 42° over proof, or about 74·6 % by weight, is of great interest both from a scientific and technical point of view, and the investigation is likely to prove of considerable importance.

John Jeffrey

In the *Proceedings* of the Biological Society of Washington (vol. xi., pp. 57-60), Mr F. V. Coville gives a sketch of the route taken by John Jeffrey, "one of the most obscure" of the botanical explorers who have done important work in North America. Botanists know him only as the subject of the dedication of a Californian pine (*Pinus jeffreyi*), described by Andrew Murray from material sent home by Jeffrey. The brief account of his work as a traveller and collector has been drawn up by Mr Coville by the aid of documents both manuscript and printed, which have hitherto been almost unknown, or at any rate unexamined. We know that Jeffrey was a Scotsman, and that in 1850 he was sent to North America under the auspices of an organisation formed in Edinburgh, with Prof. J. H. Balfour as chairman, and known as the "Oregon Botanical Association." He was to go to Western North America, and collect the seeds of trees, shrubs, and other plants suitable for horticultural purposes, in the region traversed by David Douglas, "to complete his researches, and to extend them into those parts of the country not fully explored by him." Starting from York Factory on Hudson Bay in August 1850, he worked by way of the Saskatchewan and Athabasca rivers to the Rocky Mountains, which he crossed between Mounts Brown and Hooker, and then descended the Columbia river to Fort Colville. He arrived at this place in May 1851. The next two years were spent in exploring the coast region between the Fraser river and San Francisco. Collections were made on Mount Baker, the Cascade Mountains, the Sierra Nevada, and other ranges in Southern Oregon and California, and along many of the river valleys. Several collections were sent to Edinburgh, the last being those made in 1853, when his term of employment by the association ceased, the original contract being for three years' service. A letter to Andrew Murray from a brother in San Francisco, dated May 1854, gives the last information we have of a hard-working and enthusiastic but ill-fated botanist. He planned an expedition to Fort Yuma on the Gila river in Colorado, from which he never returned, and there seems little doubt that he perished of thirst in the desert.

The Camel in Europe

It is difficult to determine the natural geographical distribution of an animal which has been so long domesticated as the camel. Discoveries of its remains in surface-deposits need to be carefully investigated by competent geologists before they can be accepted as actual fossils, not as bones merely buried by man. Great interest therefore attaches to an announcement by Dr G. Stefanescu, the eminent Roumanian geologist, of the discovery of two portions of the mandible of a species of *Camelus* in an undoubted Quaternary gravel, six metres below the surface, on the river bank of the Olt at Milcovul-de-jos, near Slatina, Roumania (*Anuarulu Mus. Geol.*, Bucharest, 1895). Dr Stefanescu disinterred the specimens himself, and there can be no doubt as to their geological age. He regards the species to which they belong as new, and names it *Camelus alutensis*. We believe that there are similar fragments from the Volga basin in the collection of Prof. A. Rosenberg of Dorpat (Jurjeff), but we are not aware whether any account of these has been published.

Steenstrup

We regret to record the death of the *doyen* of Danish zoologists, the veteran Prof. Steenstrup. We hope next month to publish a short account of his life and work by Prof. Chr. F. Lütken, with a recent portrait.

I

The Influence of Woman in the Evolution of the Human Race

THE recent discussions of Mr Reid's book, "The Present Evolution of Man," in *Natural Science* (vol. x., pp. 184, 242, 305, 393) have interested me, both on account of their able treatment of this subject from so many different sides and also on account of their omissions of certain points of view. Man's place in nature, the possible influence on his destiny of the position he occupies as the terminal form of his own group, should, it seems to me, be given more consideration as a possible factor in his evolution. This has received incidental consideration by the writer in connection with studies upon the phenomena of evolution among the Invertebrata, especially Cephalopoda, and the results are instructive and quite similar to those reached by the distinguished English palaeontologist, Mr S. S. Buckman.

The way in which man's position may possibly affect his evolution and further prospects has been treated by the writer in a lecture upon "Woman's Occupations and Habits and the Suffrage from a Biological Point of View." This can be used as an example of a certain mode of treating the subject, and an abstract of this lecture may perhaps interest the readers of *Natural Science.* It is also appropriate that it should appear first in an English periodical, since, if the reports are true which reach this side of the ocean, some leading Englishmen are so sadly deficient in knowledge of the subject and its importance, that they consider the question of whether the suffrage shall or shall not be granted to women as a huge political joke rather than as a question dealing with matters of importance to the future evolution of civilised races. People do not yet recognise that the tendency of evolution is quite as often towards retrogression and extinction as in the direction of progression; the former indeed being the final result both in the life-history of the individual and of his family, and finally of the race to which he belongs. The laws of biology have not hitherto been used to test the assumptions, that co-education and the changes of occupations and habits induced thereby and by the legal freedom of choice of occupation conferred by the use of suffrage upon women, will be beneficial factors in the evolution of the future. The writer has thus been endeavouring to call attention to this side of these questions in Boston and Cambridge, U.S.A., and the following is a brief abstract of the arguments employed in the lecture referred to, lately delivered in these two cities.

(1) Men and women, like the males and females of most animals, show by their organisation that they have been evolved from a type in which both sexes were combined in the same individual. The separation of the sexes did not destroy this dual nature, as is demonstrated by the development of secondary male characters in the old age of many species of animals and of women in extreme age, and of feminine characters in aged men. This opinion can also be supported by the structure of the tissue cells in the body, the nuclei of which are made up of paternal and maternal parts. This dual structure enables us to understand the fact that secondary sexual characters are latent in both males and females, and liable to make their appearance after the reproductive period is passed through, or before this time and prematurely in abnormal individuals, or perhaps under certain conditions of habit or surroundings.

The maternal (in larger degree or wholly feminine) parts of the nuclei are certainly prepotent during the entire reproductive or adult stage of growth, and their constant employment in the performance of feminine functions prevents the development of latent male characters. During this time the paternal (in larger degree or wholly male) parts of the nuclei have remained inert and may be supposed to be still capable of multiplying by division and producing extra growths, thus even in old age building up secondary male characters, such as the comb, wattles, etc., in some birds, or giving rise to secondary male characteristics in old women. This may also take place prematurely through suppression of the natural functions, either by change of habits or by surgical or other artificial operations. These statements apply equally well to men, and some of the most remarkable examples are to be found in this sex, but the dangers of feminisation to the men, although possibly greater than we now suppose, do not seem at least to be so important or threatening as those that lie in the possible future of the women. These are striking out into new paths, and are being helped by men who are equally ignorant with themselves of the nature of their own organisation and of the possible dangers to their race of the success of their efforts.

(2) In the early history of mankind the women and men led lives more nearly alike and were consequently more alike physically and mentally, than they have become subsequently in the history of highly civilised peoples. This divergence of the sexes is a marked characteristic of progression among highly civilised races. Co-education of the sexes, occupations of certain kinds, and woman

suffrage may have a tendency to approximate the ideals, the lives, and the habits of women to those of men in these same highly civilised races.

(3) Such approximations in the future, while perfectly natural and not in a common sense degenerative, would not belong to the progressive stages of the evolution of mankind. Such changes would be convergences in structure and character, and, although they might lead to what we might now consider as intellectual advance, this would not in any way alter the facts that women would be tending to become virified * and men to become effeminised, and both would have, therefore, entered upon the retrogressive period of their evolution. The danger that men may become effeminised may be greater than would at first sight seem probable, but this might not take place at all or to such a slight extent as not to affect seriously the progressive evolution of the race. On the other hand, the danger to women cannot be exaggerated nor too carefully considered, in view of the fact that advanced women have adopted the standards of men, and have not tried as yet to originate feminine ideals to guide them in their new careers and thus maintain the progressive divergence of the sexes.

(4) There is a rise of the individual through progressive stages of development to the adult and a decline through old age to extinction. In the evolution of the stock to which the individual belongs there is a similar law, a rise through progressive stages of evolution to an acme and a decline through retrogressive stages to extinction. These cycles of the ontogeny (the life of the individual) and of the phylogeny (the evolution of the race or stock) can be illustrated by two diagrams of lines arising from a point, diverging to represent the progressive stages and converging to represent the retrogressive. The divergences and subsequent convergences are not simply physiological analogies, as heretofore supposed, but they occur in obvious relations of structures and forms which indicate that one law governs the development of the individual and the evolution of the stock to which that individual belongs.

The various characteristics of an organism develop through youth to the adult and end in the convergences of old age, which is termed the gerontic stage. Species, genera, or genetic stocks of any kind likewise progress from their origin and diverge to an acme, finally converging in the phylogerontic period (the gerontic period of the phylum). This last word is used because it conveys an accurate meaning for which there is no exact equivalent in the English

* This term enables one to consider the future woman who has acquired manly habits and character as tending to become mannish without being necessarily a degenerate being either physically or mentally. In point of fact she may be virified and yet be, according to the standards of advanced women of to-day, superior in both respects, so far as bodily and mental vigour are concerned to the women of the present time.

language, and please observe the use of the word "degeneration" is thus avoided.

The structural changes in the gerontic stage of the individual are repeated with sufficient accuracy in the adult, and often even in the younger stages of types that occur in the decline of the evolution of a phylum, so that one is forced to consider seriously whether they may not have been inherited from types that occur at the acme of the same group. The fact that these changes occur first in the individual during the gerontic stage does not necessarily imply that they first make their appearance after the reproductive period. No gerontic limit is known to the reproductive time in the lower animals, and it may well be that the continual recurrence of gerontic stages in individuals during the epacme of groups may lead to their finally becoming fixed tendencies of the stock or hereditary in the phylum, and thus established as one of the factors that occasion the retrogression or decline of groups. The decline may also be considered as occasioned by changes in the surroundings from favourable, as they must have been during the progression up to acmatic time, to unfavourable during the succeeding declining period in evolution. Still a third supposition is also possible, viz., that the type, like the individual, has only a limited store of vitality, and that both must progress and retrogress, complete a cycle and finally die out, in obedience to the same law.

All of these views can be well supported, but, whatever may be the true explanation, it is obvious that there are plenty of declining types, which, in their full-grown and even in their adolescent stages, correlate in characters and structures with the characters and structures that one first finds in the transient old age or gerontic stages of acmatic forms of the same type. These can, therefore, be truthfully and accurately described as phylogerontic or old in the phylum.

The position of man is at the extreme end of a series of converging lines in his own stock. This is also indicated by his structure and development which is phylogerontic, and it is therefore of the highest importance for him to avoid all movements tending to the increase of his natural and possibly inherent tendencies towards retrogression. The approximation of the sexes in habits of body or mind is therefore to be avoided, as possibly leading to convergence of the progressive characters non-existing between the sexes and the inauguration of retrogressive evolution.

It is hoped that no pretence of being able to solve problems requiring such vast knowledge and many-sided considerations will be attributed to this article, which has been intended simply to call attention to the scientific side of the question. It seems obvious that the time has come when thoughtful men and women should be warned, if this be possible, that their organisations are not of such a kind that they can rely upon continuous and certain progress. The laws of evolution point distinctly to a future in which retrogression and extinction is perhaps certain; but man's past history and the same laws also hold out hopes for the maintenance of progress through an indefinite time, if he is capable of controlling his own destiny through the right use of experience and of the wonderful control over nature that his capacities have enabled him to attain.

ALPHEUS HYATT.

BOSTON SOCIETY OF NATURAL HISTORY,
BOSTON, MASS., U.S.A.

II

Primeval Refuse Heaps at Hastings

(*Concluded from p.* 44)

THE most remarkable of all the flint implements found in the Hastings Kitchen Middens—or we may perhaps even say anywhere else—is a group of highly specialised diminutive forms totally unlike anything outside their own class. They are usually exceedingly small, very rarely exceeding an inch and a half in length, and sometimes are not one-sixth that size. They are characterised by the peculiarity of their shape and outline, and the method by which the flint has been worked. So persistent are the quaint and curious types, and so unique the working of the flint, that these delicate little implements have been recognised in Egypt, Arabia, Spain, the Valley of the Meuse, and in England by four different field workers. Around Sevenoaks I have found several settlements, and in one case a barrow of these people, in which, probably, the chief was cremated, with implements. Experiments lead me to the belief that this peculiar work, in which the delicate flakes average sometimes only one-thirtieth of an inch in length, was performed by a slot in a piece of bone similar to a saw-setter, as with a tool of this sort I can reproduce this work with its characteristic rectilinear outline. More recently I have noticed a few spheroidal flints, with the edges finely contused. I find that by using these upon a flake lying upon a banker I can lever off small flakes, giving rise to a kind of working very similar to that found on the Midden flints. Amongst these queer little forms are crescents, such as Nos. 8, 85, 10, 11, 12, 8, 4, and 81, which were probably employed for fishing, by a method of suspension that has come down to us, as shown in Plate VI.; also oblique (Nos. 4$\frac{5}{1}$ and $\frac{5}{2}$) and incurved pointed tools (Nos. 85, 87, 88), probably used for tattooing and other rites; others are drills; while the use of others, such as those with trapezoidal outline (No. 40, Plate VI.), is past conjecture. Many are simply sharp points, and were doubtless used for fish hooks, being bound upon a crutch twig, in the manner suggested in Plate VI. It is the extreme dissimilarity of these little things from everything else that makes us feel justified in recognising them as the work of one and the same people wherever they are found. They are not scattered indiscriminately all over the surface like the ordinary neoliths, but are confined to settlements, which are sometimes thirty acres in extent, but more frequently are not more than a hundred yards square.

THE WORKED BONES

One or two of the fragments of bone showed signs of carving. One was a well-made stiletto; another a portion of a needle. A third specimen was probably a potter's tool, as by its use the marks seen round the rim of the pots could have been readily made. The most interesting circumstance connected with the bones was, that in two cases the flint wedges were found *in situ* in the bones, as they were used for splitting them. One of these is shown in Plate VI., left lower figure. The whole of the marrow bones were thus split up for marrow, and the skulls for the brains; and even bones which contain no marrow were often similarly reduced, possibly for either boiling to extract grease or for use in making bone tools. Several other flint wedges similar to the above illustration were also found; and numerous bones showed deep cuts connected with the death of the animals, or those that were made in cutting up the trophy of the chase.

THE POTTERY

The pottery of the refuse heaps is of special interest, as it represents probably the oldest domestic utensils with which we are acquainted. Canon Greenwell has called attention to the fact that most of our Neolithic pottery is funereal or associated with burials: it is always of well-known special types, and none of these were found at Hastings. Some of the pottery here was made from a black, coarse, gritty, carbonaceous clay fairly well baked; some was better burnt and quite red. The majority of the vessels are of one of these kinds. There was a much coarser kind of a deep red colour, apparently composed of coarsely pounded flint, quartz, and clay-iron-stone; from this large utensils were made, and these were often ¾ of an inch thick. The vessels were for the most part of two types—the cauldron and the dish; they were all hand-made, none showing a sign of the use of the wheel. The cauldrons were very like the modern tar-kettle, with a flat bottom and no feet, the reflected rim-flange reaching out nearly as far as the widest part of the vessel. Several of these which I have restored give the following measurements:—Height, 9 cm.; widest part of rim, 16 cm.; widest part of body, 17 cm.; bottom slightly convex outwards, 13 cm. Another gives in the same directions 12, 20, 22, and 16 cm. respectively. A flat dish gave height, 3·5 cm.; width of rim, 23 cm.; width of flat bottom, 18 cm. Two fragments of rims showed decoration upon their upper edges. The first consisted of circular hollows, such as could have been made by the flat round-pointed bone tool, about 11 cm. long, found in the Middens; and the other was a crescent and line marking, such as could have been made by a plano-convex tool with a straight edge. Usually the upper edges of the rims were rounded; in the flat vessels they were not always reflected outwards; and now and then an attempt was made to thicken the top edge into a bead, although the thumb only allowed of very poor success.

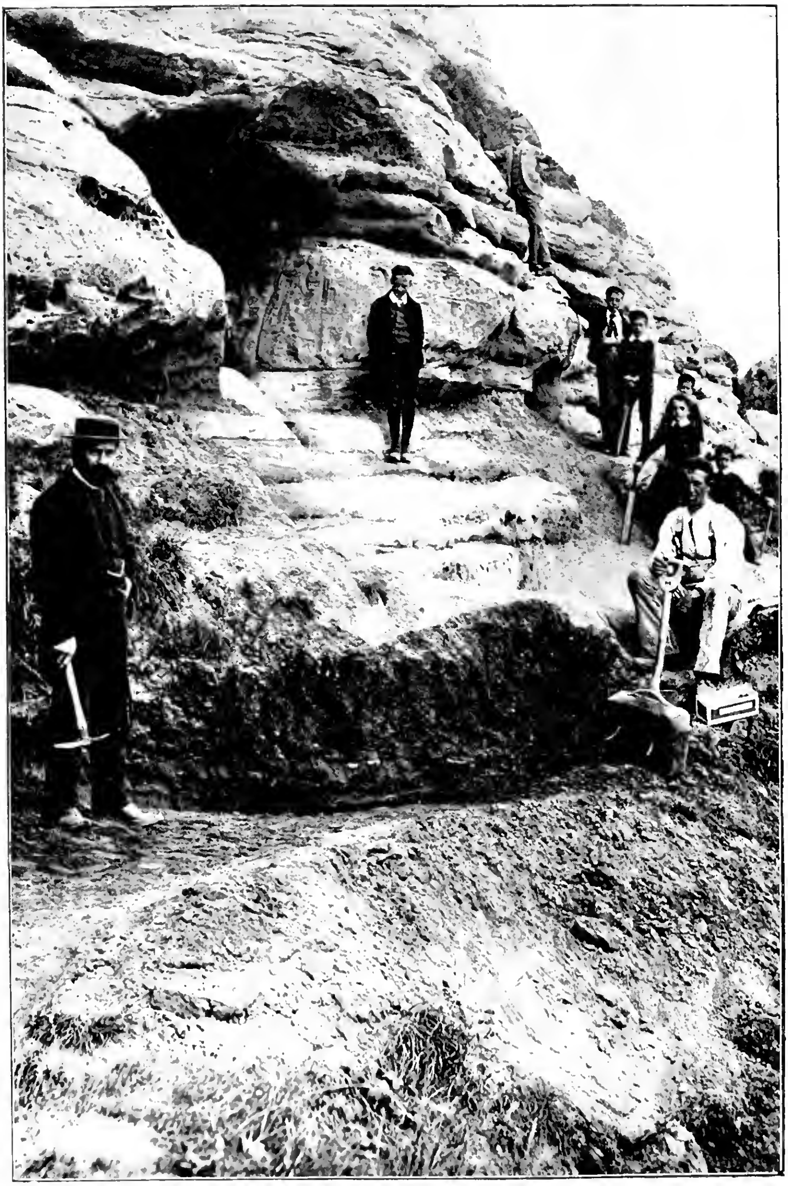
THE ANIMAL REMAINS

We next come to the animal remains found in the Middens, which, for many reasons, are extremely interesting. In practically all cases the marrow bones were split for the contained grease; they indicate an unaccountable number of young animals. Many of the bones are also minus their spongy ends or have lost their epiphyses, and some show the marks of teeth. The following are some extracts from my notes upon the species represented:—

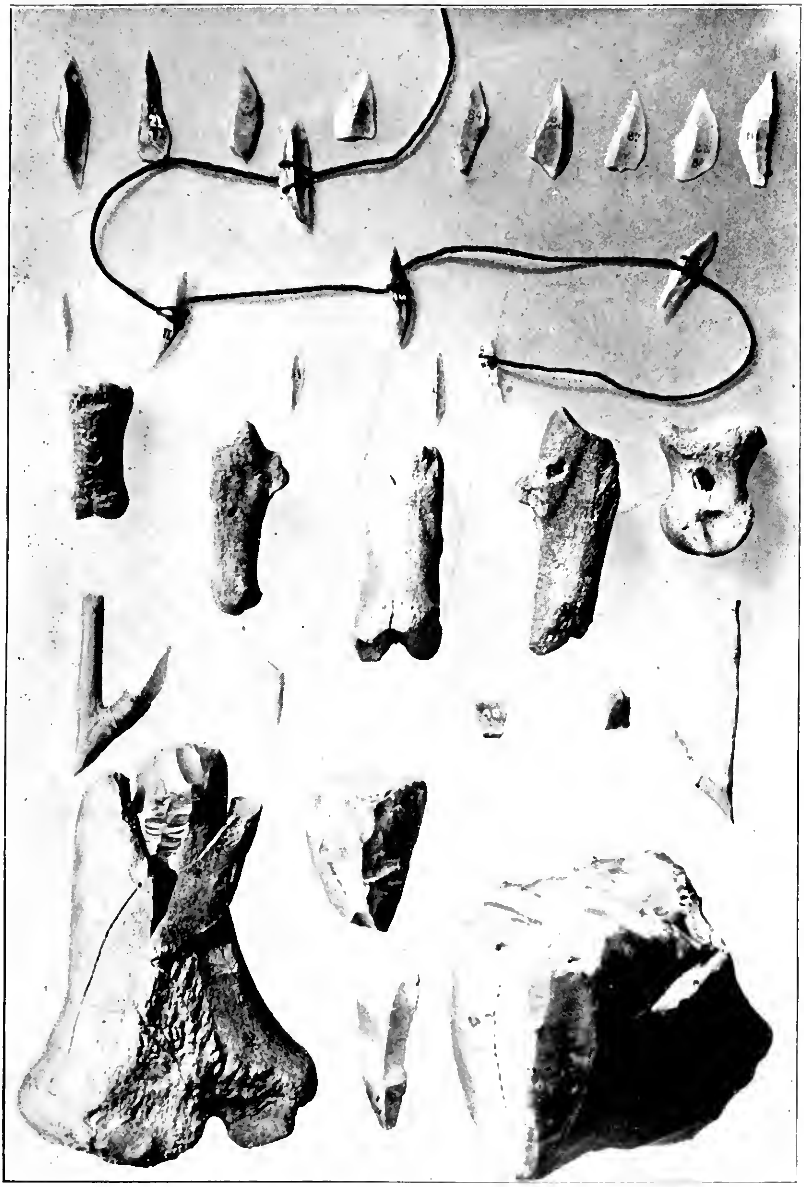
Bos longifrons (Celtic shorthorn).—Large limb bones were very rare, and those discovered were all split for the marrow. Most of the bones are young without epiphyses, and indicate small animals; two or three bones and fragments, however, are large and robust enough for *B. primigenius*. The toe bones are all small. The horns are small, short, and slightly curved, the longest measuring eight inches along the outside and seven inches girth; most of the horns must have measured about five inches along the outside curve. In eight cases the ends have been gnawed away. Vertebræ and all irregular bones were very numerous. Jaws were always broken into fragments, none carrying more than four teeth. Loose teeth were fairly numerous, some of which were very much worn.

Sus scrofa (pig).—Parts of all bones were present, and all marrow bones split. Nearly half have lost their articulating surfaces, and only part of them show teeth marks. One bone shows the marks of more spatulate teeth, which may have been human. All the bones are young, few with epiphyses anchylosed. Two or three of the latter were found not to be gnawed. One radius is exceptionally stout; otherwise the bones in general are quite small, many being not more than one-third grown. Pieces of head are numerous; some of the jaws appear rather heavy, and the longest canine measures five inches in length. Pigs' bones were often burnt.

Equus caballus (horse).—There were about twenty bones of horses, chiefly leg and toe bones, one piece of jaw and three odd teeth, all pointing to small or very small animals: they appear



CLIFF AT HASTINGS WITH KITCHEN MIDDEN ON LEDGE BELOW A CAVERN



FLINT IMPLEMENTS AND WORKED BONES FROM THE HASTINGS KITCHEN MIDDENS

to have been mere ponies. The limb bones are split for marrow, and two pieces are burnt, though none show traces of gnawing.

Ovis aries (sheep).—All the bones of the sheep were found, and, with two or three exceptions, they point to very small animals. They were exceedingly numerous, about as much so as either pig or ox. All the humeri are broken off short; all the radii are split, as are also some of the metacarpals and metatarsals. The bones of this genus are extremely perplexing, as they differ both in detail and proportions from any of the modern sheep with which I have been able to compare them: nearly all of them belong to a small long-legged variety, sometimes even approaching a small deer. One or two bones point to an animal just as much the other way, being altogether stouter than anything I am able to find.

Capra hircus (goat).—There are a few bones and a fragment of a skull with horn core, which Mr E. T. Newton has identified as goat.

Capreolus caprea (roebuck).—About the same number has been referred to roe deer.

Canis familiaris (dog).—Two tibiae represent a dog of very large proportions, almost stout enough for wolf; and some half dozen other bones point to one of full medium size.

Canis lupus (wolf).—One or two bones have been assigned to the wolf.

Felis catus (cat).—There are some half dozen bones belonging to this species which are of large size, and one tibia has lost its soft end, although it shows no teeth marks.

Canis vulpes (fox).—Several bones unquestionably belong to the fox.

Lepus timidus and *L. cuniculus* (hare and rabbit).—Both of these species are represented by few bones. The former was not very large.

Meles taxus (badger).—This species is represented by an ulna; it agrees well with a fine specimen I have from the Thames Pleistocene brick earth, although perhaps hardly so robust.

Birds.—When we come to the birds, I regret to say that our public museums are still so deficient in skeletons for comparison that the determinations are not all quite so satisfactory as could be wished. A larger series for comparison may add several species to the list or perhaps otherwise alter it. Of course there is always the difficulty of being able to say how much variation from the present examples one ought to allow, and this can only be decided more correctly when we have much more modern material in our museums. Mr Newton has identified the following:—Black and red grouse, a species of duck, guillemot and carrion crow.

Fishes.—These include cod in very large quantities, gurnard, mackerel, whiting, *Cottus*, turbot, plaice, and thornback (*Raja*).

The Batrachia are represented by the frog or toad.

Mollusca.—These offer several points of interest. The first to claim attention is a number of very large *Helix aspersa*, which are represented both by whole and broken shells. These were found under about three feet of midden material, and I have no doubt they were eaten by man. *Littorina littorea*, in point of numbers, stands first, and the shells are also of very large size. The limpet (*Patella vulgata*) occurs next in quantity, and is represented by two or three varieties. *Cardium echinatum* is fairly represented, but *C. edule* was not met with. The whelk (*Buccinum undatum*) is plentiful, but *Trophon* was always absent. *Purpura lapillus* is represented by a score or more shells, but whether eaten or employed for their purple juice it would be difficult to say. One piece of bone recovered was of a purple colour, suggestive of having been dyed, although it is highly improbable that the stain would have lasted so long. Most of the oysters (*Ostrea edulis*) are of medium size, suggestive of shallow water; but here and there some were very large, and may have spent the greater part of their time in deep waters. The mussel (*Mytilus edulis*) often occurred in large masses, although the shells were always badly preserved. *Natica*, *Pholas* and *Mactra* are represented by a single specimen each.

CONCLUSION

With this amount of material we are able to restore a fairly satisfactory picture of the men who left the Refuse Heaps at the mouth of the Hastings caves. It is certain that they lived largely upon fish, both those which could be obtained between tide marks and those which would require the use of boats of some sort and the employment of the hook and line; although the spear in the hands of dexterous people, perched upon the rock masses which strew this coast, might have been the favourite instrument. We have no evidence that they knew anything about the use of metal; but they were exceptionally skilful fabricators of flints, which they worked in a manner essentially their own, of such diminutive sizes as to suggest that the delicate little instruments were largely employed for fish hooks. The absence of battle axes and all other heavy tools would point to their not being much employed in wars or agricultural pursuits. No cereals of any kind were found, and no querns or large mealers; although leaves and twigs were often preserved. They knew the use of fire, which they lit upon hearths, and kept them going until they had baked the underlying ground for seven or eight inches. They also employed cooking stones, or "pot boilers" as they are sometimes called. Their pottery, although coarse, was fairly well modelled considering there is no sign of the knowledge of the wheel. It was also fairly well baked, and would stand the fire, as is shown by the deposit of soot upon some of the fragments. They appear to have known nothing about the art of polishing flint or the barbing of arrow heads. In none of the settlements where the characteristic implements have been found, has anything been obtained to conflict with the evidence of the Middens in any way. The barrow at Sevenoaks, which contained similar relics, it is true, was nearly round, pointing to the close of the Neolithic period; but further research induces me to consider that these people might nevertheless have preceded the days of the barbed arrow and the polished axe, and this conclusion is strengthened by the geological evidence of Dr Colley March in Lancashire. It is probable that the ox, the 'sheep,' and the pig were confined in enclosures, where they lived in a semi-domesticated state. Man also seems to have domesticated the dog, which possibly assisted in keeping the cattle, although the canine bones sometimes look as if they had been gnawed. It is certain from the large quantities of bones present that animal food was indulged in whenever obtainable, perhaps even more so than was the case with the Baltic Midden men. But the motley group of animals represented at Hastings show the men there to have been anything but epicures, as they appear to have eaten anything upon which they could lay their hands.

The occurrence of these implements thirty or forty miles inland in a number of places, suggests their being the work of a Nomadic people, but whether or not the trans-European localities can be taken to indicate the line of original migration, further researches will alone decide. This much is however proved, that the Midden men will henceforth have to be added to the pre-historic races of Britain.

W. J. Lewis Abbott.

III

Bees and the Development of Flowers

IN the "Origin of Species," Darwin expressed the view that we owe the gay colours and varied forms of our flowers to the selective action of insects. "We may safely conclude," he writes, "that if insects had never existed on the face of the earth, the vegetation would not have been decked with beautiful flowers, but would have produced only such poor flowers as are now borne by our firs, oaks, nut, and ash trees, by the grasses, by spinach, docks, and nettles."

This theory of the selective action of insects on flowers has been elaborated in great detail by Hermann Müller in Germany, and by Sir John Lubbock and Mr Grant Allen in this country; and almost every writer on Natural Selection has accepted this view as a part of the Darwinian scheme of evolution. In this case, as in many others, more recent observers, assuming that the foundations are secure, have spent their time in elaborating structural details in the hypothetical edifice. But when we lay the ingenious conception along the straight rule of facts in nature, the measures do not correspond. In other words, the foundation, the habits of insects with regard to flowers, does not support the hypothetical superstructure.

Professor Henslow, again, has proposed an amendment, in which, although selection by insects is still the motive power, the *modus operandi* is different.

Stated briefly the Darwinian theory is as follows:—

Insects come to flowers for honey and pollen, and in so doing do not visit all indiscriminately, but select those which take their fancy, or suit them best. If they are seeking honey they will choose those flowers which afford the most, or in which it is most easily obtained; if they have a special liking for any colour, say blue, they will pick out the bluest flowers; if any special shape of flower affords them greater convenience for alighting, they will visit these rather than others. Now in any species of flower all these things—amount and position of honey, colour, and shape—vary in different individuals. If, then, insects possessed the requisite discrimination, we might suppose them selecting, generation after generation, those flowers in which these desirable points were most highly developed. The flowers thus visited would obtain the benefits of cross-fertilisation, and their descendants would therefore be more numerous and vigorous than of those not visited. This more vigorous progeny of the insect-visited and cross-fertilised flowers would gradually live down the less vigorous offspring of the unvisited flowers. Hence any particular character perseveringly selected by insects for many generations would come to predominate, be gradually perfected, and finally form a new specific type.

Such is a brief outline of the theory which has captivated the minds, not only of men of science, but also of the public, and which has been accepted by almost every evolutionist as the explanation of the form and colour of flowers. Professor Henslow's amendment, while still depending on selection and the benefits of cross-fertilisation, attributes the alteration in the shape and colour of the flower to the direct stimulus of the insect's action. Thus an insect hanging to the lower petal of a flower elongates the same by its weight, and the lengthened petal is transmitted by heredity; the irritation caused by its feet in walking along the flower causes the appearance of colouring matter, and the colour is likewise transmitted; as it probes for honey it causes a flow of sweet sap to that part, and this also becomes hereditary. This view is quite too Lamarckian for England or Germany, whatever may be its fate in America.

The weight of authority supporting the insect selection theory, and its wide acceptance in scientific circles, renders it perhaps a little rash to criticise it adversely; yet a series of observations on the habits of bees with regard to flowers extending over many years, has forced me to the conclusion that it has no sufficient foundation in fact. Details of some of these observations have been given elsewhere, and I shall here state only the general results. Bees have been chosen because, of all insects, these are the easiest and most interesting to watch in their visits to flowers. They are also the most important of insects for the theory, since they are by far the most frequent and regular visitors of flowers. If the bee's action can be shown to be insufficient, no one will support the claims of any other insect. Let us, then, see what the bee really does in the way of selecting special forms and colours of flowers by watching it in the fields and gardens, and consider what effect this can have on the flowers visited.

It has been frequently pointed out that Sir John Lubbock's experiments prove that bees have special tastes for certain colours, and that they prefer red and blue, especially the latter. This preference of the bee for blue is one of the cardinal points of the theory, and has been considered almost sufficient in itself to justify the assumption that blue flowers have been evolved by their selective action. But without wishing in the least to disparage the work of one of our great authorities on insects, I submit that observations of what bees actually do in the way of visiting flowers is of more importance to the theory than any such experiments; and, as we shall see presently, bees in visiting flowers do not show any marked preference for blue ones.

The analogy between man's selection resulting in artificial breeds, and natural selection resulting in species, was often and strongly insisted on by Darwin; while the action of the insect in evolving a new species of flower specially adapted to its requirements in form and colour, has been compared to that of the gardener in the production by selection of his special varieties. The extreme care required from the gardener, not only in the selection of the requisite variation, but also in its isolation and preservation is well known. If the new variety in the process of its evolution be allowed to cross with the parent stock it is lost. Darwin even went so far as to warn gardeners against allowing crossing between different individuals of the new variety, as such tends to produce reversion. It is a little difficult to understand how the bee, even if it possessed all that nice discrimination of form and colour, and all that constancy required by the theory, could accomplish that which demands such care and patience on the part of the gardener. In order to evolve a race of blue flowers from those normally of another colour, bees would have to select those among the slight natural variations which had a tinge of blue. If they really preferred to gather honey from blue flowers—they would not have merely to prefer blue in the abstract—they would do so, and these bluish flowers would receive the benefits of cross-fertilisation. But the few bluish flowers among the many normal ones would not suffice: the bees, from the necessity of obtaining honey, would be obliged to visit the normally-coloured ones also. Thus the new variety would be blended with the parent type and lost, for pollen would be carried from the one to the other. In order to isolate the bluish variety, the bee would not only have to prefer blue, but also steadily refuse to visit any other colour; and if it did so when the bluish flowers were first appearing it would starve for want of honey. But we have no proof that the bee even possesses the taste. In all my observations of bees I have met with nothing to support the view that they prefer to take their honey out of blue flowers. Some blue flowers they visit frequently, others they visit very seldom. No blue flowers are more frequently visited than others which are yellowish-green, pink, and various other shades. Some uncompromisingly green flowers—as the plane tree and red currant—are frequently visited. As regards colour, then, the bee seems to have neither the taste to select, nor the ability—through the necessity of obtaining sufficient honey—to restrict itself in the manner required. Moreover, the fact that the same species of bee may be seen visiting flowers of the most diverse shapes, from the simplest to the most complex, does not seem to imply that they have the requisite selective tendency as regards form. This is further illustrated by the fact that bees may at times be seen visiting flowers which have lost their corollas wholly or in part. Thus I have seen them visit petal-less flowers of wild geranium, bramble, and cistus. Darwin relates his observation of the same fact. More recently Professor Plateau of Ghent has removed the corollas of certain flowers, and found that this proceeding made little difference to the insects visiting them. Bees may also be seen to visit abnormally developed, as well as half-faded flowers. In many cases, again, bees instead of using the form of the flower supposed to be specially fitted for their convenience, and the outcome of taste, will bite a hole near the base of the corolla, and get the honey through it. These holes may frequently be seen in heath and the bush vetch.

Again, if our native flowers are the result of the selective action of our native bees, and those which they have specially chosen for countless generations, how is it that bees take so readily to many flowers of very different forms introduced into our gardens from abroad? For such introduced plants are in many cases freely visited by native bees.

In order to evolve and keep distinct new species bees would have to be extremely constant in their visits to flowers: in a single journey from the nest, or until they got rid of all the pollen adhering to their bodies, they would have to visit only a single variety. If they did not do so they would not merely be unable to develop and differentiate new varieties; they would even retard by intercrossing, varieties developing into species by any other means. It is pretty generally believed that the bee is very constant in its visits to flowers, and that when it begins with any particular species it keeps to that until it has obtained its load. So long ago as the time of Aristotle, indeed, the constancy of the bee was noted as a fact in natural history. But while it is true that bees do show a considerable amount of constancy and often visit a large number of flowers of the same species in succession, they are far from possessing that amount of constancy required by the theory. For this they would require to restrict themselves, not merely to a single species, but to one variety of that species. This is obvious, since all species are supposed to have begun as varieties; and it is even more important that they should restrict themselves to one variety than to one species, since such varieties will be more readily crossed by transference of pollen. But it is a well-established fact that bees pass freely from variety to variety of the same species in our gardens. Darwin has observed this, and it is one of the most firmly established results of my own observations. They do not even confine themselves in a single journey to varieties of the same species. In numerous cases I have seen bees visit two, three, and even four species in the course of a minute or two. The general results of my observations on this point are as follows:—

Hive bees are much more constant than wild bees, yet they pass freely from variety to variety, and not by any means rarely from species to species. As to the latter, take any wild bee, and if you can follow its movements for twenty visits or more, the chances are something like ten to one that it will be seen to change its species of flower. If we suppose that the bee of the past acted as the bee of to-day, then it seems to me that in this habit alone we have a complete refutation of the theory.

Another of the foundations of the theory is the benefit supposed to result from the cross-fertilisation effected by the bee in flying from flower to flower. Darwin's well-known experiments on cross-fertilisation point to the conclusion that the seedlings of cross-fertilised plants are more numerous and vigorous than those of the self-fertilised. Without wishing to throw doubt on the general deductions from these experiments, I may be permitted to point out that certain facts regarding fertilisation in nature render them of doubtful support to the theory. First, there is the fact that certain species of flowers which are habitually self-fertilised are among the most numerous and vigorous of our native plants. Such, for example, are *Polygonum aviculare*, the least visited by insects, and yet the most abundant of its genus: *Veronica hederaefolia*, one of the commonest of the veronicas, yet very seldom visited by insects, as H. Müller points out: while among the geraniums, *G. molle* and *G. pusillum*, which Müller states to be the most frequently self-fertilised, and perhaps the most common of their genus with the exception of *G. robertianum*. Professor Henslow, indeed, has gone so far as to state that "in nature whenever self-fertilisation can be effected more seed is borne than by the forms requiring crossing." Among the orchids again, some species exhibit the most complicated arrangements for avoiding self- and securing cross-fertilisation; others exhibit equal complications for securing the former and avoiding the latter. And if the inference is that the contrivances in the former case were evolved because cross-fertilisation was an advantage, then it follows equally that in the latter case they were evolved because self-fertilisation was an advantage. Darwin, in accordance with his general views on cross-fertilisation, believed that such self-fertilised orchids were dying out, but the increased number of such now known seems to forbid this view, and it is difficult to understand how such self-fertilised orchids can have been evolved from a race specially fitted for cross-fertilisation on the supposition that this latter method is always beneficial.

Those flowers of the original race which were cross-fertilised should have survived rather than those self-fertilised. The passage from perfect adaptation for cross-fertilisation to perfect adaptation to self-fertilisation is a long one, and must have been spread over many generations in each of which the latter was an advantage, if it is to be accounted for on the principles of natural selection. But it has been suggested that the change has been due to the absence of insects and that thus only those plants which were able to fertilise themselves survived. If, however, we suppose that in the ancestral orchid the apparatus for cross-fertilisation was as perfect as in many species at the present day, it would be incapable of self-fertilisation, and therefore die out in the absence of insect visits. Even if it could in a few cases fertilise itself, how could its fewer and weaker progeny compete with the stronger seedlings of nearly related and cross-fertilised species probably occupying the same station? If, however, in the ancestral orchid the arrangement for fertilisation was such that self-fertilisation usually took place in default of insect visits, then no benefit would arise from change of form to perfect adaptation for self- and avoidance of insect-fertilisation. With regard to the benefits resulting from cross-fertilisation generally, Professor Henslow points out that orchids, the most remarkably adapted of all plants for cross-fertilisation by insects, "set the least amount of seed even when fully exposed to insects."

Another fact established by Darwin in relation to cross-fertilisation is that the offspring of the cross is more vigorous when between slight varieties of the same species, or between individuals grown under slightly different conditions. This fact is also adverse to the theory of the development of a species of flower by the selective action of the bee. For among the offspring of the crosses affected by it those will be strongest which occur between varieties, or between plants grown at a distance, and therefore likely to differ slightly from each other. But these are precisely the individuals in which the incipient characters tending to the formation of a new species will be least marked. Hence the action of the bee is rather to retard development; and Darwin himself has remarked that frequent in-crossing tends to give uniformity to species varying slightly as they do in a state of nature.

A brief allusion to Professor Henslow's amendment of the Darwinian insect selection theory will suffice. Apart from the extreme improbability—as shown by recent research—that such acquired characters as the lengthening of the petal of a flower by the weight of an insect stretching it, or the coloration caused by the irritation of an insect's feet, are transmitted, Professor Henslow's theory splits on the same rock as the older one. For, like the other, it requires a discriminating taste and constancy on the part of the bee. It involves, moreover, the assumption, that while certain simple and regular flowers visited by insects have remained simple, others originally equally regular and simple have had impressed on them all sorts of irregular and complex forms by the same insects visiting them in the same way and for the same purpose. If the direct action of the insect in visiting one simple and regular flower is to elongate one petal and form a hood of another, how has it been possible for it to visit a host of others for countless generations without producing any such effect, or altering the simple regularity of their form? It is not probable, then, that Professor Henslow's amendment will be adopted, at least in these days of scientific doubt as to the transmission of acquired characters.

Other insects, it is generally admitted, are even less discriminating, and more erratic, in their visits to flowers than bees. Hence, if bees cannot be accepted as evolvers of new species of flowers by their selective action, the whole theory of insect selection fails. It remains a fact that no alternative explanation of the origin of the colour, scent, and form of flowers on Darwinian principles has yet been brought forward. In this fact, indeed, we have the only —if insufficient—reason why the theory has been so long retained.

G. W. Bulman.

REFERENCES

1. **Darwin, C.**—"Origin of Species." London, 1859.
2. **Lubbock, Sir John.**—"British Wild Flowers in relation to Insects." London, 1875.
3. **Darwin, C.**—"Cross- and Self-fertilisation of Plants." London, 1876.
4. **Bennett, A. W.**—"Constancy of Insects in their visits to Flowers." *Journ. Linn. Soc. Zool.*, vol. xvii., p. 175. 1883.
5. **Christy, R. M.**—"On Methodic Habits of Insects when visiting Flowers." *Ibid.*, p. 186.
6. **Allen, Grant.**—"The Colours of Flowers." London, 1882.
7. **Müller, Hermann.**—"The Fertilisation of Flowers" [English translation]. London, 1883.
8. **Henslow, Rev. George.**—"Floral Structures." London, 1888.
9. **Bulman, G. W.**—"On the Supposed Selective Action of Bees on Flowers." *Zoologist* [3], vol. xiv., p. 422. 1890.
10. ————.—"The Constancy of the Bee." *Science Gossip*, 1892.

IV

Polymorphism in the Algae

THE following is a translation of an extract (pp. 171-186) from Professor Klebs' recent work "Die Bedingungen der Fortpflanzung bei einigen Algen und Pilzen," Jena, 1896, reviewed in *Natural Science*, vol. x., p. 128 (Feb. 1897).

The pages in question are written *à propos* of Prof. Klebs' interesting discovery that the well-known alga, *Botrydium granulatum*, is not in reality so polymorphic as the botanical world has believed since the publication in 1877 of Rostafinski and Woronin's researches, but that the forms therein described belong, in reality, to two very distinct algae, *Botrydium granulatum*, Wallroth, and *Protococcus botryoides*, Kützing, the latter being renamed *Protosiphon botryoides* (Kützing) Klebs.

But my immediate object is not so much to call attention to this or any other of the interesting discoveries of which an account is given in Prof. Klebs' important book, as to put into a form easily accessible to everyone interested a most lucid and admirable discussion on a question which, to judge from my own experience, must continually perplex students of algal literature. No one who knows his work can doubt Prof. Klebs' authority to speak on the topic, and it is unfortunate that his luminous remarks, which are perfectly complete in themselves, should remain buried in the middle of a chapter of his bulky volume, where they can only be read by a few specialists. Prof. Klebs and his publisher, Dr Fischer of Jena, have most kindly accorded me permission to publish this translation. A. G. TANSLEY.

Although a number of the lower green algae were described in the first half of this century, it was by Kützing and Nägeli that the foundation of our present knowledge of these forms was laid. Kützing described an enormous number of species which he distributed partly into new genera, partly into those which Nägeli had with admirable judgment already established. Side by side with the tendency to a thorough-going splitting of species, we find in Kützing the belief that lower algae transform themselves into higher forms, even into moss-protonema. Hitherto these polymorphistic ideas of Kützing's have not succeeded in establishing themselves, since they are obviously based on too cursory investigations. The description of new species and genera of lower algae went quietly forward till Cienkowski published in 1876 his observations on the palmella-condition of the filamentous algae, *Stigeoclonium*, *Ulothrix*, etc. These very reliable observations seemed to prove that lower algae are only developmental stages of the filamentous forms. Cienkowski himself, and other workers also, went so far as to express doubt as to the independence of most unicellular green algae. In order to decide the question, I established from 1879 to 1881 many cultures of these forms. My observations, from which I published a short extract in 1883, showed that the lower algae, treated by the methods then accessible to me, were as much independent organisms and remained as constant as any higher plant. The common, universally distributed organism, *Pleurococcus vulgaris*, for instance, reproduced itself only by vegetative division, and it could not by any method be converted into another alga. Artari (1892) again investigated this question, and answered it in the same sense. Gay also (1891) has proved by means of cultures the independence both of *Pleurococcus* and of other forms. Nevertheless, the old idea of a far-reaching polymorphism among the algae is constantly reappearing.

Since Hansgirg in 1885 came to the conclusion that all possible algae are forms of a single species, and thus showed himself as uncritical as Kützing before him (*cf.* my criticism, *Botanische Zeitung*, 1886), Chodat, and especially Borzi, have quite recently announced the genetic connection of many algae which were hitherto considered independent. Both workers have tried to give greater weight to their opinions by means of evidence obtained from culture experiments. To what an extent Borzi is dominated by the idea of polymorphism is shown in his most recent work (1895) by the treatment of the life cycle of "Protoderma viride," to which he assigns species of the following genera: *Protococcus*, *Botryococcus*, *Chlorococcus*, *Limnodictyon*, *Physodictyon*, *Palmella*, *Tetraspora*, *Nephrocytium*, *Trochiscia*, *Oocystis*, *Scenedesmus*, *Raphidium*. The connection of all these forms Borzi thinks he has proved by means of cultures. According to the view of this worker, the various developmental states have "stadii anamorfici"; thus, for instance, the *Raphidium*- and *Scenedesmus*-forms have the power under certain circumstances of remaining and multiplying as such for years, and then when opportunity offers they again turn into the higher forms.

Chodat does not go nearly so far as Borzi in his belief in polymorphism, and he expresses himself less clearly and distinctly. He contents himself with believing (1893-4-5) that such an alga as for instance *Raphidium*, is very variable, so that it may take on the form of a *Scenedesmus*, a *Protococcus*, a *Characium*, a *Dactylococcus*, or a *Sciadium*. The *Scenedesmus* behaves under certain circumstances as a *Dactylococcus*, and the latter as a *Scenedesmus*. But in spite of all his belief in the existence of transitions between the various forms of lower algae, in spite of the fluctuation of all their characters and of the melting away of all limits, Chodat still holds to a certain independence in the various types; only it does not appear from anything he says whether the deviations from these types are due to the definite effects of external conditions or whether they belong to constant varieties or races. Neither has Chodat always drawn accurate inferences. If his observation is correct, that the already-mentioned *Pleurococcus vulgaris* can grow out into a *Stigeoclonium*, it must be assigned to the branched filamentous type of algae. But Chodat says, "I do not think that *Pleurococcus* is a state of a higher alga. I think that it must be considered as a type degenerated through the influence of its habitat, and which in water and under favourable conditions can tend towards its primitive state." Thus *Pleurococcus* is to be considered a degeneration-form of a *Stigeoclonium*, although it behaves as an independent simple alga. If one takes the observations of Chodat and Borzi together, so far as they relate to the same form, for instance *Raphidium polymorphum* (or *braunii*), one obtains a picture of a species which for excentricity certainly leaves nothing to be desired, and which is unique in the organic world. *Raphidium*, according to Borzi, belongs to the cycle of forms of various higher algae (*Prasiola*, *Ulothrix*, etc.); it is produced in this way—two zoospores, in consequence of their escape from the sporangium taking place abnormally, grow together by their hinder ends, and this pair then turns into a *Raphidium* (Borzi, 1895, p. 231. "My cultures exclude all doubt as to this fact"). If it had not been such a well-known and eminent algologist as Borzi who put forward these opinions they would not have been worth the trouble of wasting a word upon, but under the circumstances they must be taken into account. In any case it is clear from what has been said, that unlimited confusion prevails in the classification of the lower algae, and that the most contradictory views are held even about the simplest and commonest organisms such as *Pleurococcus*, *Raphidium*, etc.

We cannot decide on theoretical grounds whether a form is independent or genetically connected with another. It has in fact happened in the history of the subjects that forms which at one time were considered independent have been certainly proved to be developmental states of other species. It is conceivable that out of the enormous number of described species of algae many others will meet with the same fate. The only possible way to arrive at clearness and knowledge is to use a scientific method which will stand the test of criticism.

It is remarkable that so little has been learnt from the history of science. In the history of botany the same struggle about polymorphism has taken place twice, first in the case of the fungi and then in that of the bacteria. On both occasions as it gradually became recognised that the pure culture of the organisms in question is the necessary starting point of every research, the fruitless theoretical discussions were replaced by work on really scientific lines. Likewise in the present case; for the solution of the problems with which they were concerned Chodat and Borzi ought to have started with pure cultures; but this is just what they failed to do. The method of pure cultivation has hitherto played no part in algology; our knowledge has been obtained by the method of direct observation, which has been employed in a most thorough way by such investigators as Thuret, Cohn, Pringsheim, De Bary, and others. The fact is that the distinctive characters of many algae are quite sufficient to allow of their recognition among other organisms, and to enable their development to be followed by continuous direct observation; and the artificial cultivation of many of these algae is beset with great difficulties. There are also many of the lower algae among which no confusion arises in impure cultures. Forms such as *Hydrodictyon*, for instance, allow of their entire development being observed in the presence of other larger or smaller forms. But the matter is quite otherwise in all those algae which possess but few external readily recognisable characters, and among which at the same time there is such a multiplicity of closely allied species that all possible so-called transitional forms exist. This is the case, for instance, in the subaerial *Ulothrix*-like organisms, and also among the lower green algae such as the numerous species of *Pleurococcus*, *Protococcus*, *Palmella*, etc., which are extremely easy to confound with one another. Furthermore, the higher algae possess developmental stages which to outward appearance are exactly like certain lower forms; and the false conclusion that therefore all lower algae are developmental forms of higher algae is often enough drawn. Swarming gametes of *Chlamydomonas* cannot be distinguished from gametes of *Ulothrix;* but it does not follow that *Chlamydomonas* belongs to *Ulothrix*. Similarly filamentous algae form *Protococcus*-like stages; but again it does not follow that all species of *Protococcus* belong to filamentous algae. Simple similarity in appearance or the apparent identity of two forms decides nothing, and anyone who relies upon it runs the risk of falling into the grossest error. Even two such distinguished investigators as Rostafinski and Woronin were baffled in their observations on *Botrydium* simply because the developmental stages of the real *Botrydium* and of *Protosiphon* have a striking external resemblance.

The question now presents itself, how must we proceed in order to solve the problem of the independence or genetic connexion of two or more algae with the greatest possible certainty? In my view such an investigation demands attention to three important points:—

(1) The pure cultivation of the organisms in question

Just as a pure culture is obviously essential for any research into the history of development of a fungus or of a bacterium, a similar culture is the necessary starting point in the investigation of the life-history of a lower alga. In general, the algae grow much more slowly than the fungi and bacteria; cultures, therefore, have to be maintained for a longer period, and the chance of the accidental introduction of foreign forms into an originally pure culture must not be neglected. Minute cells or spores of Protococcoideae, etc., are present in the dust of the air. It is only necessary to leave a sterilised solution of nutritive salts, not very well protected, in the light, in order to convince oneself that algae get into it with the dust. It is clear from the works of Chodat and Borzi, that these workers had only impure cultures at their disposal, since, on the one hand, they used material taken direct from its natural habitat, and containing numerous species of algae, and on the other, they paid no attention to the sources of error arising from the exposure of their cultures to dust.

(2) Direct observation

In the case of the lower algae it is always necessary to observe directly under the microscope, the course of development or the transformation of one form of cell into another. In default of a pure culture, this method may, under certain circumstances, do instead; but it should be used in any case, even if the culture is to all appearance pure. A combination of the two methods leads to very certain results. Chodat and Borzi have employed them far too little. For instance, Borzi ought to have isolated the double zoospores of *Prasiola*, etc., and then uninterruptedly observed their subsequent fate, in order to convince himself that they actually turned into *Raphidium*. And similarly it would be necessary to observe directly the development of the filamentous alga, whether *Prasiola* or *Protoderma*, from *Raphidium*, a thing which it appears Borzi never really saw. The same criticism holds in regard to Chodat's statement that *Pleurococcus vulgaris* changes into *Stigeoclonium*. The immediate transformation of an undoubted *Pleurococcus* cell into a *Stigeoclonium* has not been seen, any more than a transformation of the latter into *Pleurococcus*.

(3) An accurate knowledge of the conditions under which the individual developmental stages occur, or the transformation of one form into another obtains

This point has received as yet scarcely any attention in algae or other organisms, *e.g.*, fungi, and such knowledge has never been recognised as necessary, since it has not hitherto been believed that it was possible to determine these conditions. In works which deal with polymorphism, from Kützing to Borzi, great significance has, it is true, been attributed to external conditions in the transformation of forms, but only in quite a general, undefined and vague way. Never in any case has a given developmental form been clearly recognised and demonstrated as the necessary consequence of definite external conditions; such forms have mostly been observed merely by chance. In his work on *Eremosphaera*, Chodat describes, besides the well-known typical cells, certain dwarf forms with somewhat different structure, he describes *Palmella-Gloeocystis*-stages, he also brings a *Chlamydomonas*-form into connexion with these; all of which are developmental forms or (it may be) independent species found accidentally in the same culture of *Eremosphaera*. We never get a hint of an explanation how such various states of the same alga can appear in the same culture.

But since my observations have shown that external conditions actually decide the appearance of the reproductive stages of many algae, it has become necessary in all similar work to attempt at least to discover the appropriate conditions of the appearance of each developmental form.

The next goal to be attained is such an exact knowledge of the conditions that we can elicit a given developmental form at will. Such investigations as these naturally demand much time and trouble; and even so in the case of many organisms they do not lead to the desired result. Thus, in certain species, in spite of the firm conviction that external conditions must be of great significance, these conditions are not yet sufficiently clearly understood, as for instance in the case of *Ulothrix zonata*, *Hormidium nitens*, etc. Thus there remains a gap in our knowledge, which, later on, with the aid of better methods, will be filled up. On the other hand, there is the alternative that a given developmental form is produced as a result of the operation of inner causes which we are not able to elucidate; in that case we shall find by experience that it will appear quite regularly and can always be observed at the appropriate stage in the life-history of the species. But in all the lower algae—and these are the forms I am specially considering here—my whole experience leads necessarily to the conviction that external conditions determine the appearance of each developmental form; and hence in all work on such algae this line of investigation demands the greatest attention.

If in the future the lower algae are investigated on the lines I have indicated, it will be possible for us to emerge from the confusion prevailing at present, and to bring the study of these organisms on to a higher plane. Such investigations will, according to my view, be of very great value in advancing the systematic knowledge of species, not only because they will enable the cycle of forms belonging to a given species to be completely determined, but also because in the diagnosis of the species they will enable new characters to be recognised. The way in which the various stages in the life-history of a lower organism react to external conditions, especially the way in which its reproduction depends upon the external world, furnish specific characters as important as the morphological ones. And these physiological characters become so much the more valuable in proportion as the external characters become less conspicuous. In the bacteria we have already been compelled to take such characters into account; and the time is not far distant when it will be self-evident that in the diagnosis of a new alga, there must be placed alongside of the accurate description of its structure and the history of its development, a clear account of its behaviour in relation to the external world. To-day the mere determination and the giving of a name to a species is far too generally the sole aim in systematic botany, and it is here, among these lower organisms, that the proper goal of the systematic knowledge of plants may be soonest reached—to present a complete picture of all the peculiarities of each several organism.

The whole of my more recent experiments with algae confirm my earlier experience, and correspond with the results of the investigation of bacteria and fungi—they show, namely, that within the time available for experiment, the hereditary characters of an organism are not markedly altered by external conditions. The variations in size, form, cell-structure, and reaction to external influences, oscillate within definite limits—limits which up to the present we have not been able to pass. The constancy of the species meets us with striking clearness in all cultivations and experiments under existing conditions; it remains for further experiments, carried on for longer periods, and with the aid of better methods, to decide whether these limits cannot be broken through. The important observations on certain bacteria, in which it was found that hereditary characters such as virulence and pigment-production, could be suppressed for a long time, point in this direction. But anything like such a result has not hitherto been obtained among the algae, although it is possible that it may be obtained in the future.

In spite of the actual constancy of specific character among the algae, the difficulty of arranging the species into genera and families is extraordinarily great, since there has been an enormous multiplicity of species production. There exist, between the species typical of the various genera, numerous other forms, varieties, etc., each of which is a perfectly constant type, but which confuse the limits between genera and families. Very soon the dictum which I laid down in connexion with my systematic working up of the Flagellata in 1892, will apply equally well to the algae, namely, that the more we take into consideration the multitude of forms, the more difficult to construct and the more artificial our system becomes. The contradiction between the constancy of the single form, whether we call it species or variety, and the variability of all characters within the limits of an extended circle of forms, be it genus or family, has not yet been explained; Darwinian teaching has brought clearly to light the existence of this contradiction, but it has not yet discovered how to resolve it.

GEORG KLEBS.

V

On the Scientific Measure of Variability

TO review the reviewer is always a profitless task, and yet I am tempted to repeat what must be more or less of a failure. In this case, however, the reviewer happens to be a man whose opinion deservedly carries weight, and many readers may consider that he must have fairly epitomised the statements made in my paper on "Variation in Man and Woman." This does not seem to me to be the case; and, in justice to myself, I wish to distinctly repudiate one or two opinions Professor Weldon fastens upon me (*Natural Science*, vol. xi., pp. 50-54).

In the first place, Professor Weldon states that the object of my paper "is to support the contention that women are, on the whole, more variable than men." I wish to entirely disclaim any such object. The paper was written with the purely scientific aim of comparing the variation of man and woman, and was due to the fact that a study of numerous writers on the subject had led me to believe that there was as yet no evidence to show greater variation in one sex than the other; that most of the reasoning on the subject was invalid and nearly all partizan. I may safely say that the two friends who undertook with me the lengthy arithmetic involved had no "contention" and no bias. We simply thought that no evidence of a satisfactory kind was forthcoming, in the case of man, for Darwin's law of the greater variability of the male; and we determined, so far as was possible, to undertake a thorough investigation of the question. And what is the general conclusion reached? That the female is more variable than the male—which is the impression any reader must form from Professor Weldon's review? Not at all. In the summary I distinctly state that, in the material considered, there is no evidence of greater male variability, but rather of a slightly greater female variability. In the body of the paper it is stated that the less civilised races have nearly equal variability for the two sexes, while, in the more highly civilised, woman—probably owing to the lessening of her struggle for existence as compared with man—has apparently greater variability. I conclude:—"I would ask the reader to note that I do not proclaim the equal variability of the sexes, but merely assert that the present results show that the greater variability often claimed for man remains as yet a quite unproven principle." The "contention" of the paper is that there is no proof forthcoming of man's greater variability. Whether either sex is the more variable is left for the future to settle in the following words:—

"When more material is available, and finer methods are applied, then perhaps it will be possible to detect a more noteworthy preponderance of variability in the one or other sex." And, again, in referring to the slight preponderance of variability observed in woman:—"I strongly suspect that this preponderating variability of women is mainly due to a relatively less severe struggle for existence." These are not the words of one whose "object is to support the contention that women are, on the whole, more variable than men." They seem to me the words of one who wishes to reach a scientific conclusion without any party or sex bias.

In the next place, Professor Weldon objects to my use of the co-efficient of variation. He apparently wishes to assert that absolute variation is the real test of most things. I am somewhat surprised to see him advocating this test. It is not so many months since an American critic pointed out how fatally this measurement of variation affected the conclusions of a certain paper of Professor Weldon's on selective mortality in crabs. I have not seen any answer to that criticism, and I very much doubt if one can be found. Some years ago I pointed out to him that the same measurement of variability led to absurd results in the case of the selective mortality of men.

But even here Professor Weldon puts in my mouth opinions I have never expressed. He writes:—"The violent assertion that there is only one 'scientific' measure of variability is therefore to be regretted." Now, so far from asserting the validity of only one measure of variability, I carefully state in paragraph (*c*) of my conclusions:—

"There is more than one method of quantitatively measuring variability, but the measure which is really significant for progressive evolution has not hitherto been determined."

On p. 343 I write, "We may stay to ask whether the statistics of skull capacity do not in themselves give us any information with regard to the superiority of either standard deviation or the co-efficient of variation as a test of that variability which is valuable for progressive evolution," and on p. 345 I conclude that the results do not enable us to say offhand that absolute or percentage variation is a better measure of the variability which is a source of progressive evolution. These are hardly the words of one who has made a "violent assertion that there is only one measure of variability."

What is it then that I have asserted? Simply this, that if it be necessary to compare the variability of the same organ in two sexes which have on the average different sizes, it is absurd to use absolute variations. This conclusion is nothing new; it has long been familiar to craniologists and anthropologists. They have, for instance, compared brain-weight relative to body-weight or to stature. I contend that the proper measure is the percentage variation on the mean. My words are, "I hold that the only useful sense in which we can study relative variability is by endeavouring to answer the problem. Is one sex closer to its mean, more conservative to its type than the other? and that the only scientific answer to this lies in the magnitudes of the percentage variations of the two sexes for corresponding organs." It will be seen at once that this is not, as Professor Weldon appears to misinterpret it, an assertion of a single scientific measure of variability but a statement of opinion as to the only useful way in which we can compare in the two sexes the relative variability in the same organ. Professor Weldon, indeed, seems to confuse two things, the scientific measure of variability and the effectiveness of this variability for different organs in the struggle for existence. Because the variability of one organ is said to be twice that of another organ, it does not follow that the functional importance is doubled. The scientific measure of variability is one thing, the effectiveness of this amount of variability in the struggle for existence is another and different thing. But even here I am prepared to assert, although I have not done so in my paper, that the co-efficient of variation, without being proportional to the "effectiveness," is far more reasonable as a measure of effectiveness, when we are dealing with the same organ in different sexes, or in individuals of the same sex at different ages, than absolute variation. It seems to me that the non-regard of this point has led to the nugatory character—not of the splendid system of measurements on crabs made by Professor Weldon—but of several of the conclusions he has endeavoured to base upon those measurements. I cannot get over the fact that the variation of an inch in the leg of a pony is not the same thing as a variation of an inch in the leg of a horse. Out of the 155 cases dealt with in my paper, woman is in 62 or 63, I think, absolutely more variable than man, and man absolutely more variable in some 85; in the remainder the sexes are sensibly equal. But since woman is smaller than man in the weight and size of nearly all organs, absolute variability can only be adopted with the same justification as we should say that an inch is the same variation in the leg of a pony or a horse, or a cubic centimeter the same variation in the capacity of the brain of a man or a new-born infant.

If Professor Weldon asserts that taking the co-efficient of variation as a standard, then the same amount of variation in man and woman has more effectiveness in one sex than the other, I must reply, no one has yet investigated this point; my own conclusions on skull measurements, so far as they have yet gone, seem to show that the co-efficient is at least a rough measurement of effectiveness. But it must be clear that until we have investigated the relation of effectiveness to some clear measure of variation, Darwin's law of the greater variability of the male is entirely unproven. Whether we put effectiveness as a function of mean and of standard deviation, or as a function of mean and the ratio of standard deviation to mean, is not, at first sight, a matter of great importance; it is to be settled rather by what the algebraist considers a convenient shape for his formulae. It is the biologist who has to determine the form of the function. It probably varies widely from species to species and organ to organ, but it may reasonably be supposed to vary only continuously and gradually with age and sex. If the selective death-rate of any species, however, be a function only of the mean and standard-deviation of any particular organ, then the theory of dimensions shows us at once that the death-rate cannot be a function solely of absolute variation, but must be a function of the ratio of absolute variation to the mean, *i.e.*, of the co-efficient of variation.

Lastly, if Professor Weldon thinks I have reviewed my biological critic harshly, I would remark that I submitted my paper a year ago in proof to a valued biological friend, I still have in a familiar hand-writing "no suggestions to make." That Professor Weldon should find in my paper a "violent assertion" to be regretted, confirms my view that modern biology is a house divided against itself.

KARL PEARSON.

VI

Initiation Rites of the Arunta Tribe, Central Australia

In *Natural Science* for April of this year (vol. x., pp. 254-263) we gave an account of the Horn Expedition to Central Australia, and drew special attention to the valuable anthropological observations of Mr F. J. Gillen. This gentleman's residence of fourteen years among the Aruntas of Alice Springs in Central Australia has enabled him to associate himself with them on terms of the closest intimacy, and he is looked on as a full member of the tribe. Since the Horn Expedition, three years ago, Professor Baldwin Spencer of Melbourne has been in constant correspondence with Mr Gillen, and has twice used the university vacation to revisit the district, although the heat during the summer months is exceedingly trying to any European. During the summer of 1896-7 Professor Spencer undertook the long and difficult journey to Alice Springs in order to witness the most mystic rite of the Aruntas, and the one of rarest occurrence, namely, the fire-ceremony, for which preparations had been made by the tribe for eighteen months beforehand. His unique experiences were communicated to the Royal Society of Victoria on his return, early in April, and the following interesting account is given in *The Australasian* for April 17, 1897:—

Within a mile or two of the picturesquely-placed telegraph station, with its tiny cluster of stone houses, the strange aboriginal ceremonies were to be celebrated, and here for four months Professor Spencer made his headquarters. In order to be at hand when all the rites were being performed, Mr Gillen and the professor occupied a wurley, built on the sacred ground of the natives, and provisions were brought out from the station. Driven to desperation by flies, which had to be actually brushed off every article of food while it was being put into the mouth, slowly grilling under the tropic sun, and choked by the clouds of dust which every gentle breeze raised, the two observers had to make notes, take photographs, and measure evil-smelling natives for scientific purposes, when other employment slackened. The uncertainty as to when and where the next ceremony would take place kept Mr Spencer and Mr Gillen at all times on the *qui vive*, and on several occasions they had to tear after the blacks at mid-day over two or three miles of scrubby, stony ground, carrying heavy full-plate camera and notebook to get an accurate record of what was going on. In all, 200 photographs were taken under extremely trying conditions. It is little wonder that the many friends of Professor Spencer were rather shocked to see him looking so parched and sun-dried on his return to civilisation.

Initiation Rites.—The Arunta tribe, like several other Australian tribes, is divided into sections or classes, which are four in number. In their details the relationships of these classes are very complicated, and are fixed by definite rules which are carefully observed by the blacks. It may be briefly stated that a man must marry out of his own class, while the children belong to yet a third class, certain members of which class are then his tribal brothers and sisters.

There are four grades of initiatory ceremonies which an Arunta man must go through before he becomes a full member of the tribe. Up to about ten years the boy lives in the women's camp, and accompanies them in their search for such food as roots, seeds, grubs, and the like. His tribal brothers then paint him on the chest and back, and he is thrown up into the air and caught. This is supposed to be beneficial to his growth. After this he now lives in the bachelors' camp, and accompanies the bachelors on their hunting expeditions.

Eight or ten years later he has to submit to circumcision and subincision, as described by Dr E. C. Stirling and Mr Gillen in the results of the Horn expedition. After that he may take a wife, and engage in other ceremonies. In the tribes of the East of Australia this stage is marked off by the knocking out of one of the front teeth, a ceremony to which a good deal of importance is attached. Amongst the Aruntas, though a front tooth is occasionally knocked out, yet the habit seems devoid of any sacred import, and appears to be a survival, the meaning of which is forgotten.

Totem and Churinya.—When the candidate has reached thirty, or in some cases forty years, he takes part in two sets of ceremonies which extend over several months, and it was these ceremonies which Messrs Spencer and Gillen had such unique opportunities of observing. The first set deals with the various totems of the tribe. There are very large numbers of totems in the tribe, and to one of these each black owes allegiance, and may be called by its name. Some may be kangaroos, others native peach trees, others dingoes or witchetty grubs, and so on. It has long been known that the marriage rules of the Arunta were governed, not by the totems, but by the classes previously alluded to, and why certain persons are attached to certain totems is one of the most peculiar and important results which Messrs Spencer and Gillen have obtained. Closely interwoven with the idea of the totem is the significance of the churinya, or sacred stones and sticks. These objects are flat, oval, or elongate pieces of stone or wood, carved all over with incised lines which, in the Central Australian tribes, are circles or segments of circles, while in Western Australia they take the form of zig-zag lines. Each man has his own churinya, which is apparently looked on as another embodiment of himself, and yet at the same time it possesses a mysterious sacred significance. The women and the uninitiated are not allowed to look at it. The carvings on the churinyas of persons of the same totem are very similar. The churinyas are not kept by the blacks to whom they belong, but they are carefully hidden in some definite locality by one or two of the old men, each totem having its own particular set of such stations. The blacks state that in the 'dream-times' of the far distant past, when their ancestors came into the country, those of each totem kept strictly by themselves. At this time they are not quite clear as to whether those whose totem was, say the wild duck, were really human beings, or partly the animals or plants the names of which they bear.

The lines of these migrations are related in great detail in their traditions, and each camping ground is exactly located, so that the whole country is interlaced with lines of route, and dotted over with innumerable camps. When one of these 'dream-time' ancestors died, he was turned into a spirit-child, and as such dwells near one of the camping grounds, always carrying in his hand one of the churinyas. Conception is believed to take place by the entry of one of these spirit-children into the mother, the spirit-child dropping his churinya on the ground at the time. On the birth of the child the place is searched for the lost churinya, and by the kindly offices of one of the old men the search is usually successful. If it be not, a wooden one is made of hardwood, such as mulga. The stone churinyas are the more ancient form, and do not appear to be made at the present day. This then fixes the totem for the individual, and explains why in the Arunta tribe the child is not of the same totem as one of the parents, as is the case in some of the neighbouring tribes of Central Australia.

The members of each totem have a ceremony connected with their totem, which they alone are allowed to perform, and which has for one of its objects the increase of the animal or plant from which the totem takes its name. The eating of this animal is not tabooed to those who bear its name, as is frequently the case in other parts of the world; in fact, it is considered necessary for the chief performer to eat a portion of his totem, or the ceremony will fail.

General Programme.—In their general plan these ceremonies are much alike. The chief performer is elaborately decorated with patterns in eaglehawk down stuck to his body with blood drawn from some member of the party. This down is coloured red and yellow with ochre; other parts of the body are smeared with a black pigment mixed with grease. The amount of blood drawn on these occasions is at times surprising, it being estimated that one man allowed five half-pints to be taken from him during a single day. The decoration of this performer is completed while the black candidates, if they may be so termed, are away hunting. On their return to the sacred ground they dance vigorously round him for some time. In most of these performances the decorated men then imitate the actions of the animal whose totem they bear, and in some cases the acting is described as wonderful. In one mock combat two performers represented two eaglehawks struggling for a bone, and wildly flapping their wings, which were represented by a bunch of gum leaves in each hand.

The 'parra,' or sacred ground, was laid out with great care, and one of the most peculiar sights was to see the candidates lying in a row with their heads close to a long bank of earth, as they were required to do during most of the nights. Absolute silence was entailed, and the strain during the months through which the ceremonies lasted must have been great, and have considerably influenced the hysterical, exalted frame of mind which they at times showed.

Fire Rites.—After a month devoted to preliminary rites the fire ceremonies began. The men to be initiated formed into a body, and, holding a shield of gum leaves over themselves, went to the women's camp. They were accompanied by a number of the old men swinging bull-roarers. This seems to be the only occasion on which the women, on hearing the dread sound, do not run and hide themselves, nor are they at any other time allowed even to gaze upon the sacred implement. The women who were prepared, ran at the body of men, and threw burning branches on to them, which the men tried, not very effectually, to ward off with their roof of leaves. This ceremony was repeated daily for about a fortnight. Next, a large fire, about twenty feet across, was made and covered with green leaves; on this terrible heap the candidates lay for some time, several at once, others calmly standing by and waiting their turn. The heat of the fire was very considerable. Professor Spencer knelt on the heap to try it, but could not endure it, even with thick trousers on. This performance was concluded by all present howling and hurling firesticks about.

During the evening, when all the candidates were lying in a row as usual, one of the old men seated himself before them with a decorated piece of wood which he held upright, and slowly and steadily knocked on the ground. At each side of him another old man sat holding his wrist, and assisting in this wearisome work, which, with most remarkable endurance, was kept up without a pause from half-past nine at night till about five next morning.

The number of candidates was very large, there being more than a hundred who were initiated. On the day following the final ceremony took place, the men crossing over to the women's camp, and each kneeling on a fire there.

Concluding Notes.—Each of the old men who were directing operations had men of his own totem under his charge, and for their proper initiation he was responsible. During the whole period of nearly four months they were not allowed to speak to him. At the conclusion of the rites they had to bring him some food-offering, such as cooked wallaby, and begged him to make them speak. He then touched their lips, and the ban of silence was removed.

A good deal of the significance of many of the ceremonies has probably been lost, but their main object seems to have been to test the endurance of the young men, and to teach them the past history of the tribe, while the possession of a knowledge of the correct method of procedure by the old men, who practically formed a council for the administration of the whole series of rites, would naturally cause them to be held in high esteem.

As this ceremony is only performed at intervals of many years, it is more than likely that, with the advance of the white man, the present may be the last occasion on which it will be performed with the completeness in which it was witnessed by Professor Spencer and Mr Gillen. The results so laboriously obtained are consequently of peculiar value. *θ.*

SOME NEW BOOKS

The Structure of Corals

Microscopic and Systematic Study of Madreporarian Types of Corals. By Miss Maria M. Ogilvie, D.Sc., *Phil. Trans. Roy. Soc.*, London. Vol. 187, 1896, pp. 83-345. Price, 11s. 6d.

Miss Ogilvie's work on the microscopic and systematic study of Madreporaria, read before the Royal Society in November 1895, was a long time in appearing in print. It is a copious and an ambitious work, one which reflects great praise on the industry and the capacity of the authoress, yet, as must always be the case in a work of such pretensions, it is open to a considerable amount of criticism in details. To criticise it adequately one would have to enter into minutiae which are of little interest except to the specialist; to criticise it at all one must necessarily enter into details which are unfamiliar even to zoological readers, unless they happen to have made a special study of corals.

It is well known that the stony corals, though they present but a limited range of structural peculiarities, are so rich and various in detail and display such infinite variety of form, that their classification presents great difficulties. These difficulties have been enhanced by the fact that even now the anatomy of the polyps is only known for a relatively small number of forms, and that there is a vast assemblage of extinct corals of which we only can know the structure of the skeletons: the nature of the polyps to which the skeletons belonged can only be inferred from the small knowledge we have of the anatomy of recent types. A great number of the palaeozoic forms of corals appeared to differ so much in their characters from later and recent corals that they were classified apart by Milne Edwards and Haime under the names Rugosa, Tabulata, and Tubulosa. The groups Tabulata and Tubulosa have disappeared some time since, but the group Rugosa has remained, in spite of the fact that several authors, but especially Gottlieb von Koch, have shown that the intimate structure of the coralla of many Rugosa is in all essential characters the same as that of recent corals. The group of Rugosa has survived, against the better judgment of many investigators, because it was convenient to palaeontologists, and in the study of corals as in that of many other groups, palaeontologists and zoologists have worked with too little heed to each other's doings. Miss Ogilvie writes as a palaeontologist, but as one whose ideas are moulded by the teachings of zoology. A great part of her work refers to the structure of recent forms, and her conclusions as to the nature and systematic position of ancient corals are founded on the knowledge which she has gained from her studies of living forms. The result is that she has turned the old classification of Milne Edwards and Haime upside down, and even those who have accustomed themselves to the modifications introduced by Martin Duncan and Quelch will scarcely recognise the classification set forth in a very ingenious diagram on page 331 of this work. Corals are divided into two sections, Zaphrentoidea or Madreporaria Haplophracta and Cyathophylloidea or Madreporaria Pollaplophracta. The former section is divided into two sub-sections, the Coenenchymata, including the families Poritidae, Madreporidae, Pocilloporidae, Oculinidae; and the Murocorallia, including the Zaphrentidae, Turbinolidae, Amphiastraeidae and Stylinidae, the two last named being new families, or nearly so. *Euphyllia* is taken as the living type of the Amphiastraeidae, *Galaxea* of the Stylinidae. The section Pollaplophracta is divided into two sub-sections; the Septocorallia, including the families Cyathophyllidae, Astraeidae and Fungidae, and the Spinocorallia, including the family Eupsammidae. It will be seen that the old groups of Aporosa and Perforata as well as the Rugosa disappear altogether; that corals which were known as perforate are placed alongside of aporose corals and *vice versâ;* thus the Eupsammidae are ranked near the Astraeidae, the Pocilloporidae near the Madreporidae.

These sweeping changes are based upon a microscopic examination of the coralla of many recent and extinct forms. Make a section through a coral skeleton and you will recognise in the middle of each septum, or other component, a dark line or centre. With thin sections and high powers the dark line resolves itself into a series of dark spots, from which the crystalline elements of the corallum radiate outwards in diverse ways. A close comparative study of these features has convinced Miss Ogilvie—or we should rather say now, Mrs Gordon—that they afford a new and natural basis for classification, one which is applicable to the study of both extinct and recent corals, because the feature in question is usually well preserved in fossil remains. A further convenience is the fact that the microscopical structure of the corallum may often, if not always, be inferred from its superficial characters, *e.g.* granules, striae, and serrations of septa. To give details of the septal characters is here impossible; the structure is intricate and demands much space for explanation. It need only be said that anybody, having read this part of the work, may easily verify the truth of the statements made. Points which have hitherto escaped notice are here brought forward for the first time, and the new observations are invested with an importance which, if not always acceptable, is invariably interesting and suggestive.

Miss Ogilvie not only describes the microscopic character of the corallum; she also accounts for it by seeking to prove that the ultimate elements of the coral skeleton are minute scales, each composed of a bunch of minute crystalline fibres, and that each such scale is in fact a calcified cell or calicoblast, which is bodily converted into the calcareous tissue of the skeleton. In making this assertion Miss Ogilvie treads on contentious ground. She adopts and expands a view originally put forward by von Heider, but not generally accepted, because it appeared to be negatived both by examination of fresh adult coralla and by the embryological researches of von Koch. The latter author, whose statements are worthy of the utmost credit, states most positively that in the development of *Astroides calycularis,* the first deposit of calcareous tissue takes place *between* the basal ectoderm and the surface of attachment, and that it is secreted by, not formed within the ectoderm cells. These statements are confirmed by H. V. Wilson for *Manicina areolata,* and in a question of this sort it requires very strong evidence to upset the proofs from embryology. Miss Ogilvie's evidence is hardly strong enough; the appearances which she describes are not unfamiliar to students of corals and are susceptible of a different interpretation, but she has at least reopened the question, which will have to be settled on better evidence than that which she has adduced.

Amongst the many figures which illustrate the work there are several showing the relations of hard and soft parts in recent corals. Some of them are correct, others are misleading, if not positively incorrect. Take, for instance, the diagram of *Turbinaria* on p. 209. The anatomy of this genus has been thoroughly described by Dr Fowler, and we learn from him that there is a system of canals which permeate the corallum and communicate with the polyp cavities. These canals anastomose freely, but Miss Ogilvie's figure shows only a few digitate or branched diverticula; no anastomoses, no network, and no transverse communications with the polyp cavities. The figure of *Fungia* on p. 169 can only be called a diagram of theoretical relations. As a matter of fact the soft parts of *Fungia* have not the structure shown in the figure. Miss Ogilvie homologises the tissues on the aboral face of *Fungia* with the edge-zone of other corals. This is right enough, but it is not right to assume, as she does, that there is no communication between the synapticula and through the theca between the cavity of the edge-zone and the general cavity of the body. As a matter of fact definite canals pass between the synapticula, some are united below the level of the synapticula by a radial canal, some are directly continuous with the cavities of the edge-zone. The mesenteries are best developed above the synapticula, but some extend also between and even below them, the rule being that the mesenteries are attached to synapticula, either above or at their sides, but some extend far down and send narrow mesogloeal bands to be attached to the basal wall of the disc. Other features, such as the position of the tentacles, are not correctly represented. The writer has the more confidence in making these statements since he has recently examined the anatomy of *Fungia,* in order to test the correctness of this figure. One is inclined to suspect that Miss Ogilvie, whilst making plentiful use of the anatomical researches of other authors, has not herself any great familiarity with the structure of coral polyps. There is some internal evidence that, after deciding in her own mind how the corallum was formed, she has inferred the anatomy of the polyps from the microscopical characters of their coralla, without studying the actual relations in a sufficient number of instances. Such inferences are apt to be misleading. Whether this is the case or not, Miss Ogilvie has been led by her views on the formation of calcareous tissue to give a lively but an unlikely account of madreporarian development and evolution, an account which is in harmony with the figures criticised above, but which does not and cannot explain the diagnostic character of the Madreporaria Perforata of M. Edwards and Haime—viz. the presence of a complex canalicular system in the wall, the septa and the coenenchyme. For her the form and structure of the calcareous laminae or bars are the result of foldings, wrinklings, and tuckings in and over of the flexible zooid. On p. 315 there is an imaginative description of the evolution of the madreporarian zooid, which is represented as pulling in and tucking up its body in various places, forming invaginations here, evaginations there, as if guided by some predetermined impulse, and we are to believe that the form of the corallum is determined by these almost purposive wrinklings and corrugations of the zooid, which fills up the cavities and folds of the creases in its body with calcareous tissue. References, scattered throughout the volume, to invaginations—a word, by the way, which is used in a most puzzling variety of meanings with regard to spatial relations—show that Miss Ogilvie is dominated by the idea that the wrinkling and pitting of the soft tissues was the antecedent, the formation of calcareous structures the consequent. The skeleton, it is true, is formed by the soft tissues, but it by no means follows that the form of the skeletal parts is the result of the pre-existing form of the soft parts. The two elements have been formed *pari passu*, changes in the one reacting upon the other, and the final shape and mutual relations are the result of a continuous correlated development of which we cannot affirm, at any given point, that the growth of the one part preceded or dominated the growth of the other. On Miss Ogilvie's supposition it is most difficult to account for the formation of the canal system in perforate corals, and we cannot but suspect that this difficulty has led to her giving a theoretical rather than an actual picture of the structure of *Turbinaria* and of *Fungia*. Not that *Fungia* is a perforate coral.

In classifying the Fungidae, Miss Ogilvie has left out of consideration the fact that the young *Fungia* is a true aporose coral, indistinguishable from a Turbinolid, even to the absence or at least the very slight development of synapticula. This fact points to a close relationship between the Fungidae and the Turbinolidae, yet they are classed far apart, the former among the Pollaplophracta Septocorallia, the latter among the Haplophracta Murocorallia. In fact, the more one examines the grounds of the classification adopted in this work the less satisfactory does it appear. The group Coenenchymata strikes one as purely artificial. The subsections Murocorallia and Septocorallia are founded on the presence or absence of so-called 'thecal' pieces. This is a partial revival of the classification proposed by von Heider and adopted by Ortmann, it has been severely criticised by von Koch and others, and it is not too much to say that it is founded on a misconception. There is no essential difference between 'thecal' and 'septal' structures. Both are formed in the same manner from the same regions of the polyp. In many forms sections taken low down in the corallum show an alternation of septal and apparently thecal pieces. Higher up it is found that calcareous lamellae project inwards from the supposed thecal pieces, so that the last named appear as septa. The same coral appears to have an 'eutheca' in one part, a 'pseudotheca' in another part, is therefore a Murocorallian in one region, a Septocorallian in another. This fact has been repeatedly emphasised by von Koch, and a clear and convincing discussion of the question is to be found in his recent work in Gegenbaur's "Festschrift." The Turbinolidae are a standing example of the unreliability of a classification founded upon the supposed difference between an Eutheca and a Pseudotheca. They are positively stated on p. 319 to have "a well built *theca*, whose fibrous elements are set in a direction at right angles to those of the septa." *Caryophyllia* is regarded as a typical Turbinolid, yet the writer has sections of *Caryophyllia smithii* which demonstrate conclusively that there are no such thecal pieces with fibrous elements set in a direction at right angles to those of the wall. The wall is in fact a typical pseudotheca, formed by the coalescence of the thickened peripheral ends of the septa. The same must be asserted of *Stephanotrochus* and others. Remembering that the wall of *Caryophyllia* is a pseudotheca, it is difficult to place *Flabellum* in the same group with it, and Miss Ogilvie's remarks on *Flabellum* suggest that she has not grasped the meaning of von Koch's argument that its 'theca' is in fact an epitheca.

It would not be difficult to find other materials for criticism, but enough has been said to show that Miss Ogilvie's more general conclusions must be received with caution. So long as she deals with the matter of her own original observations she is on safe ground, and in describing the finer structure of the corallum she has added largely to our knowledge, and has opened up a suggestive field of research. But she has marred her work by an effort to be too comprehensive, and in her attempt to form a complete system she has been obliged to rely on characters other than those which have been the object of her researches, and therefore has, in our opinion, been led into error. However extensive and excellent her work, it is not yet sufficient to allow of the making of a wholly new and general scheme of classification. But whilst one cannot accept the classification as it stands, one must feel that it abounds in suggestions. The suggested lines of descent of living from extinct forms, offer most interesting material for enquiry. There is an ingenious, but not wholly new, suggestion as to the probable relation of mesenteries in palaeozoic corals, involving an explanation of tetrameral arrangement of septa. The explanation of the nature of the fossula is ingenious and probable; unfortunately it is one of those points which can scarcely be proved by observation.

In conclusion, the work, though it may have defects, is a most useful one; it must always influence other workers in the same field, and it is probable that some of the main features of the classification will come to be universally adopted. One thing, at least, is certain, nobody will henceforth speak of the group of Rugosa.

G. C. Bourne.

R. Hertwig's Text-book of Zoology

Lehrbuch der Zoologie. By Dr R. Hertwig. Fourth improved edition. 8vo. Pp. xii. 612, with 568 text-figures. Jena: G. Fischer, 1897. Price in paper wrappers, 11 marks 50 pf.; bound, 13 marks 50 pf.

It is just two years since we reviewed the third edition of this clear and correct text-book, a fact that proves, at all events, the success of the work. The present edition differs from its predecessors, not only in the slightly increased size, but in some extension of the parts dealing with the Sporozoa and the Vertebrata. The Sporozoa have of late years assumed great economic importance, especially in reference to the breeding of fish and to certain diseases of man, as was insisted on by Professor Ray Lankester in *Natural Science* for August 1896. Dr Hertwig has therefore done well to give them greater prominence. Among the Vertebrata, while Dr Hertwig has endeavoured to accommodate Boulenger's classification of the Reptilia to the restricted needs and limits of a text-book, he has found himself still unable to introduce the modifications in the classification of birds that have been held necessary by certain recent anatomists.

In our former review we alluded to the weakness of the palaeontological part of this otherwise admirable book, and we regret to find that weakness just as conspicuous. It leads to sins, not merely of omission, as the passing over of all the differences between an ammonoid and a nautiloid shell, and the absence of any account of the shell in the decapodous cephalopods, but also of commission, as the long obsolete division of the crinoids into Palaeocrinoidea and Neocrinoidea. A knowledge of palaeontology, too, would have saved Dr Hertwig from devoting space to the views of Haeckel on the Cystidea, when that space is so valuable that this most important class of all the echinoderms has to be dealt with in a dozen lines. We trust that the fifth edition, which is sure to be called for, will show some consideration to those extinct animals on the knowledge of which our classifications must ultimately depend.

Arthropods for Beginners

Through a Pocket-Lens. By Henry Scherren, F.Z.S. 8vo. Pp. 192. London: The Religious Tract Society. 1897. Price, 2s. 6d.

Mr Scherren is well known as a writer of attractive little books on natural history for beginners, and his reputation in this respect is not likely to suffer from the volume that is now under notice. Its object is to show how much may be learnt with an ordinary pocket-lens and simple appliances; but it is surely somewhat of a pity that the title is not more indicative of its contents. For the purpose could have been carried out with equal satisfaction if minerals, or any group of the smaller plants or animals, had been selected for study. As a matter of fact Mr Scherren's choice fell, and fell wisely, upon the Arthropoda, a group to which he has devoted much of his spare time, and which is peculiarly suitable for the purpose in hand on account of the abundance and obtrusiveness of its species, its attractiveness to young naturalists and collectors, and the extent of the variation in structure and habits that it displays.

The first chapter contains much useful advice on the question of lenses, needles, forceps, beakers, etc., showing that all needful appliances for the investigations illustrated in the following chapters of the book may be obtained by the expenditure of a small sum of money. In the way of lenses, however, there is one object which seems to have escaped Mr Scherren's notice, and to which he will perhaps be glad to have his attention drawn. This is the ordinary watchmaker's lens, which will be found invaluable for dissecting purposes, since with a little practice it can be held in the eye, leaving the two hands free for the manipulation of the needles and forceps.

The types for study have been advisedly selected, being those, like the cockroach, water-beetle, garden-spider, and prawn, which can be obtained in almost every country district, or in any of our towns or their neighbourhood, and may very easily be kept alive in suitable surroundings. For one of the great merits of this little book is, that it recommends an observation of the habits of the species before they be submitted to the process of dissection.

The errors of the book are few and, on the whole, unimportant. It is not however usual to regard the carapace in the Crustacea as synonymous with the cephalothorax; and to say that the former consists of fourteen segments in the prawn, when it really represents the tergal elements of but two is misleading. Moreover, and since Mr Scherren—not without the countenance of authority—applies the word 'joint' to the internode or segment of a limb, it would be interesting to learn by what term the point of junction of two 'joints' is to be recognised. Lastly, Dr David Sharp will not be flattered by the ascription to him of the authorship of the 'Myriapoda' in the Cambridge Natural History. In spite of these blemishes, however, the book may be cordially recommended to beginners as an excellent practical lesson in the elements of the morphology and bionomics of the Arthropoda. R. I. Pocock.

For the Young Entomologist

Faune de France: Orthoptères, Neuroptères, Hymenoptères, Lépidoptères, Hémiptères, Diptères, Aphaniptères, Thysanoptères, Rhipiptères. Par A. Acloque. viii. and 516 pp., with 1235 figures. Paris: Baillière, 1897. Price, 8 francs.

The Young Beetle-Collector's Handbook. By Dr E. Hofmann, with an introduction by W. Egmont Kirby, M.D. 8vo. viii. and 178 pp., 20 coloured plates. London: Swan Sonnenschein & Co., 1897. Price, 4s. 6d.

The fauna of France in the groups mentioned above may be estimated at about 15,000 species, and M. Acloque disposes of them in this small volume. We have previously (*Natural Science*, May 1896, p. 346) explained the plan of the work, and need only add that, in the volume now before us, the necessity of keeping the number of pages within assigned limits has caused the author to abandon the attempt at dealing with species in the more extensive and difficult families; so that in these cases we find only tables of the genera.

In his preface the author recognises that this volume does not accomplish all that was intended when the scheme of dealing with the whole fauna of France in four small volumes was adopted. The system of terminations used in the names of the systematic groups is extremely repellent: the well-known family name Apidae becomes Apisidi; and as sub-family names we find Andrenii and Bombii, derived from Andrena and Bombus. Possibly the system is theoretically excellent, but it reminds us of Montgomery's lines about Nebuchadnezzar, who

> "murmured as he cropped the unwonted food,
> It may be wholesome but it isn't good."

Dr Hofmann's work is remarkably well printed, and the twenty plates include about 600 almost tolerable figures. But the matter contained in the pages is somewhat disappointing. It consists of brief descriptions, and of an introduction, too short and vague to be of much use, dealing with more general points. D. S.

The Museums Association

Museums Association: Report of Proceedings with the Papers read at the seventh Annual General Meeting held in Glasgow, July 21 to 25, 1896. 8vo, pp. 167. London: Dulau & Co., 1896. Price 5s.

If anything were needed to show the growth of interest in the questions connected with the administration and development of museums it may be found in the institution and continued prosperity of the "Museums Association," and the issue of the compact volumes of papers read at their annual meetings. Last year's meeting took place at Glasgow, and for the first time since its foundation seven years ago the association had an "Art-man" as its president in the person of Mr James Paton, curator of the Kelvingrove Museum and Corporation Art Galleries in that city. In his presidential address Mr Paton gives an extremely interesting account of the institutions under his direction, but considering that the majority of his audience must have been men of science, I think that he might safely have omitted telling them that the aim of the picture gallery is "higher and holier," presumably from the context, than the scientific museum. Few scientific men are wholly blind to Art, many are in the highest degree susceptible to its influence, but all the same—comparisons are invidious!

Mr F. A. Bather's paper entitled "How may museums best retard the advance of science," being of a satirical vein throughout, is very entertaining reading; and, on the whole, I think we may give ready assent to most of the ideas which he has chosen this method of conveying.

Other papers in the volume are:—Mr H. Coates on the Arrangement of the Perthshire Natural History Museum; Mr E. M. Holmes on Type Specimens in Botanical Museums; Descriptive Geological Labels, by Mr H. Bolton; Mr F. A. Bather on Electrotypes in Natural History Museums; Mr G. W. Ord on Chemistry in Museums; Suggestions for a Proposed Natural History Museum in Manchester, by the late Prof. Huxley; Clara Nördlinger on a Visit to the Directress of the Schleswig-Holstein Museum; Illustrated Lectures in Art Galleries and Museums, by T. Rennie; and the Lighting of Museums, by Thomas White.

The perusal of these papers brings under our notice two important questions connected with museum economy. The first of these relates to Type Specimens. It is extremely desirable that all museums should publish catalogues of the types contained in their collections, and by many museums this has already been done. But the idea of bringing together all the type specimens in the kingdom, perhaps in the world, as some people possibly desire, is not only utterly impracticable, but from some points of view not even desirable. A little travel is not at all a bad thing for a zoological worker, and no doubt when he visits other towns and other countries for the purpose of consulting type specimens he will have the opportunity of enlarging his mind in other directions as well. At the same time it may be readily conceded that the possession of type specimens by small provincial museums in out-of-the-way places, is not for the advantage of science.

But perhaps of greater importance, from a general point of view, is the disposal of the exhibited collections for educational purposes.

The now well-known idea that the exhibited portion of a museum should be a collection of labels, illustrated by specimens, may be carried a great deal too far—so far as to upset the fundamental idea of a museum without attaining the object desired. Those who wish to study any particular branch of Natural History ought to find, in a well-arranged typical collection, where the specimens are provided with suitable descriptive labels, much invaluable assistance—but such labels can never supply the place of proper text-books, studied at home as well as in the museum or laboratory. We may take an example in this connection from Mr H. Bolton's set of descriptive labels for the geological collection in the Peel Park Museum, Salford, which are published in full in this volume. These labels are in themselves very good and praiseworthy summaries of the present knowledge of the geological formations in Great Britain, but to the serious student who possesses a good text-book of geology they are wholly unnecessary, while to those who, like most of the general public, are previously entirely ignorant of the subject, they must be utterly unintelligible. The use of a label is, I presume, to tell what a specimen *is and what it shows*, and not to enter into a dissertation on any general subject, that being the business of the text-book and of the teacher.

Mr Ord's plan for teaching chemistry by specimens, diagrams, models, and descriptive labels, is to my mind carrying the educational theory of museums to a pitch of absurdity. A collection of metals, salts, &c., is no doubt a desirable feature in connection with the chemical department of a school or college, but you will learn chemistry only in the laboratory, and certainly not in a museum.

But, however some of us may disagree with some of the notions of individual writers of papers in the present volume, the Museums Association is bound to do good by promoting the free discussion of the questions at issue. R. H. TRAQUAIR.

THE GEOLOGICAL DEPARTMENT OF THE BRITISH MUSEUM

A GUIDE TO THE FOSSIL INVERTEBRATES AND PLANTS in the Department of Geology and Palaeontology in the British Museum (Natural History), Cromwell Road, London, S.W. 8vo. Pp. xvi. 158, with 182 text-figures. Printed by order of the Trustees, 1897. Price, 1s. ; also in two parts, price 6d. each.

THIS is a remarkable shillingsworth, so much so that anything less than effusive thanks for it smacks of ingratitude. Thanks, we are sure, the public will offer to the trustees, to Dr Henry Woodward, the popular keeper of the department, and to the able set of collaborators whose help he acknowledges. Only had the book been priced at five shillings or so could we have ventured on any criticism. We might then have asked for a little more co-ordination between the parts, a keener sense of proportion, and more careful selection of illustrations. With thirty-six pages devoted to the cephalopods, the four other classes of the Mollusca might have had more than eight. We should not, even in a geological guide, expect to find the Brachiopoda and Bryozoa associated with the Arthropoda and Vermes as a 'Subkingdom Annulosa.' To balance the fifteen pages on sponges, or the seventeen figures of trilobites, we should have asked for more than twenty-four lines on those particularly interesting forms, the Cystidea and Blastoidea, especially as our national museum possesses not only a fine collection of these rarities, but an officer well qualified to deal with them. And, in the account of the sponges, one might have suggested that a simple division into Silicispongiae and Calcispongiae scarcely represented modern ideas of classification. Finally, we should have demanded very much better paper and printing; and even now we hardly consider that the get up of the work befits a great public department—it is certainly inferior to that of previous Guides.

But whether regarded as a text-book or as a guide there is no doubt that in many respects the work is a great advance on anything hitherto attempted at the price. We hope that the public will recognise this, and that the speedy exhaustion of the edition may pave the way for another with all the merits and without the few defects of the present one.

"Palaeontographica" among Criminal Literature

Relics of Primeval Life. By Sir J. W. Dawson, K.C.M.G., F.R.S. 8vo, pp. ix. 336, with 67 figs. London: Hodder & Stoughton, 1897.

Sir William Dawson's book on "The Dawn of Life" having been for some time out of print, he has prepared the present volume to take its place. A good deal of the old matter and many of the illustrations therefore naturally reappear. The familiar story of the discovery of Eozoon, and of the spread of the belief in its organic structure, is again told, and Sir William Dawson refers to the principal criticisms on the other side. On pp. 273-274 Eozoon is made to tell the story of its own existence in an imaginary autobiography. It candidly admits its low intelligence and that it did not know whence it came; but "at length a change came. Certain creatures with hard snouts and jaws began to prey on me." Apparently the most objectionable of the hard-snouted generation was Möbius, whose work, in spite of its "large and costly figures" (p. 161), is described as valueless, owing to "that narrow specialism and captious spirit for which German naturalists are too deservedly celebrated." Möbius, according to Sir William Dawson, "did his best;" but so bad is his best that the publication of his memoir "was a crime which science should not readily pardon or forget on the part of the editors of the German periodical" in which it appeared.

Sir William Dawson does not give his opponents a very cordial invitation to continue the discussion, for he remarks in reference to the honest way in which Eozoon did his duty, that those who "dispute as to his origin and fate" are "much less perfectly fulfilling the ends of their own existence." So we will try to fulfil the ends of our own existence by discussing subjects in which an adverse verdict is not a "crime."

Our Naturalists

Mr L. Upcott Gill, 170 Strand, has kindly sent us "The Naturalist's Directory," 1897; price 1s. This, the third edition, will undoubtedly be useful to us, for it contains a large number of names that are not to be found in the ordinary lists of learned societies or in the invaluable "Zoologisches Adress-buch" of Friedländer. We presume the majority of those included in the above-mentioned works are here omitted of set purpose; there would be no difficulty in comprising them. At the same time, some hint might have been given as to the principles on which the selection was made. It is pleasing to find that there are so many people claiming to be naturalists in the British Isles. As for the foreign and colonial lists, their similar vagaries are perhaps due to the fact that they are avowedly restricted to persons desiring to correspond or exchange specimens with collectors and students in this country. The extension of these lists, no difficult task, would be of much use. The book also contains a trade-directory, a list of societies, field-clubs, and museums in the British Isles, a list of the principal natural history works published during 1896 in the British Isles, and a somewhat erratically selected but useful list of natural science magazines, in which, if we may judge from our own case, the information is not always so correct as it might be.

Botanical Bibliography

The Cambridge Botanical Supply Co. are distributing samples of their card catalogue of current literature relating to American botany. Items are arranged according to authors' names, but an edition of subjects is also in preparation. The matter is prepared by a board of editors, which includes the leading botanists of Columbia College, the National Herbarium, and other institutions, and is published under the direction of a committee of the American Association for the Advancement of Science.

The cards used are of heavy linen ledger paper made to order for this purpose. They are cut with extreme accuracy by an expensive machine. The size is 50 by 125 mm. The number of cards issued in 1894 averaged 49 per month; for 1895 the average was over 60, and the total number of cards to April 1897, 2319. Subscriptions ($5, paid in advance) may be sent to Wm. Wesley & Son, 28 Essex Street, Strand, London.

We commend this useful enterprise to the notice of the British Association for the Advancement of Science.

Westmorelandshire's Field Geology forms the subject of a paper by Mr H. G. Foster-Barham, which was read before the Burneside Mutual Improvement Society on February 11, 1897. The paper, which is illustrated by sketch-maps and sections, is published by R. Atkinson, Stramongate, Kendal, at 1s., post free, and gives a general account of the interesting district.

Scraps from Serials

The ever interesting *Scottish Geographical Magazine* gives two lively articles in its July number. Sir Henry Tyler writes on the

Geography of Communications, showing the enormous progress that has been made during the Victorian era, and Major A. C. Yates describes Loralai, a frontier cantonment in Baluchistan.

An article that should interest ethnologists is J. F. Hewitt's "The History of the Week as a Guide to Prehistoric Chronology," in the *Westminster Review* for July.

The *American Journal of Science* for July contains a description of *Ctenacanthus* spines from the Carboniferous Keokuk Limestone of Iowa, by Dr C. R. Eastman; a morphological account of two species of Cyperaceae, *Fuirena squarrosa* and *F. scirpoidea*, by T. Holm; contact metamorphism between slate and diabase in the El Pasco range, California, described by H. W. Fairbanks, who also writes on tin-deposits at Tenescal; notes on outliers of the Comanche series (Lower Cretaceous) in Oklahoma and Kansas, by T. W. Vaughan.

The July *Photogram* contains yet another article on the Photography of Birds' Nests, by Dr R. W. Shufeldt. An article on Technical Photography describes the studios of J. Bulbeck & Co. We should like to see something on the application of photography to the illustration of scientific papers. It is a failure in nine cases out of ten, no doubt, but whether it is worse than the ordinary draughtsman is a delicate point.

The *Irish Naturalist* for July is chiefly devoted to "Some Observations by English Naturalists (R. Standen, L. E. Adams, G. W. Chaster, and J. R. Hardy) on the fauna of Rathlin Island and Ballycastle District."

The Naturalist for July contains Mr John Cordeaux's Presidential Address to the Yorkshire Naturalists' Union. It deals with glaciers, plant-distribution, the antiquities of Holderness, and Yorkshire ornithology. Following this, G. O. Benoni encourages others by his example to take notes on natural history matters. One thing to be noticed is "the young oak thrusting up from [the field mouse's] abandoned home and store after a mild winter, as he stalks his rabbits down the woodside." It is indeed.

New Serials

Messrs Ginn & Co., Boston, U.S.A., announce *The Zoölogical Bulletin*, a companion serial to the *Journal of Morphology*, designed for shorter contributions in animal morphology and general biology, with no illustrations beyond text-figures. It is proposed to publish six numbers a year of about fifty pages each in the same form and style as the *Journal of Morphology*. The *Bulletin* will contain nothing but scientific communications. The editors are C. O. Whitman and W. M. Wheeler, assisted by a number of collaborators. The subscription price per volume of six numbers is $3.00, and single numbers are sold separately at 75 cents. each.

We have already announced the new quarterly *East Asia*, edited by Dr H. Faulds of Stoke-on-Trent, and published by Hughes & Harber of Longton, Staffordshire, at one shilling a part. The first number, published at the beginning of July, proves both entertaining and instructive. The chief articles are "Judicial Reform in China," by Dr Sun Yat Sen; "The Numeral System for the Blind in China," by Miss

C. F. Gordon-Cumming; "At a Japanese Barber's," by F. A. Bather; and an interesting account of "The Community in Cocos-Keeling and Christmas Islands," abstracted from a Blue-book. There is a list of recent books and magazine articles dealing with the far East. The Reviews and Notes would be better for more exactness of reference, and prices of books should be given. Anthropologists will find in this excellently printed journal many an item to interest them.

Further Literature Received

Synoptical Flora of North America, vol. i., pt. i., fasc. ii., A. Gray, ed. B. J. Robinson: American Book Co., New York. The Life-histories of the British Marine Food-fishes, W. C. McIntosh and A. T. Masterman: C. J. Clay. Traité de Zoologie, fasc. xi., xvi., ed. R. Blanchard: Rueff, Paris. Catalogus Mammalium tam viventium quam fossilium, ed. nov., fasc. ii., E. L. Trouessart: Friedländer. Thirty-first Ann. Rep. Museums and Lecture Rooms Syndicate, Cambridge. Ann. Rep. Raffles Library and Museum, Singapore, 1896. Third Rep. Whitechapel Public Library and Museum. Society for the Protection of Birds—Educational Series, No. 12.

The Reading of Words, W. B. Pillsbury: *Amer. Journ. Psychol.* Humanitarian League Correspondence. An Extraordinary Case of Colour Blindness, F. H. P. Coste (extract?).

Jersey Times, July 9; Amer. Geol., July; Amer. Journ. Sci., July; Amer. Nat., July; Annot. Zool. Japan, May; Feuille des jeunes Nat., July; Illinois Wes. Mag., June; Irish Nat., July; Journ. School Geogr., June; Knowledge, July; Literary Digest, June 12, 19, 26, July 3; Nat. Novit., June; La Naturaleza (Madrid), Nos. 18, 19; La Naturaleza (Mexico), No. 17; Naturalist, July; Nature, June 24, July 1, 8, 15; Nature Notes, July; Photogram, July; Rev. Scient., June 26, July 3, 10; Science, June 11, 18, 25, July 2; Science Gossip, July; Sci. Amer., June 12, 19, 26, July 3; Scot. Geogr. Mag., July; Westminster Rev., July; Proc. Biol. Soc., Washington, vol. xi., pp. 145-174; Bull. de l'Inst. Internat. Bibliogr., ii., fasc. 3; Trans. and Ann. Rep. Manchester Micros. Soc., 1896; Bull. Alabama Agric. Exper. Station, No. 80; La Bibliographie Scient., Bull. Trimestr., vol. ii., No 4; L'Année Biol., 1895.

NEWS

The following appointments are announced :—

Dr J. Büttikofer, of the State Museum in Leiden, to be director of the Zoological Garden at Rotterdam ; Dr Johannes Martin to be director of the Natural History Museum in Oldenburg ; Dr W. B. Pillsbury to be instructor in psychology and director of the Psychological Laboratory in the University of Michigan ; Dr Antoneo Crocichia to be professor of biology at the Catholic University, Washington ; Prof. W. T. Engelmann to succeed the late Prof. Du Bois Reymond as professor of physiology at the University of Berlin ; Adolf Beck, from professor-extraordinarius to professor of physiology at Lemberg ; Mr Muir, of Halifax University, to be professor of psychology in Mount Holyoke College ; Dr C. E. Seashore to be assistant-professor of psychology at the University of Iowa ; E. M. Weyer and M. Matsumoto to be assistants in the Yale Psychological Laboratory ; Jas. H. MacGregor to be assistant in zoology at Columbia University ; Dr Ludwig Heim to be professor-extraordinarius of bacteriology at the University of Erlangen ; Dr G. Boccardi to be associate-professor of microscopical anatomy at Naples ; Dr J. J. Zumstein to be professor of anatomy at the University of Marburg ; Dr Mayr to be prosector at the Veterinary College, Munich ; Dr H. Baum to be professor of osteology at the Dresden Technical High School ; Miss Mary E. Pennington to be Thomas A. Scott fellow in hygiene at the University of Pennsylvania ; Dr Brault to be professor of tropical diseases at Algiers ; S. I. Franz to be assistant in psychology at Columbia University ; Dr T. Fuchs to be associate-professor of palaeontology at Munich ; Miss Bertha Stoneman to be professor of botany in the Huguenot College for Women in Cape Colony ; Prof. Georg Volkens to be assistant in the Botanical Museum of the Berlin University ; J. R. Campbell to be lecturer in agriculture at the Harris Institute, Preston ; Dr A. O. Kihlman to be assistant-professor of botany at Helsingfors ; Herbert M. Richards to be tutor in botany at Columbia University ; Dr J. Szadowski to be associate-professor of geology at Klausenburg ; W. S. Boulton, of Mason College, Birmingham, to be lecturer in geology at University College, Cardiff ; Dr Philippi to be assistant in the Geologico-Palaeontological Department of the Natural History Museum in Berlin ; Frederick L. Ransome to be assistant geologist on the U.S. Geological Survey ; T. I. Pocock, of Corpus Christi College, Oxford, to be assistant geologist on the Geological Survey of the United Kingdom ; R. E. Dodge to be professor of geography at the Teachers' College, New York ; Dr W. F. Hume and L. Gorringe to be assistants on the Geological Survey of Egypt ; E. W. MacBride to be professor of zoology in McGill University, Montreal.

A biological station will shortly be opened near Sebastopol.

A summer school of biology on the Mississippi, not far from Monmouth, Ill., has been organised by Drs Maxwell and Swann.

The Derby and Mayer Museums at Liverpool have acquired the fine collection of flint implements brought from Egypt by Mr Seton-Karr.

L'Association Française pour l'Avancement des Sciences meets at St Etienne, August 5-12, under the presidency of Prof. E. J. Marey.

According to *Science* the University of Montana, at Missoula, has decided to erect a main building at a cost of $47,500, and a science hall at a cost of $12,500.

It is proposed to erect a monument at Moscow to the zoologist and anthropologist, Anatole Bogdanow, who died in April 1896.

A GIANT salamander of Japan, that had lived in the Jardin des Plantes for thirty-seven years, died on June 15, having a length of 1·30 metre and a weight of 24 kilograms. Two survivors mourn its loss.

SCIENCE states that a zoological club of nineteen members has been organised at Springfield, Mass., the president being W. W. Colburn, and the secretary Miss M. A. Young.

WITH reference to the note in our last number on the biological station at Plön, we now learn that the Prussian Government will assist it after October 1898.

AN expedition, under the leadership of Mr C. M. Harris of Augusta, Me., and at the cost of the Hon. Walter Rothschild, is studying the fauna and flora of the Galapagos Islands.

AN expedition to Okhotsk and Kamtschatka, under the leadership of K. Bogdanowitsch, has found gold at thirteen different places in the river-systems of the Jana, Kyran, Nemuj, Mute, and Lantar.

A BOTANICAL SOCIETY, named after Baron F. von Müller, has been founded at Perth, W. Australia. Its president is Sir John Forrest, the indefatigable Premier, and its secretary, Mr Skews.

SIR MARTIN CONWAY and Mr E. J. Garwood have returned to Spitzbergen to continue the exploration of the interior of the main island. Afterwards they will go to Horn Sound and finish the exploration of the southern peninsula.

Science states that it is proposed to enlarge the Missouri Botanical Garden, by the gradual addition of 80 acres, of which 21 will be drained and graded during the present season.

DR J. E. HUMPHREY, botanist, and Prof. W. K. Brooks, zoologist, are conducting a course of marine biology in Jamaica for students of Johns Hopkins University. The laboratory has formerly been at Port Henderson, on the south side of the island, but this year it is at Port Antonio, on the north.

THE U.S. Senate has agreed to admit free of duty printed books over twenty years old, books in foreign languages and those devoted to scientific research, and books and scientific instruments imported for public and educational institutions.

DR J. WALTER FEWKES, of the Bureau of American Ethnology, is making a third expedition to the Pueblo Region, where, says *Science*, he will survey and excavate the ruins of Kintiel, near Navajo Springs, Arizona. He is accompanied by Dr W. Hough of the U.S. National Museum.

MESSRS E. M'ILLHENNY, W. E. Snyder, and N. G. Baxton have gone to Point Barrow to collect the fauna and flora of N.E. Alaska. *Science* hears that the collections will go to the National Museum, U.S., and the University of Pennsylvania.

A BIOLOGICAL station, under the direction of Prof. C. W. Dodge, is to be established by the University of Rochester, N.Y., on Hemlock Lake. We have not yet heard that any fresh-water biological station is to be established in England.

THE *Scottish Geographical Magazine* states that on May 8 an expedition under Lieut. Drizhenko left St Petersburg for Lake Baikal, which will be sounded and surveyed, while natural history collections will be made. The work will be continuedfor five years.

The tenth congress of Russian naturalists and physicians, which was to have been held this August in Kiev, has, in consequence of the International Congresses of Geology and Medicine both meeting in Russia, been postponed till August 1898.

The University of Pennsylvania is to have a new Museum of Archaeology and Palaeontology. The architecture, says the *American Naturalist*, will be in Italian renaissance style. A botanical garden, covering ten acres, will surround the museum.

The International Postal Congress has decided that henceforth objects of natural history, animals, dried plants, or preserved zoological specimens may be sent as samples of merchandise, at ½d. for every two ounces, the maximum weight being 350 grammes.

Another expedition to Alaska is that of Dr W. H. Evans of Washington, who has gone to examine the agricultural resources of the district south of the Aleutian peninsula. Dr Sheldon Jackson goes on a similar errand to the Yukon basin.

The Société helvétique des Sciences Naturelles holds its eightieth annual meeting at Engelberg, near Mt. Titlis, Sept. 12 to 15. The president of the annual committee is Dr E. Etlin, Sarnen, Obwalden, to whom those who wish to attend should apply.

Mr R. H. Kitson, of Trinity College, has been awarded the Harkness Scholarship in Geology and Palaeontology at Cambridge University. Mr V. H. Blackman of St John's College and the British Museum, has been awarded the Hutchinson Studentship, for his researches on Algae.

The Zoological Society of London has awarded its silver medal to Mr Alexander Whyte, recently naturalist to the Administration of British Central Africa, who has sent home large collections illustrating the fauna and flora of Nyassaland.

With reference to our note on the extinction of the bison (which some call buffalo), it is interesting to learn from *Nature* that a variety known as the 'wood-bison' is still to be met with near Fort Chipewyan, south of the Great Slave Lake, where it was seen in 1894 by Mr Caspar Whitney. There is no specimen in the British Museum. *Nature* says there ought to be, and so do we.

At Danesdale, near Driffield, Yorkshire, are some 200 mounds, locally known as Danes' Graves. These have recently been excavated by Canon Greenwell, Mr J. R. Mortimer, and Mr T. Boynton, who have found remains of a chariot and various articles of iron and bronze, tending to show that the graves are of pre-Roman age, though more exact determination is at present not attempted.

General Russell Sturgis has offered New York University a site on his estate at Hamilton, Bermuda, for the establishment of a marine biological station. Prof. C. L. Bristol, Prof. W. H. Everett, Dr Tarleton H. Bean, Dr W. M. Rankin of Princeton, and three students of the University have gone to prospect and to collect.

The seventh session of the Australasian Association for the Advancement of Science is to be held at Sydney in the second week of January next, under the presidency of Prof. Liversidge. Capt. F. W. Hutton is to be president of the Geological section, Prof. T. J. Parker of the Biological, and Mr A. W. Howitt of the Ethnological.

A living specimen of *Pleurotomaria beyrichi* was obtained last March by Mr Alan Owston of Yokohama, and was examined by Prof. Mitsukuri. It appears that two lobes, one on either side of the foot, envelop the shell to some extent, and this may account for the fact that the shells of this genus are always very clean. The relations of the mantle to the slit on the outer lip could not be observed.

After descending Aconcagua, Zurbriggen and Mr Stuart Vines ascended the neighbouring Tupungato, which proved to be a volcano, 21,000 feet high. An active volcano was seen to the west.

Another large volcano, Orizaba, has recently been ascended by Mr K. T. Stoepel. Its extreme height is 18,333 feet, the length of the crater 1540, its breadth 1300, its depth 330.

With the idea that a fresh outlet may be provided for the grain-bearing provinces of Canada, an exploration of Hudson Bay is now in progress, under the command of Capt. Wakeham. Dr R. Bell and Mr Low, of the Canadian Geological Survey, will make geological and topographical surveys of the coasts and islands, while Capt. Wakeham on the *Diana* will investigate the navigability and fishing resources of the waters.

On August 10, Lord Kenyon, President of the Shropshire Horticultural Society, will unveil the statue of Darwin that has been erected by the Society at the entrance to the Public Library and Museum, the former school-buildings, of Shrewsbury. The statue, which is in bronze on a granite pedestal, is the work of Mr Horace Montford of Shrewsbury, and is not wholly unlike the fine statue in the Natural History Museum, London, though somewhat more alert in expression.

An influential meeting was held in the rooms of the Royal Geographical Society on July 5 to induce the Australasian Premiers to bring the subject of Antarctic exploration before their respective Governments. It was stated that the Society was prepared to contribute £5000 towards the amount subscribed by the Colonies. Eloquent and convincing speeches were delivered; but the Premiers were unable to be present.

Under the directorship of Dr T. Kochibe, the Geological Survey of Japan has been making good progress, and the staff has been increased. There has for some time been accumulating a collection chiefly illustrative of practical geology, and it is now proposed to build a proper geological museum in Tokyo. A short time ago some valuable phosphatic beds of Tertiary age were discovered along the north-east shore of the province of Kyushu, and Dr Tsuneto, of the Agronomic division of the Survey, has been experimenting with the material so as to make it available for the small Japanese peasant-farmers to use as manure. The organic remains in the deposit are those of marine invertebrates.

The Commissioners of the Whitechapel Public Library and Museum, in their third report, are glad to note the life infused into the museum by the Curator, Miss K. M. Hall. The average daily attendance is 275. A series of science lectures has been given free by eminent workers, and has been fully attended. Two exhibitions of spring flowers, and two of children's natural history collections, have been held. Twenty visits of classes from Elementary Schools have been made under Article 20 of the Education Code. The only thing in this report that is not satisfactory is the absence of *Natural Science* from the list of periodicals in the news-room.

Last October the Museum, Art Gallery, Public Library, and Technical Institute of Worcester, combined under the title of the Victoria Institute, were moved into a new building. It was soon found, oddly enough, that the space for the museum was less than in the old building, and it was necessary to appropriate the basement for the exhibition of the geological and ethnological specimens in spite of the little light available. This is a pity, for the local geological collection is a good one in itself, and further contains specimens of some historic interest from the collections of Hugh Strickland, W. S. Symonds, and Wynnington Ingram. Among other collections in the Museum are one of local birds, and a good one of foreign marine shells the gift of the late Sir Geo. Whitmore. The heavy task of transferring and re-arranging all the material falls on the shoulders of the curator, Mr W. H. Edwards.

THE Zoological Museum of the Royal Academy of Science, St Petersburg, has acquired thirty-three specimens of fossil bones and numerous remains of Post-Tertiary mammals collected by J. Savenkov at Krasnoyarsk. Among them are some bones and a piece of skin of *Rhinoceros tichorhinus*, which were taken from a well-preserved specimen of a rhinoceros, covered with skin, found 60 versts east of Kasatschje, on the bank of the Charaula, a left tributary of the Tomskaja.

In the *Annuaire Géologique et Minéralogique de la Russie*, vol. ii. livr. 3-4, from which this news is taken, Marie Pavlov describes, with photographs, the occurrence of a mammoth (*Elephas primigenius trogontherii*) near the town of Yaroslavl, found during the making of a railroad, at a depth of 6 metres. The remains have been sent to the Geological Museum of Moscow University.

THAT there is still something new to be found in England is constantly being shown by the active members of the Hull Scientific and Field Naturalists' Club. The last item is the Moonwort (*Botrychium lunaria*), which Mr Waterfall has seen growing wild at York. Mr Fierke, in a lecture on crabs, gave a list of those found on the Yorkshire coast, and urged members to devote a little more of their attention to the rocks and pools of the sea-shore, where, we agree with him, they will find a rich field for useful work. The programme of excursions and meetings for July-September should induce a large number to join this vigorous body, which also holds out the inducement of a new and better room over a cycle shop.

AMONG those who received Jubilee honours were: Dr Edward Frankland, Dr Huggins, Mr J. Norman Lockyer, Dr Thorne Thorne, Mr Wolfe Barry, President of the Institute of Civil Engineers, and Admiral Wharton, Hydrographer to the Admiralty, to be K.C.B.; Mr W. H. M. Christie, Astronomer Royal, to be C.B.; Sir William MacCormac, President of the Royal College of Surgeons, Dr S. Wilkes, President of the Royal College of Physicians, and Mr Thos. Smith, Surgeon-in-ordinary to Her Majesty, to be Baronets; Sir Joseph Hooker and Lieut.-General Strachey to be G.C.S.I.; Mr William Crookes, President-designate of the British Association, and Dr Gowers, to be knights; Sir Herbert E. Maxwell to be Privy Councillor.

WE have received the report of the Raffles Library and Museum, Singapore, for 1896, by Dr R. Hanitsch. A number of new cases have been introduced, and the museum has undergone entire rearrangement. There is now exhibited the beginning of a collection of the local marine fauna. A shifting of specimens has also taken place, allowing more room to the birds and reptiles, and concentrating the ethnological collections. Owing to the absence of a workshop, the exhibition rooms had to be closed while the changes were in progress. Among the acquisitions of the museum is a specimen of *Madreporaria reticulata* (?), 4 ft. 5 in. by 2 ft. 8 in., picked up on October 20, 1896, by the Cable ship *Sherard Osborne* in the Bali Straits, at fifteen fathoms, where it had grown round a cable laid in 1888, thus giving another proof of the rapid growth of corals. Lieut. Harvey, R.E., lent a boat and crew for dredging excursions on several occasions, and other dredging trips were arranged by Mr G. Holt and the Committee. A zoological station has often been proposed, and is greatly needed; a simple movable shed, with plain furniture, jars, and preserving fluids, is all that is absolutely required.

Many interesting matters are dealt with in the report of the Albany Museum, Cape of Good Hope, for 1896, to which we have already alluded in part. The alarming spread of insect pests in the Eastern province was thought to be largely due to the wholesale destruction of insectivorous birds. The protection of certain birds under an Act already existing was therefore recommended by the committee, who also suggested that saloon rifles, air-guns and catapults should be placed under the same restrictions as firearms. These proposals have been agreed to by the municipalities of Grahamstown, Port Elizabeth, Port Alfred, Uitenhage, East London, Somerset, East Cathcart, and the divisional Councils of Albany and Bathurst. The birds for which protection is desired are: Vultures, secretary bird, several hawks, especially the jackalsvogel (*Buteo jakal*) and the black-shouldered kite (*Elanus coeruleus*), owls, goatsuckers, swallows, kingfishers, hornbills, cuckoos, honeyguides, woodpeckers, barbets, thrushes (excluding fruit thrushes), warblers, sunbirds or honeysuckers, flycatchers, butcherbirds, crows (but not the rook), spreeuws (excluding redwing spreeuw *Amydrus morio*), larks, wagtails, plovers, and sandpipers. This list purposely omits rarities and game-birds.

Dr Schönland has started a small botanic garden for S. African plants, and intends to transfer these to the ground round the museum.

We are glad to see that the geological and mineralogical collections of the museum are being used for teaching purposes, since Dr Schönland lectures to those students of St Andrew's College, Grahamstown, who are studying for the first mining examination of the University of the Cape of Good Hope. This, it is hoped, will lead to more thorough geological examination of the surrounding country.

Dr Schönland has examined some peculiar rock-drawings in Bechuanaland, supposed to be the work of bushmen. They can, he says, only be looked upon as some kind of writing resembling to a certain extent early Semitic writing. An account of these, with photographs, was published in the *South African Telegraph*.

On June 3rd a second expedition to make deep borings into the coral atoll of Funafuti set sail from Sydney. Towards the expense of this, Miss Eadith Walker, of Yaralla, has contributed £500; the Government of New South Wales has lent a diamond drill; the Hon. Ralph Abercromby has furnished an oil-engine at a cost of £100; the Hon. H. C. Dangar and Prof. T. P. Anderson Stuart have provided a fine boat; the Royal Society, London, contributes £100 directly, and probably another £100 through its coral-boring committee; finally the London Missionary Society has offered to bring the party back to Sydney in September. The expedition is under the auspices of the Royal Geographical Society of Australasia, and its leader is Prof. T. W. E. David, of Sydney. He and Mr G. Sweet of Melbourne are going at their own expense, and will take charge of the borings. Mrs David accompanies them as store-keeper and botanical collector. Mr W. Poole, an engineer of Sydney University, will manage the light boring apparatus, and will be aided by Mr Woolnough, who also takes charge of the zoological collecting. These gentlemen give their services free. The large diamond drill is in charge of Mr Hall, a foreman of considerable experience, who has under him two sub-foremen and three drill-workmen. In view of the difficulties already met with at Funafuti, a special boring plant has been provided under the direction of Chief-Inspector W. H. J. Slee, and weighs over 25 tons. The main bore, on the central island of Funafuti, will be begun with a standpipe having an inside diameter of 6 inches, and the lining pipe at first is to be 5 inches inside diameter. If, at two or three hundred feet, the friction should become too great, 4-inch pipes will be lowered inside these. It is thought that the foundations of the atoll will be reached between

200 and 500 feet, but the apparatus taken permits of a depth of 1000 feet being reached. The core obtained will be forwarded first to the Royal Society of London, which will return one-half to the Royal Geographical Society of Australasia. The expedition will also make smaller borings on the sand cay in the middle of the lagoon, will conduct dredging operations for Sydney University and the Australian Museum, and will collect samples of sea-water for Prof. Liversidge to examine for gold.

Our information is gathered from an article in the *Sydney Morning Herald* of June 3, kindly sent us by Mr C. Hedley.

From the thirty-first Annual Report of the Museums and Lecture Rooms Syndicate, Cambridge University, we glean the following information. The course in Botany is still largely attended, too largely for the accommodation; plans for enlargement of the buildings are under the consideration of the Sites Syndicate. The Herbarium has received a fine collection of Pyrenean and Alpine plants made by the late Chas. Packe of Christ Church, Oxford, and presented by his widow. Large collections of Canadian and Indian species have also been received. In the Zoological museum a fine adult male skeleton of *Balaenoptera musculus*, a specimen known as the 'Pevensey whale,' has been articulated by Mr E. Lane, and supported on iron-work designed by Mr W. E. Dalby. The collection of Polyzoa has been stored in the cabinet made after the pattern described by Canon Norman in the *Report of the Museums Association* for 1895. "I am confident," says Mr Harmer, "that anyone who tries this system will be grateful to Dr Norman for its excellence." Some such method of storing was much needed owing to the large increase in the collection of Polyzoa, mainly owing to the generosity of Miss E. C. Jelly. The series is very rich in Australian species, and excellently illustrates the papers of the late P. H. MacGillivray. Another notable addition is a series of slides of the appendages of cirripedes, made by Darwin when working on his well-known monograph. Unfortunately many of the preparations have greatly deteriorated. Prof. Mitsukuri, a former student, has presented some beautiful specimens of deep-sea hexactinellid sponges. The additions to the collection of Reptilia are noteworthy, including the large cast of *Iguanodon* presented by the King of the Belgians, a fine male of the rare *Testudo elephantina*, presented by the late Lord Lilford, a skeleton of *Gavialis gangeticus* from the Jumna, the gift of Mr E. H. Hankin, and many valuable skeletons sent from Borneo by Mr C. Hose. The trustees of the late Duke of Hamilton have presented the skeleton and skin of a bull from the Cadzow herd in Hamilton Park, believed to descend from the ancient wild cattle of Great Britain. These gifts, which at present stray into the lecture-rooms, render the enlargement of the museum a matter of pressing necessity. Dr Sharp states that Mr G. D. Haviland's collection of Termitidae, already alluded to by us, is the most valuable ever formed, for it almost doubles the number of known species. The professor of Human Anatomy remarks on the increase in the anthropology classes, due to the enthusiasm of Dr A. C. Haddon. Geology also continues to increase in popularity, and the want of space under which it has so long been suffering is naturally not less felt. The chief donation is that of several of Mr Whidborne's type and figured specimens, illustrating his monograph on the Devonian fauna of S. England. The list of books presented by Rev. T. Wiltshire is printed, and includes many rarities.

In our comment on willows last month (p. 14), we regret to have overlooked the fact that the growth of stamens inside the ovary in *Salix* had already been noted by the Rev. George Henslow. In his "Origin of Floral Structures" (p. 296, Fig. 78) he figures two antheriferous carpels of *Salix*, and one example of the same arrangement in *Ranunculus auricomus*.

CORRESPONDENCE

ANATOMY OF BIRDS

It is hard to be accused of heresy by one whose orthodox faith has been disturbed by his own misreading of a very good text. Mr Pycraft (*Natural Science*, vol. x. p. 415) complains of my having described "certain membrane bones, to wit, the maxilla, premaxilla, quadrato-jugal, and jugals, as modifications of the first visceral arch," and that this statement does not tally with the analytical diagram. (Article Skull, Newton's Dictionary of Birds.) Of course it does not, because that diagram is correct, and because I did *not* include the premaxilla and the maxilla as modifications of the first visceral arch. In my copy of the Dictionary (p. 872, line 5), the words "the right and left maxillae" are separated from what follows by a semicolon, and this alters the meaning of the sentence as much as the proverbial fly's dot in Hebrew texts. In the diagram the premaxilla and the maxilla are treated as visceral arches, just as they should be, but I am so orthodox, or courteous, as to leave to the palato-quadrato-mandibular arch its time-honoured name of first visceral.

Mr Pycraft would have done better not to mention Mehnert, as all "those who have given the matter their attention" ought to know, that Mehnert's conception of the pectineal process is erroneous, and this process is one of the chief clues to the homologies of the pelvic components.

Lastly, are not the Saxon terms "greater, middle, and lesser" as good as major, medium, and minor? And if the minor wingcoverts come too near the edge of the wing they become marginals, which as such, by the way, have been mentioned in the article "Tectrices."

However, I have no reason to complain of my reviewer. He has let me off kindly, and has drawn a veil over certain real faults which I should find it difficult to explain away.

H. Gadow.

Cambridge, *June 21st*, 1897.

THE OSTRACODERMS OF PROFESSOR COPE

In reference to Professor Ray Lankester's interesting note (*supra*, pp. 45-47) on the affinities of the early Palaeozoic organisms termed Ostracodermi by Cope, I regret that no new facts of fundamental importance for the discussion of the problem have been obtained since my brief summary published in *Natural Science* for October 1892. A re-statement of the basis of Cope's view would thus be merely a repetition of the facts and comparisons contained in the literature of the subject up to that date. I should like, however, to remark that neither Professor Cope nor I have ever placed the Ostracoderms in the Marsipobranchii. In the obituary notice of Cope, I expressly referred to them as 'allies' of those animals; and they have always been mentioned as at least a distinct sub-class. The chief difference between the views of Profs. Cope and Lankester seems to be, that the latter considers the unpaired character of the nasal aperture in the Marsipobranchii of fundamental importance, while the former regards it as a secondary specialisation of no notable significance from a phylogenetic standpoint. Prof. Cope believed that at the base of the craniate vertebrata, immediately below the true fishes, there could be recognised a class of organisms destitute both of the lower jaw and of paired limbs. He termed those the Agnatha, and eventually placed among them the two distinct sub-classes of Ostracodermi and Marsipobranchii. It still seems to me that this was a great step in advance towards the true phylogenetic arrangement of the lower vertebrata, and it was this that I ventured to 'acclaim' in the sentence which led to Prof. Lankester's protest. It is well that we who are accustomed to spend so much time in deciphering the tattered relics of extinct organisms in the rocks should occasionally be checked thus in our tendency to speculation; but, notwithstanding the imperfection of our materials, it becomes continually clearer as we proceed that Palaeontology alone furnishes the criterion for estimating the relative taxonomic value of the different morphological characters of any group of organisms that happen to possess hard parts capable of fossilisation.

A. Smith Woodward.

NOTICE

To Contributors.—All Communications to be addressed to the Editor of Natural Science, at 67 St James' Street, London, S.W. Correspondence and Notes intended for any particular month should be sent in not later than the 10th of the preceding month.

NATURAL SCIENCE
A Monthly Review of Scientific Progress

No. 67—Vol. XI—SEPTEMBER 1897

NOTES AND COMMENTS

Polar Exploration

The balloon voyage of Andrée in the Arctic Regions seems to have monopolised public interest this month, and the absence of news from him has caused some very unnecessary anxiety as to his safety. Even if the winds be favourable and the balloon do not leak, he is not due on the coast of either America or Asia until well into August; and it is then quite possible that he may have to spend the winter in some northern post, from which news may not reach Europe until the spring. Several other important expeditions are in the field. Lieutenant Peary is leading another party to Greenland, where he hopes to make arrangements for his proposed Polar expedition and to secure the great block of meteoric iron which could not be removed last summer. He will be accompanied by Mr Charles Schuchert and Mr White, who hope to make large collections of the famous fossil plants of Disco Island. Sir Martin Conway and Mr Garwood are continuing the exploration of Spitzbergen by crossing the northern ice-sheet on ski. The *Windward* has again sailed for Franz Josef Land, and ought to be back in September with news of Mr Jackson's latest achievements. Meanwhile but little has been done in Antarctic research. The Belgian expedition under Dr Gerlache and M. Arctowski have at length obtained the necessary funds, and left Europe in the middle of August. The proposed German expedition is still appealing for support, but does not appear to be very warmly taken up. We hope, however, that the Royal Geographical Society will see its way to a vigorous agitation during the winter in favour of its proposed British Antarctic Expedition.

Arctic Geology

Regret at an ignorance concerning Antarctic geology is increased by two papers in *Nature*, in which Dr J. W. Gregory reminds us of the interesting problems connected with the history of the Arctic regions. In the first paper, a summary is given of the geological structure of the land masses surrounding the Arctic Ocean; the variations in the relative positions of land and water are traced, and it is argued that the Polar Basin has been formed by subsidence during Tertiary times. In the second paper, the author considers the changes in climate that have taken place in the North Polar Regions. He refers to the famous theory according to which the Arctic regions were once clothed in tropical vegetation and their shores were once fringed by coral reefs. The evidence on which this theory rests is, however, shown to be very untrustworthy. The plant determinations made by Heer are unreliable, and there is no evidence that coral reefs were ever formed within the Arctic Circle. Corals grew in Arctic seas in earlier times as they do to-day, but there has been no adequate proof that they ever formed reefs. Dr Gregory accordingly distrusts all the theories as to the great size of the sun in Palaeozoic times and the universal uniform climate in the pre-Tertiary period, which have been based on the asserted Arctic palm-groves and coral seas. That climatic changes have occurred is not disputed, but the author does not think it possible to estimate their extent until the palaeontology of the Arctic regions has been carefully revised. The most important work on this subject now being carried on is Professor Nathorst's redescription of the fossils about which Heer theorised so wildly. Dr Gregory also concludes that palaeontological evidence tells strongly against the view that the position of the Poles has altered to any great extent.

Birds and their Stomachs

The United States Department of Agriculture, knowing that the welfare of the country depends largely on the prosperity of the farming class, has undertaken for long past a proper consideration of birds in their relation to agriculture. In its fifty-fourth bulletin it deals with the stomach-contents of some twenty common birds. Among these may be mentioned the cuckoos, woodpeckers, bluejays, ricebirds, blackbirds, orioles, cedarbirds, catbirds, bluebirds, &c. There is a good deal of practical common-sense in the introduction of this pamphlet by Mr F. E. L. Beal, who points out the tendency to dwell on the harm done by birds rather than the good. He goes on to say:—

"Within certain limits, birds feed upon the kind of food that is most accessible. Thus, as a rule, insectivorous birds eat the insects that are most easily obtained, provided they do not have some peculiarly disagreeable property. It is not probable that a bird habitually passes by one kind of insect to look for another which is more appetizing, and there seems little evidence in support of the theory that the selection of food is restricted to any particular species of insect, for it is evident that a bird eats those which by its own method of seeking are most easily obtained. Thus, a ground-feeding bird eats those it finds among the dead leaves and grass; a flycatcher, watching for its prey from some vantage point, captures entirely different kinds; and the woodpecker and warbler, in the tree tops, select still others. It is thus apparent that a bird's diet is likely to be quite varied, and to differ at different seasons of the year.

"In investigating the food habits of birds, field observation can be relied on only to a limited extent, for it is not always easy to determine what a bird really eats by watching it. In order to be positive on this point, it is necessary to examine the stomach contents. When birds are suspected of doing injury to field crops or fruit trees, a few individuals should be shot and their stomachs examined. This will show unmistakably whether or not the birds are guilty."

In his notes on the tree-sparrow (*Spizella monticola*) Mr Beal shows that the stomachs of these birds in winter are crammed with the seeds of weeds, and he estimates that in the State of Iowa alone, if there are only ten birds to a square mile, no less than 875 tons of weed seed are consumed by this single species in a single season, basing his calculations on the modest estimate that each bird eats one fourth of an ounce a day for a winter season of 200 days. This may be used as an argument by the ignorant to show how much they eat of grain in the summer, but examination of stomachs of the same birds in summer shows conclusively that one third of the bulk is made up of insects (not available for consumption in the winter), grass and weed seed, and a little oats. The young birds also are largely fed on insects.

We cannot spare space to quote the statistics of other birds, but the story is much the same in each case. It is for the farmer to decide whether he cares to spare a little grain in the summer in order that his fields may be kept comparatively free of weeds from year to year, or whether he prefers to kill the birds and have his pockets emptied by paying for weeding, and the destruction of hosts of insects which are kept at bay solely by the birds he so religiously endeavours to destroy.

We have already one work on the economic ornithology of Great Britain on the lines of this bulletin ("Ornithology in relation to Agriculture and Horticulture," by various writers, edited by John Watson, 1893); but a real benefit would accrue to the farmers in enabling them to know accurately their friends and their enemies (should they care to do so), if the Government or the County Councils would take up the question officially and systematically. But such a work would involve much original research.

American Economic Entomology

We have also received from the U.S. Department of Agriculture a short but interesting pamphlet by Mr C. L. Marlott on "Insect Control in California." The well-known plan of introducing lady-bird beetles to prey upon the imported scale-insects which devastate the western fruit-orchards has been successfully extended; while an efficient artificial insecticide has been found in hydrocyanic acid gas with which the trees are fumigated after being covered with a temporary canvas tent. Mr F. H. Chittenden writes on the European Asparagus beetles which, like so many old-world insects, have been introduced into the Atlantic States. Dr L. D. Howard gives an illustrated account of various portable steam pumping-engines used for spraying trees with insecticide fluids.

Glands in Insects

In the latest part of the *Transactions* of the Entomological Society of London (1897, pp. 113-126, pt. v.), Mr Oswald H. Latter describes the structure and function of the sternal gland found in the prothorax of the caterpillar of the "Puss" moth (*Cerura vinula*). The formic acid secreted by this gland has long been recognised as a defence to the larva against its enemies. Mr Latter has now shown that at the end of larval life the secretion has another function. Mixed with the silk the acid serves to make the cocoon which contains the pupa exceedingly hard and waterproof as well as strongly adherent to foreign substances such as the chips of wood which this caterpillar habitually works into its cocoon.

Mr Latter points out that in other lepidoptera and insects of different orders, many segments of the body possess glands which may reasonably be considered serially homologous with that under consideration; he suggests that all these glands represent the coxal glands of arachnids. The prothoracic gland of *C. vinula* opens into a shallow vestibule, whence arise branched eversible tubes bearing groups of spines in their cavities. Mr Latter is unable to suggest a satisfactory function for these tubes, but he points out that the groups of spines recall the parapodial setae of chaetopods, and that the whole structure supports Mr Bernard's view that such glands are derived from the acicular gland sacs of ringed worms. Should these relationships prove to be correct, Mr Latter believes that they "will go far towards establishing the primitive nature of the cruciform larva of which many observers are already in favour."

The Photography of Microscopic Organisms in Motion

According to the *Scientific American*, the principles of the kinetoscope have been applied to the microscope with some interesting results by Dr Robert L. Watkins of New York. The instrument employed, termed a micromotoscope, has been very difficult to devise, owing to the manipulation of the light and lens. When the light is concentrated sufficiently for photography, it very quickly kills or seriously injures almost any kind of life in the microscopic field. The greater the magnification, the more intense and the nearer the lens the light must be. Difficulties are also multiplied by the length of time sometimes taken in arranging the focus on the sensitive film. After repeated efforts, however, Dr Watkins has obtained some measure of success, and motions that are not too rapid have been very satisfactorily recorded. He has been able to produce about 2500 pictures per minute. This is not a sufficiently rapid process to photograph the motion of the blood circulating in the web of a frog's foot; but it has served admirably in the case of at least one rotifer, which exhibits the most interesting form of cell motion yet reproduced.

The Great Auk in Ireland

Remains of the extinct Great Auk (*Alca impennis*) have already been recorded from the north of Ireland, but the known range of this interesting bird has just been considerably extended by the discovery of a few bones in a Kitchen Midden on the coast of Waterford, nearly as far south as 52° N. latitude (R. J. Ussher, *Irish Nat.*, vol. vi., p. 208). A humerus, tibia and metatarsus have been identified by Dr Hans Gadow and Professor Alfred Newton. They were associated with bones of common domestic animals and the red deer, and thus probably do not date back to an earlier period than the remains already found in the refuse-heaps of Caithness and Durham.

Extinct Birds of Madagascar

During his stay in Madagascar Dr Forsyth Major spent several months in the Sirabé district searching for remains of *Aepyornis*. What success attended his efforts has already been noticed in these columns, but besides *Aepyornis*, Dr Major discovered remains of numerous other birds associated with it. Mr C. W. Andrews, to whom we are indebted for the careful description of these *Aepyornis* remains, is now collecting in Christmas Island, but before setting out on this expedition he left for publication a paper which has just appeared in the *Ibis* (July), dealing with the most remarkable of these other Madagascar birds. Those here described are mainly carinate and were associated with *Ae. hildebrandti* of Burckhardt, in a marly layer indicating an old lake bottom at a depth of 12 to 15 feet. Above the marl comes a coarse gravel consolidated with carbonate of lime and containing rolled and broken bones, which may mark a volcanic outburst accompanied by hot springs charged with that mineral. Above this deposit is another of black earth from 5 to 6 feet in thickness, in which bird bones occur though rarely. It is interesting to note that *Ae. hildebrandti* does not occur in the black earth, but remains of the smaller *Ae. mulleri* were found together with well-preserved bones of *Mullerornis agilis.*

The most important of Dr Major's discoveries as described by Mr Andrews may be briefly enumerated. A large Anserine bird, having resemblances to *Chenalopex pugil*, from Lagoa Santa, Brazil. This has been called *Centrornis majori.* Another Anserine is closely allied to *Chenalopex aegyptiacus,* but the numerous slight differences between the fossil and the recent species induced Mr Andrews to term it *C. sirabensis.* He however thinks it possible that when further remains are found, it may turn out to be *Sarcidiornis mauritianus,* an extinct bird described by Newton and Gadow from Mauritius. A new rail, *Tribonyx roberti,* is described from a pelvis; while a well-preserved tibia is also referred to this species. *Ardea, Platalea, Astur,* and *Plotus,* are among the other remains discovered, but at present the material is not of a sufficient quantity to justify further description. It may be as well to note, however, that *Centrornis* is described from remains of four or five individuals; and the *Chenalopex* from a large collection of bones, many of which were found associated.

The Origin of the Edentate Mammals

The phylogeny of the edentate mammals has long been a standing puzzle to palaeontologists, and this gap in our knowledge has rendered it impossible to come to a full understanding of the South American fauna. So far as the typical or American forms (sloths, ground-sloths, ant-eaters, and armadillos) are concerned, the problem has been solved by the labours of Dr J. S. Wortman, of the American Museum of Natural History, of which an illustrated account has appeared in the *Bulletin* of the Museum (vol. ix. pp. 59-110). A valuable illustrated article on the same subject, by Prof. O. C. Marsh, has also been published in the *American Journal of Science* (vol. iii., 1897, pp. 137-146).

For many years there have been known more or less imperfect remains of certain remarkable and puzzling mammals from the Eocene of the United States, which have been described under the names of *Hemiganus*, *Psittacotherium*, *Calamodon*, and *Stylinodon;* the two first being from the Puerco beds, while the third is from the Wasatch, and the fourth from the Bridger and Wind river. The unfortunate animals to which these bones and teeth belonged have been shifted about from place to place, according to the fancy or bias of each individual describer; one of their last resting-places being among the Tillodontia.

Dr Wortman has, however, succeeded in showing that whereas in the latter it is the second incisor in each jaw which (as in the rodents) undergoes hypertrophism, in the animals forming the subject of his memoir it is the canine which undergoes special enlargement. Obviously, therefore, there can be no intimate relationship between the two groups; and as the one he has specially investigated requires a new title, the name Ganodonta has been proposed.

To enter into details of the structure of these ganodonts would obviously be out of place here. But any competent anatomist who may take the trouble to consult the excellent descriptions and figures given in the original memoir can scarcely fail to be convinced that in these animals Dr Wortman has succeeded in identifying the long-missing ancestors of the American edentates. Although the Puerco forms have enamelled and rootless molars, in the latter types the roots at first become confluent, and finally disappear, while at the same time the enamel becomes restricted to bands, and the whole structure of the tooth is simplified. The canines, too, become more and more like those of the Pliocene and Pleistocene ground-sloths; while the resemblance between the skulls and limbs of the latter and these of the ganodonts is such as to render no other conclusion possible but that the one group is the forerunner of the other. Not only, therefore, have the ancestors of the true edentates been discovered, but we have proof that the first tooth of the modern sloths is a canine, and not a premolar.

The Ganodonta are regarded as forming a sub-order of the Edentata; the genera mentioned above constituting one family (*Stylinodontidae*), while a second family (*Conoryctidae*) is made up of the genera *Conoryctes* and *Onychodectes*, to which further allusion is unnecessary in this place. Whether the living Old World families (*Orycteropodidae* and *Manidae*) should or should not be included in the Edentata, Dr Wortman leaves an open question; but in either event he confesses himself unable to draw up a satisfactory definition of the order.

The South American Edentate Mammals

Having satisfactorily demonstrated the ancestral position of the Ganodonta to the modern American Edentata, Dr Wortman goes on to observe that if this be true, "it follows that all the South American edentates must have been derived from the North American Ganodonta, since their earliest appearance in South America does not antedate the Santa Cruz epoch. In this formation they appear suddenly in great numbers and variety, without apparently any announcement in the older *Pyrotherium* deposits. This fact in itself would seem to indicate that they were migrants from another region, and while we are as yet unable to place these deposits in the time-scale with accuracy, it is yet highly probable that the Santa Cruz beds are not older than our North American Oligocene. In North America the Ganodonta appear in the very earliest Puerco deposits, and continue without interruption into the Bridger, where they disappear. No evidences of them have up to date been detected in the Uinta or White River beds.

"Now it is currently believed by geologists that no land connection existed between North and South America from the close of the Cretaceous to the close of the Miocene, when an extensive land bridge was formed. I am not familiar with the geological evidence upon which the conclusion rests, but if one is permitted to judge from the subjoined statements of Mr F. C. Nicholas, it is at the very least open to question. It is, of course, possible that the Ganodonta may have reached South America by way of Europe, Africa, and Antarctica, but on the whole it seems infinitely more probable that there was a land bridge of short duration during Eocene time between North and South America, and that they reached their destination in this way, than by the questionable and circuitous route just mentioned. If they gained entrance into South America by the European-African route, it seems indeed strange that they should have left no remains in the later Tertiaries of Europe. With the exception of a single specimen of *Calamodon Europaeus*, from deposits corresponding with the Wasatch in age, all traces of the American Edentata are absent in Europe, Asia and Africa."

To the first paragraph in this quotation no exception can be taken. With regard to the second, we have not the pleasure of being acquainted, either personally or by his writings, with Mr F. C. Nicholas, who may be a most excellent person, but the rambling extracts from a letter of his, which Dr Wortman prints in a footnote, can scarcely affect the problem of a land connection between the two Americas in early Tertiary times. Apart from this, the evidence adduced by Dr W. B. Scott and others as to the separation of North and South America cannot be overthrown by the conclusions drawn from one group of animals, more especially when an alternative route of migration will explain the facts equally well, if not indeed better.

The Old World Edentate Mammals

While it may be admitted that one swallow does not make a summer, it cannot be contended that a single tooth is not amply sufficient to prove the existence of the group of animals in the country where it was found. And as Dr Wortman expressly states that *Calamodon europaeus*—founded on a canine from the Swiss siderolithes—is a member of the Ganodonta, there is ample evidence of the existence of that group in Europe during the Eocene. Probably Dr Wortman is unaware how rare mammalian fossils are in those deposits, and why he should make a point of their absence from the later European Tertiaries passes our comprehension. With regard to Africa, no Eocene or middle Tertiaries are known, and consequently no arguments can be drawn one way or another. Moreover, it is known that when the later South American ground-sloths succeeded in entering the northern half of the New World during the Pliocene, they flourished excellently well, and if their ancestors reached the South from the North, it is difficult to see why the group should have immediately died off in the latter area.

To our own thinking it is much more probable that the Eocene Ganodonts of the northern hemisphere migrated southwards from Europe to Africa, and eventually reached South America by that route, as appears to have been the case with certain other groups of mammals. This, of course, opens up the question whether the Old World, so-called Edentates may not after all really belong to that group. Without denying the possibility of this, it may be urged that whereas the skulls and limb bones of the Ganodonta are strikingly like those of the South American edentates, those of *Manis* and *Orycteropus* are as strikingly unlike. If, therefore, they belong to the same stock, they would appear to have diverged before the Ganodonta assumed their characteristic type. But as this was acquired in the early Eocene, the Edentate origin of *Orycteropus* and *Manis* seems very problematical. At the same time we have at present no other group in which to look for the parentage of those strange creatures.

New Light on the Ova of Vertebrata

In the series of observations published by K. Mitsukuri, of Tōkyō, in the *Journal of the College of Science* of the Imperial University, Japan, we have a fresh instance of the admirable work done by certain of the Japanese morphologists. Mitsukuri's researches concern the fate of the blastopore, the relations of the primitive streak, and the formation of the posterior end of the embryo in Chelonia, together with some remarks on the nature of meroblastic ova in vertebrates. But, as is not infrequently the case, the most important results are those which receive least consideration in the title of the paper. The nature and fate of the 'yolk-plug' (or cell-mass projecting between the lips of the blastopore), which undergoes very complex changes and shiftings of position, is far more interesting than that of the blastopore itself, owing to the theoretical considerations which Mitsukuri's view of it involves. The previously-asserted homology of this cell-mass with the yolk-plug of the Amphibia, and with a similar structure observed by Van Beneden in Mammalia, is well-maintained. The necessity for a re-classification of vertebrate ova into 'primary' and 'secondary' types is clearly established, if the theory of the loss and acquisition of yolk in vertebrate eggs several times in the course of phyletic development be correct. The primitive plate and yolk-plug in Chelonia are shown to be rudiments of a large primary yolk-mass which existed in the early history of amniote eggs. The large yolk-mass seen in amniote eggs of the present day has been secondarily acquired, and the enclosure of this mass by the blastoderm is a coenogenetic process having nothing to do with gastrulation. On the other hand, the enclosure of the primary yolk-mass by the blastoderm is closely connected with gastrulation. Mammalian ova are supposed to have lost even the secondary yolk-mass. Any comparison, therefore, between the various classes of ova can only justly be made when these facts are given due weight, and they are likely to throw additional light on questions dealing with the primitive character or otherwise of various groups.

Primitive Methods of Trepanning

In *l'Anthropologie* (vol. viii., pt. ii., 1897) a most interesting account is given by Dr H. Malbot, assisted by Dr R. Verneau, on the Trepanning of the Skull by the Chaouïas of the Aurès Mountains, in the province of Constantine, Algeria. A preliminary account of these people and their country was given in the previous number of the same journal. It is a most curious fact that in a remote district in the above-named region, this people of mixed racial origin practise trepanning on an elaborate scale, and apparently maintain this practice as an heritage from ancient times. Trepanned skulls have been found in ancient cemeteries in Algeria, and prove the practice to be an old one in the region. The Chaouïas have established a great name for success in this operation, which they conduct in a manner which is characterised by great boldness, combined with decidedly rough and ready methods. The surgical equipment is of the simplest description, the principal instruments being a kind of auger (*brima*), or centre-bit rather, and two kinds of very rude saws (*menchar*) of peculiar hooked shape and very short cutting edges. The text-book, there is but one, is a manuscript, a copy of which is possessed by each qualified trepanner. The *brima* is used for exploring, and holes are drilled into the bone of the skull, at first through the outer table only, for examination of the *diploe*; but, if necessary, the hole is extended through the inner table, exposing the *dura mater*. Large portions of the skull are, if it is deemed desirable, removed through the agency of the *brima*, several holes being drilled with it very close together, and when, after some weeks, necrosis has destroyed the narrow bridges of bone between the holes, the whole piece of bone round which the holes were drilled is detached with a lever and removed. The saw is used for grave cases, and the sawed grooves are sunk to the inner table, the remaining thickness of bone being scraped away with a hooked instrument. In other cases the grooves are less deep, and necrosis does the rest of the work, the final detachment of the bone being effected as before with a lever. Prayers and incantations always accompany the operation. They must be needed! Some stubborn cases demand the trial of every class of trepanning, and at successive sittings the operator puts them all in practice; "C'est une véritable orgie de trépanation!" The most peculiar part of the whole thing is that the patient as a rule recovers, this being due rather to the natural physical qualities of the Berber race, than to the skill of the operator. Recovery may, in fact, be said to be *in spite of* the surgeon. Dr Malbot was fortunate enough to obtain a skull showing all the methods practised, a most striking specimen of which he gives a figure. The skull is now preserved in the Museum of Natural History at Paris.

This paper should be read in connection with Dr Robert Munro's paper on "Prehistoric Trepanning and Cranial Amulets," which has been lately republished in his book on "Prehistoric Problems." This gives a good and well-illustrated general account of ancient trepanning, a special reference being made to cases belonging to Neolithic times. The use of fragments of skulls as amulets is also gone into in detail, and the fact made clear that trepanning was in some cases surgical, in others posthumous, following Broca's famous memoir of 1876. Dr Munro gives a sketch of the geographical distribution of this operation, and discourses on the methods employed in early times. It is a pity that so few details regarding the practice of trepanning amongst modern primitive peoples are forthcoming, and it is greatly to be hoped that attention may be directed to this custom wherever it occurs, as the procedure observed amongst races in a low condition of culture may help to throw further light upon the archaeological aspect of the question.

SPEAR-THROWERS FROM NEW GUINEA

MR T. JENNINGS (*Proc. Linn. Soc., N.S.W.*, 1896, p. 793) has recently described in detail and figured two Papuan spear-throwers of bambu from New Guinea. These instruments have only comparatively recently been recognised as occurring in New Guinea, though numbers have now been received in the various European museums. The type is interesting for its form, which differs from that of the well-known hook-ended spear-throwers of Australia, and resembles rather that of the socket-ended examples from the Caroline and Pelew Islands, figured by Dr von Luschan. The addition of a wooden flange as a rest for the spear is peculiar to New Guinea, and the carving on these rests is often elaborate, and is varied individually, no two, probably, being quite similar. The original design in nearly all cases has apparently been some animal form grotesquely treated. The two examples described by Mr Jennings differ somewhat in detail from those figured by Dr von Luschan in his more elaborate paper on the subject, published in the *Bastian Festschrift.* Mr Jennings adds a few remarks upon the peculiar geographical distribution of these implements, but his account does not aim at being a complete one, and the distribution is pretty well known.

CYCADS

IN our last number (p. 85) we referred to some recent work by a Japanese investigator which gave additional interest to an ancient and always interesting group of plants. The Cycads are the oldest family of seed-plants. They had reached and passed their maximum (in Triassic and Jurassic ages) before the appearance of the angiospermous type which is dominant at the present day. Their habit, a simple, short stem with a crown of leaves, recalls the tree-fern much more than our dicotyledonous forest-tree with its widely branching axis and small deciduous leaves. And the discovery, of which we gave a short account last month, was only an additional evidence of the fact, recognised now for more than thirty years, that Cycads, if not a connecting link, are at any rate representatives of a type of plant-life occupying a place in the scale of evolution between ferns and those seed-plants in which the ovules are packed away in a closed ovary-chamber. Their occurrence to-day is what we should expect in a disappearing but once dominant group. There are only nine genera with about seventy-five species, but the order is widely distributed in the warmer parts of the earth, though individual genera and species have a very limited distribution. The old world has five genera, the new world four, but America possesses by far the greater number of species, Central America and Mexico being the richest areas, while Australia is the largest centre in the old world. *Cycas* (sixteen species) is the most widely-spread type, occurring in the warmer parts of Asia up to south Japan, in Australia, Polynesia and the Malagasy Islands. *Stangeria* and *Bowenia* are monotypic genera from Natal and Queensland respectively. *Dioon* has two species in Mexico; *Encephalartos*, twelve in South and tropical Africa; *Macrozamia*, fourteen in Australia. *Zamia* is the largest genus with thirty species, and is found from Peru to the West Indies and Florida; *Ceratozamia* is Mexican with six species, and *Microcycas* is a monotypic genus from Cuba. But a much larger number of fossil genera have been described, chiefly from leaves, though fruits and other reproductive organs are also known. Thus Engler in his *Pflanzenfamilien* enumerates twenty-three "more important" ones found almost exclusively in Europe, but occasionally in Greenland and Spitzbergen. Our nine genera are obviously scattered remnants of a once large and dominant family. Even individuals are isolated; except in the case of species of *Cycas* they are few and far between.

In the June number of the *Botanical Gazette*, H. J. Webber gives an account of his investigations into the structure and behaviour of the pollen-tube in a species of *Zamia*. One of his figures shows a peculiarity in the growth of the tube, which at first penetrates the nucellus for a short distance and then resumes growth at the other end, that, namely to which the grain is still attached. The important generative cell remains at the pollen-grain end in which it is carried down into the cavity above the archegonia or female organs. Webber describes two centrosome-like structures in this generative cell, the function of which is doubtful. The most interesting part of his communication is contained in a note which records the discovery, as the paper was going through the press, of motile antherozoids. As to how or where they arise, whether they are or are not in any way connected with the strange bodies in the generative cell, we are left completely in the dark, and can only hope for a continuation in our next.

The Fossils of the English Chalk Rock

Thanks to Mr Henry Woods, we have now an intelligent and careful account of the mollusca of one zone of the Cretaceous system of England. We say intelligent advisedly, because Mr Woods has not included in his lists those scraps of fossils which are considered by some authors worthy to occupy their text and their plates. The mollusca of the Chalk Rock have been described in the *Quarterly Journal of the Geological Society*, vols. lii. and liii., and comprise ten cephalopods, sixteen gasteropods, and twenty-nine lamellibranchs, and of these some seven of the first group occur in Saxony and Bohemia, two or three only of the second group, and about one half of the third. Fossils from this zone are rarely obtained in a perfect condition, and are frequently denuded of their shell, but Mr Woods has succeeded in figuring some typical specimens which will be useful to the collection. In looking through part ii. of his paper, we do not see any mention of Dover, where the Chalk Rock is easily accessible and fairly rich in fossils; indeed, in a few hours we have collected all but two of the gasteropods mentioned by Mr Hill in the *Quarterly Journal*, vol. xlii. As the energetic members of the Geologists' Association were observed in numbers at the Chalk Rock of Dover last Easter, Mr Woods might easily have obtained a list of specimens. It is gratifying to read that the author intends to proceed with the Inocerami, for they are in worse confusion than most other shells. His synonymies of *Lima hoperi* and *Spondylus spinosus* are interesting and important. Mr Woods will forgive us perhaps if we point out to him that Salvius did not print the molluscan part of Linné's edition xii. until 1767, and therefore the date cannot be 1766; but why not use the tenth edition, 1758, now almost universally recognized?

A list of other remains identified is supplied, and discussions on the distribution and relations of the fauna and conditions under which the Chalk Rock was deposited are given. The whole is a useful and valuable paper which will be largely in request.

Tierra del Fuego

In September last year we were favoured by Dr Ohlin with an account of the zoological results of Baron Oscar Dickson's expedition to Tierra del Fuego. A preliminary notice of the geographical results of that expedition is now published in the *Scottish Geographical Magazine* for August. The country consists of a woodless tableland in the north, and a mountainous district in the south, the latter being the extreme continuation of the Cordilleras. The boundary between the two zones is almost a straight line. The northern country is stated to be of Tertiary age, covered partly by gravel and partly by moraine.

JOHANNES JAPETUS SMITH STEENSTRUP

Born March 8, 1813; died June 20, 1897

I

Steenstrup

JOHANNES JAPETUS SMITH STEENSTRUP was born on March 8, 1813, in the northern part of Jutland, in the district termed Thy, where his father was a parson. In the year 1832 he was sent from the cathedral-school of his native province at Aalborg to be a student at the University of Copenhagen. In two succeeding years (1833-35) he was obliged to remain in the paternal home, occupied with teaching his younger brothers and with natural history excursions into his native country, collecting numerous examples of its interesting natural productions, its plants and animals, its fossils and geological features. Of scientific facilities or aids he had very little; a copy of the published parts of the celebrated "Flora Danica," of Linné's "Systema Naturae," of O. M. Müller's "Prodromus zoologiae danicae," were, I believe, almost the only books of science available for Steenstrup in these early times of his scientific self-training and self-education; his only helper at this time being a gifted parson, his uncle, formerly a pupil, especially in botany and entomology, of the renowned naturalist and teacher Melchior, at the college of Herlufsholm. After his return to the university in 1835, in the full bloom of a self-made young naturalist, he became the pupil and friend of Schouw, the botanist, of Forchammer, the geologist, and of Reinhardt, sen., the zoologist, whose ingenious lectures left an impression on Steenstrup's mind never to be effaced. Among the particular friends of those days of his youth were the gifted botanist Drejer, lost at an early age, Liebmann, Schouw's successor as Professor of Botany after his return from Mexico, Reinhardt, jun., the celebrated zoologist and traveller in Brazil, etc. Only two years after his return to the university Steenstrup earned the honours for two prize essays, the one (never published, only epitomised in my text-book, "Dyreriget," and therefrom in Palmen's work on the migrations of birds), "on the differences between the wanderings of birds and fishes," the other, published afterwards (1842) in the *Transactions* of the Danish Academy of Science, "on the geological investigation of certain forest-moors on Seeland," etc.—a work of great sagacity and acute observation, the first to elucidate the sequence of the different forest-vegetations characteristic of the early periods in the recovery of our country after its emergence from the waves and the close of what is now termed "the Ice-Age"—the ages of the aspen (*Populus tremula*), of the fir (*Pinus sylvestris*), the oak (*Quercus sessiliflora*), and ultimately the alder (*Alnus glutinosa*) and the beech (*Fagus sylvatica*). Much time was destined to elapse before these studies of the gifted rising naturalist, so important for the history of the life of our globe, were taken up elsewhere in other northern regions.

In the year 1839-40 Steenstrup was sent by the Government, with an Icelandic student, Hallgrimson, and Mr Schytte, afterwards Professor of Chemistry in Chile and Governor at Puntas Arenas on the Straits of Magellan, to Iceland for an economic investigation of this country. The story of this voyage, though interesting to those who have had the good fortune to hear Steenstrup's reminiscences of the country, its nature and its people, was never published; nor was his interesting and fruitful discovery that the so-called "Surturbrand" in the Tertiary trap formation of Iceland contained a series of remains of an arboreal vegetation, with its tulip-trees (*Liriodendron*), etc., most resembling that of certain subtropical regions, until his Icelandic materials and figures were placed in the hands of Oswald Heer. Steenstrup's investigations on the volcanic formation of Iceland have been taken up by younger minds, who have no doubt been much benefited by the information that was in Steenstrup's possession. Another discovery made by Steenstrup on this trip to Iceland happened during some sunny days near the coast of Norway, viz., the discovery of the metamorphosis of crabs (*Hyas araneus*) and soldier crabs (*Pagurus bernhardus*), at a period when metamorphosis in Crustacea was very little known, and therefore was received by excellent zoologists with considerable doubt. Steenstrup's letters to Reinhardt on this subject were printed in the *Proceedings* of the Royal Danish Academy in 1870. The specimens collected were also sent down to his teacher. They were seen here by Rathke, who described them together with the material collected by himself. The history of the metamorphosis in the higher Crustacea now occupies an extensive literature; among the first pioneers in this important chapter Steenstrup's name must have its place.

Shortly after his return from Iceland in 1841 Steenstrup was appointed lecturer in Botany and Mineralogy (geology) in the Academy of Soroe in Seeland, the only place in Denmark where Natural History and a knowledge of modern languages had its rightful place among the classical lines of study. Here he remained until 1846, occupied especially with two of his best known works, published as programmes of the college, viz., "The Alternations of

Generations" (1842), and "On the Existence of Hermaphroditism in Nature" (1845). The first of these is too well known to need any explanation here; it can only be wondered that this doctrine so wonderful at the time of its publication has been so little modified in its essential points and lost or gained so little in extension since that time. Space forbids me to enlarge further on this topic, which more than any other of Steenstrup's writings has spread his name and fame over the whole civilised world. His second work, that "On Hermaphroditism," was less successful, though its subject was in intimate connection with lines of thought resulting from or connected with "metagenesis," as it is now generally termed. One may admire the author's acuteness of perception and the extent of his comparative studies, and confess that he quite rightly abolished many cases of unfounded hermaphroditism among inferior animals; but it must be allowed that hermaphroditism is still fully recognised, with few exceptions, among leeches, flukes, tapeworms, pulmonate and opisthobranch snails, barnacles, etc. (Tardigrada have lately been thrown off), without our being able to give an adequate natural reason for its presence in some tribes and its absence in others. In recent times Steenstrup's memoir has awakened the important remark, that in all probability hermaphroditism has not been the primordial rule in any division of higher or lower zoological rank, but must be a "later acquisition" in the course of evolution, for which no satisfactory reason can yet be given. While speaking still of Steenstrup's residence at Soroe, it should be mentioned that here he pursued, in the course of other faunistic studies, an examination into the specific duplicity of our common frogs (*Rana temporaria*) and the distinctive characters of what he termed *R. oxyrhinus* and *R. platyrhinus*, which have played a rather important part in the recent study of the Anourous Batrachia.

In 1846, after the death of Reinhardt, sen., Steenstrup was nominated to replace him in the chair of Zoology at the University of Copenhagen, and as Director of its modest zoological collection. He was a beloved and admired teacher for the students of medicine and for the pupils of the polytechnic school, and at the same time the gifted chief leader of the "Natural History Society." A member of the Royal Society of Science from 1842, he was its secretary after Forchammer's death until after years of great activity he gave up this post in 1878. The election to the Presidency after Madvig's death he declined, as he had more than once declined the Rectorship of the University, being anxious not to be drawn too much away from his scientific studies and his professorial duties. In 1848 he was with Forchammer placed at the head of the "Royal Natural History Museum," with the recommendation of the Minister of Education to promote its union with the University Museums—a proposal which naturally enough interested Steenstrup much, but met with some opposition, not only from the University, which reasonably feared the increased pecuniary obligations involved in such a scheme, but also from the majority of the keepers at the Royal Museum. At last the battle was won by the bill of 1863, which ordered the construction of a much wanted building in the grounds of the University. It was finished and opened to the public and to Science in 1870, and has since been the handsome, but perhaps not sufficiently large home of zoological science with us, constructed by the gifted architect, Chr. Hansen, whose genius was, I believe, strongly fertilised by Steenstrup's ideas. Steenstrup was not, as originally planned, the sole director of the new museum, but by the election of the University the president of its council, consisting of two keepers (inspectors), Schödte and Reinhardt, and himself as administrators of its different departments. I shall not here speak of the difficulties and painful controversies connected with this organisation. Steenstrup retired from his position as Professor of Zoology in the year 1885, after a painful period, rendered more distressing through an unfortunate accident (a fracture of collum femoris). I shall confine myself to a short resumé of his chief scientific work from 1846 to 1885, the years of his professorship.

It was one of Steenstrup's characteristic features that he was not only an excellent zoologist and a specialist in some of its branches, but also a good geologist and botanist, capable of discussing many topics relating to different sciences; and it may be said, that he had a certain predilection for those points of science, where its different sections meet and intercross. It will therefore be easily understood that a man with his abilities and constitution of mind must play an important part in a large scientific community. It is, of course, a difficult task to classify his works, which can often be referred to more than one of the related sciences, and whose value may be judged from different points of view. It will be understood that while his humble successor in the chair of Zoology since 1885 may think himself entitled to judge of his purely zoological work, he must speak somewhat more discreetly, notwithstanding the partly natural historical character of Steenstrup's archaeological and related publications, on this part of his literary work, and leave the ultimate judgment to his historical and archaeological colleagues.

One of Steenstrup's great services was, that he induced—what was then a rarity—some of our excellent seafaring men of the navy or of the merchant line, to devote their leisure hours to collecting the animals of the seas through which they sailed, making careful notes of the localities examined with their nets, and in this manner furnishing the museum with pelagic and other material from almost every part of the ocean. Several parts of this "Plankton" have since been worked upon by his pupils and others (*e.g.*, Boas on the Pteropoda, Traustedt on the Salpae, Lütken on the Dolphins and the "hemi-metamorphoses of fishes," Bovallius on the Hyperidae, etc.). With this series of studies may also be reckoned the memoir of Steenstrup and myself on the parasitic Entomostraca of the ocean with several other forms of the same tribe ; also the former's anti-critical note on the genera *Silenium*, *Lesteira* and *Pegasimallus*, and his papers (too numerous to be enumerated here) on Cephalopoda (*Notae teuthologicae*, etc.) in the *Transactions* and *Proceedings* of the Academy of Science, in the *Videnskabelige Meddelelser fra den naturhistoriske Forening*, and elsewhere in popular journals. I shall dwell, however, more particularly only on two points. Firstly, there is his surprising demonstration that the apparently abnormal development of one or occasionally two arms in male cuttlefishes, hitherto overlooked or not understood, was in fact the homologue of the well-known "hectocotyle" in the pelagic Octopoda. His eager desire to throw the light of his genius and of his science on obscure problems, led him also to investigate the tale of the wonderful sea-monk, the monster that was cast ashore in our vicinity in the sixteenth century, described and figured by Belon, Rondelet and Gesner, and playing an important part in the semi-mystic Natural History of the Renaissance. Nobody had been able to decipher this enigmatical monster until Steenstrup deprived it of its fabulous investment, demonstrated it to be simply a decapod giant cuttlefish. Specimens of this same kind (*Architeuthus*) have been thrown on our shore, formerly and later on the shores of Iceland, Faroe, Jutland, Newfoundland and Japan, and happily one of our captains did find such an animal floating in the Atlantic, and secured to Steenstrup some of its most important parts. Steenstrup's full account of these remains was partly in print, though never completely published; but some of his plates have been placed in the hands of his fellow-zoologists. To the other purely zoological articles of Steenstrup, I shall only allude briefly, namely, to those on *Sphenopus* (*Sabella marsupialis* Gm.), on *Philichthys*, *Rhizochilus*, *Xenobanalus*, *Pachybdella* and *Peltogaster*, on the enigmatical objects correctly interpreted as the "gillrakers" of *Selachus maximus*, on the natural systematic place of the walrus, etc. His interpretation of the wandering of the eye in young flounders has not been accepted with unanimity, but still has some trustworthy points to fall back upon. Our common memoir on the *Mola*-tribe (*Orthagoriscus*) and its larval stages, has not been published in its complete form ; perhaps it may be so still. Several palaeontological papers on mammals, birds and reptiles (turtles) found in our peat-beds or other formations have appeared at least in abstract, as also some account of the refuse-heaps or shell-heaps ("kitchen-middens") of our shores, whose correct interpretation was the work of Steenstrup, and has obtained world-wide notice and given birth to many investigations in other countries. Some papers on Helmintha (*e.g.*, *Fasciola intestinalis*) should not be forgotten. His interpretation of the Brachiopoda as not belonging to the true Acephala has in later time become popular; his interpretation of the partly operculated "Palaeozoic corals" (Cyathophyllidae) as not being Anthozoa-proper, but something else, perhaps allied to Serpolidae, Hippuritidae or Brachiopoda, has met with decided doubt and opposition, but in my opinion, not found its final decision. In the study of the newer (Glacial) geological formations, Steenstrup took an active part with his lamented younger friend and colleague, Johnstrup, and some of his later papers briefly give his views on some of the theories advanced in Scandinavia concerning this important part of Scandinavian geology. That he with Nathorst, the Swedish botanist-geologist, partook in the discovery of the earliest glacial plant remains in our Scandinavian peat-bogs, should also be remembered. He was present at most of the meetings of the Scandinavian naturalists during the period of his scientific activity and has left the marks of his influence in the reports of the meetings of the Americanists and Archaeologists at Copenhagen and Brussels. Beyond his travels in Iceland and his visit to the Faroe Islands, he made several journeys to Germany, France, Northern Italy, Switzerland, Dalmatia, and England, enriching his knowledge and adding to his acquaintance and friendly relation with eminent men of many countries and many sciences. His scientific correspondence would fill many volumes.

Already in the earlier part of his career, Steenstrup had published some papers of a chiefly historical aim, throwing light on obscure phenomena, elucidated from a naturalist's point of view (*e.g.*, on the so-called "havgaerdinger," on Ottar's relation to King Alfred on his travels in northern seas, and on the passage of King Harold through the Limfjord). After he had retired from his zoological professorship in 1885, he treated with great emphasis several archaeological problems of the same character (*e.g.*, the Haellristningar, the voyages of the Zeni, the Yak-Lungta-Brakteats, the silverplates found at Gundestrup, the mammoth station at Predmost in Bohemia. Most of them have been published, though not the first. I shall not do more than point out the existence and interest of these remarkable papers, not being competent to pass a scientific judgment upon them; but, at the same time, I would express my conviction, that they will remain through all future time a testimony of the great insight, sagacity and knowledge of my gifted friend and teacher, the glory of his country, of Europe, and of his century. That he was honoured with the highest distinctions from his country and from many other sources, I need not tell, nor enumerate the learned societies (Stockholm, Christiania, Berlin, Paris, London, etc.) of which he was a member. This sketch of Steenstrup's life and work may appear longer than usual to the readers of this journal, but it is not long or detailed enough to do justice to what ought to have been said. I cannot conclude without naming his wife, Ida (*née* Kaarsberg), the love of his youth, lost several years before her husband died. Several children died earlier or later; one daughter is left, and there is one son, Johannes Steenstrup, Doctor in Law and Professor Rostgaardianus in History at our University, whose work on the history of the Normans will especially be known to many English readers.

CHR. FR. LÜTKEN.

II

Does Natural Selection play any part in the Origin of Species among Plants?

INTRODUCTION.—The objects of the present paper are to answer this question in the negative, and to prove that natural selection is a superfluous factor as an aid in the origination of new varietal characters; though it has much to do with the "survival of the fittest" in "the struggle for existence" among beings in any particular locality. It is, of course, the Darwinian conception that these factors are somehow concerned in the origin of species; but I would maintain that they must be kept totally distinct from it. Darwin, in truth, insisted upon this fact himself; that whatever the causes or origins of variations might be, such were questions with which natural selection had nothing whatever to do. His words are:—"The direct action of the conditions of life . . . is a totally distinct consideration from the effects of natural selection . . . [it] has no relation whatever to the primary cause of any modification of structure."[1] What I wish to show is that sufficient variations to constitute a variety are always the result of a direct or indirect response to the "definite action" of a new environment; indeed many, if not all the organisms, of whatever kind they may be, which are subjected to it, often vary more or less in a like manner.[2] It will then be seen at once that not only are there no "indefinite variations" for natural selection to deal with, but as a consequence its *raison d'être*, as an aid in the origin of species is gone; and it can take no part in the origination of varieties.

I wish also to point out that Darwin's theory of natural selection rests entirely upon a series of *à priori* assumptions or deductions, which have never been verified; nor, indeed, do they seem capable of verification.

Definition of a Species.—In order to be clear, it is desirable to state precisely what one understands by the term "Species." According to the method pursued by systematic botanists in describing plants, a species may be defined as follows:—"Any particular species of a genus is known by a collection of characters taken from any or all parts of the plant. These characters are, or

[1] "Animals and Plants under Domestication," vol. ii., p. 272.

[2] Hence arises the *facies* characteristic of aquatic, desert, alpine, and other plants: as I have described in my work—"The Origin of Plant Structures."

are theoretically assumed to be, constant." One or more of these characters may be found on another species, which in a similar manner is known by its collection of constant characters.

What may have been their origin, and how the survival and maintenance of any superficial characters of a plant have been secured, are philosophical questions with which the systematist has no concern at all.

Useless Characters.—Before showing that the hypothesis of natural selection is superfluous in the origination of varietal characters, let us turn to the descriptions of plants given in some standard work, say, Sir J. D. Hooker's "Students' Flora of the British Isles." It will be found that many characters are taken as specific or generic which cannot, with any show of reason, be regarded as specifically useful; such as the numerical excess or deficiency in the number of parts in the floral whorls; *e.g., Gentiana campestris* is described as having the calyx "four-partite"; while in *G. amarella*, it is "five-lobed"; but fours, fives and sixes may be often found on one and the same plant, as in a corymb of elder flowers, due to an accidental deficiency or excess of nutriment, respectively; and no vital importance can be attributed to the trivial specific distinction between "partite" and "lobed." Such illustrations of quite unimportant characters regarded as specific can be multiplied to any extent; but they are some of the very characters which Darwin admits are not due to natural selection. He says:—"We thus see that with plants many morphological changes may be attributed to the laws of growth and interaction of parts, independently of natural selection."[1] They are, in fact, simply the inevitable results of a response to environmental conditions, using the term in the broadest sense.

With regard to such indifferent characters being hereditary, Darwin first says that he "felt great difficulty in understanding the origin or formation of parts of little importance; almost as great, though of a different kind, as in the case of the most perfect and complex organs,"[2] and he devotes a section to a theoretical interpretation of them. Indeed he, on several occasions, recognises the existence of useless characters; *e.g.*, he says, "I am inclined to suspect that we see, at least in some of the polymorphic genera, variations which are of no service or disservice to the species; and, consequently, have not been seized on and rendered definite by natural selection."[3] In this passage the word "disservice" almost seems as if he had a suspicion that "injurious" characters might sometimes be present, though he elsewhere says:—"Any actually injurious deviations in their structure would, of course, have been

[1] "Origin of Species," 6th ed., p. 175; see also p. 367.
[2] "Origin, etc.," p. 156. [3] "Origin, etc.," p. 35.

checked by natural selection." Secondly, the following are Darwin's words with reference to the inheritance of characters which are no longer useful:—"No doubt the definite action of changed conditions . . . have all produced an effect, probably a great effect, independently of any advantage thus gained. . . . I fully admit that many structures are now of no direct use to their possessors, and may never have been of any use to their progenitors." . . . He mentions the webbed feet of upland geese, etc. . . . "With these important exceptions, we may conclude that the structure of every living creature either now is, or was formerly, of some direct or indirect use to its possessor,"[1] He would thus include all rudimentary organs as having been formerly useful, but now useless; of these he remarks that rudimentary organs from being useless are not regulated by natural selection, and hence are variable. If they be so in the animal kingdom, they are not so in the vegetable; *e.g.*, the staminodes and rudiments of ovaries of flowers are constant in form to each species, genus or order which is characterised by them, respectively; as, *e.g.*, *Erodium*, *Samolus*, *Mercurialis*, *Parietaria*, *Valerianeae*, *Myrsineae*, etc., and are recognised as permanent diagnostic characters.

Injurious Characters.—In many flowers there have been acquired and retained by heredity, what may be called by Darwin's term "disservice," or even "injurious" characters. For if, *e.g.*, the use of flowers be to set good seed, then anything which tends to hinder that process is obviously injurious. Such occurs in the structure of the flowers of most orchids, and in many adaptations to insect fertilisation, as dichogamy, protandry, polymorphism, etc., whenever they tend to bar self-fertilisation.

For it need hardly be observed now, that Darwin's assumption from the numerous adaptations in flowers for intercrossing by insects, that self-fertilisation was "injurious," was based on a quite erroneous deduction altogether. The fact being that in nature autogamous, or self-fertilised plants, are by far the most prolific, perfectly healthy, most abundant in individuals, and most widely dispersed.[2]

On the other hand, all special adaptations to secure self-fertilisation are obviously useful, are quite as numerous and excellent in the adjustment of the organs, as are those for intercrossing.[3]

Now it is worth while observing that the result of such injurious

[1] "Origin, etc.," p. 160.

[2] The reader is referred (should he require it) to the writer's papers on "Self-fertilisation," *Trans. Linn. Soc.*, 1877; Review of Darwin's "Cross and Self-fertilisation of Flowers" in "Gardener's Chronicle" (1877); and "The Origin of Plant Structures."

[3] See Kerner & Oliver's "Natural History of Plants;" "Autogamy," vol. ii., p. 331, ff.

features may even be the actual extinction of a species; for it is conceivable that if a plant cannot set seed by self-fertilisation, and is not crossed by insects or the wind, it will die out, if it be an annual or not propagated by its vegetative system. It will thus be eliminated by natural selection.[1] But the process falls within the subject of the distribution of species, both in time and space, and has nothing whatever to do with the origination of such harmful structures; which, as long as they exist, are regarded as specific or generic characters.

The survival of the fittest, therefore, and the destruction of the least fit and incapable to survive, are questions altogether independent of the Origination of Structural Variations, upon which the survival, or destruction in some cases, may actually depend. The reader must constantly bear in mind Darwin's words which I again quote, because of their importance: "The direct action of the conditions of life . . . is a totally distinct consideration from the effects of natural selection . . . [it] has no relation whatever to the primary cause of any modification of structure."[2] This last is the sole matter with which I am concerned.

Individual Differences.—These according to Darwin[3] and Dr Wallace are the chief materials for natural selection to act upon. As I have already fully discussed this subject in *Natural Science*[4] and pointed out that as a rule they are quite incapable of giving rise to varietal characters which a systematist would take note of, I need say no more than invite the reader's careful attention to my article.

I might, however, briefly point out a fallacy in Dr Wallace's conclusion. He has given numerous tables in his work, "Darwinism," and argues that any excess in dimension of an organ from the mean is eliminated by natural selection; so that a species keeps its dimensions pretty constant, annually.[5] But no intimation is given as to how great a deviation, in excess or deficiency of the mean, is required to prove destructive to the creature itself. Experience, however, shows that both nanism and gigantism are common phenomena in nature among plants; in which the customary deviations are vastly exceeded. Moreover they can be induced to arise under cultivation coupled with perfect health, fertility and heredity. Therefore, the whole of this argument falls to the ground.

Supposed Requirements of Natural Selection in the Formation of New Varieties.—The primary condition assumed by Darwin and Dr Wallace is a large population. In order to produce a new variety these writers tell us that "in the great majority of cases a new species arises amidst the population of an existing species."

1 "Origin, etc.," p. 57.
2 "Animals and Plants under Domestication," vol. ii., p. 272.
3 "Origin, etc.," p. 34.
4 Vol. vi., p. 385 (1895).
5 *Journ. Linn. Soc.* (*Zool.*), vol. xxv., p. 483.

"The greatest danger," writes Dr Wallace, "to a species under new and adverse conditions is, that it should not be able to adapt itself to them with sufficient rapidity. It is for this reason that, as Darwin concludes, new species arise from those which have a large population, which occupy a wide area, and which present much variation, a combination . . . rarely found except in continental areas."[1] How far is this hypothesis borne out by facts? As a matter of fact the majority of species of a country have not a large population, nevertheless many of such species have varied as much as, if not more, indeed, than the more gregarious species with large populations; thus, it is easy to think of plants, of which large populations exist, generally gregarious, and therefore supplying the primary condition supposed to be requisite for natural selection; but the remarkable feature about them is that they have never been known to vary! Thus, Sir J. D. Hooker gives no varieties whatever to any of the following species, *Ranunculus ficaria*, *Caltha palustris*, *Lychnis diurna*, *Erica cinerea*, *Bellis perennis*, *Urtica dioica*, *Galium verum*, *Scilla nutans*, *Lemna minor*, *Pteris aquilina*, &c. And when we cross the channel (though England is really or physically part of the Continent) we find no more signs of variation there, whether in France, Germany, Switzerland, the Tyrol, &c.

On the other hand, take an extremely common plant, *Polygonum aviculare*; though abundant, it is scarcely a social plant, at least, to the extent of those mentioned. It produces several varieties, but are they found in the midst of the commonest, say, the roadside type? Sir J. D. Hooker says:—"*Var. P. littorale* (littoral) a passage to *P. maritimum* (maritime); *Var. agrestinum* (field form); *Var. arenastrum* (sand-loving form); *Var. rurivagum* (wayside form);" hence these varieties are not found in the midst of the commonest form, but away from it, in localities characterised by special physical features. In other words, these varieties arise by self-adaptation to their special environments, respectively.

The second condition requisite for variations consists of "changed conditions of life." Both Darwin and Dr Wallace admit that "a change of climate and food" is requisite for a new variety to arise among the parent type. The latter writer says:—"Now let some important change occur, either in climate, in abundance of food, or by the irruption of some new and hitherto unknown enemies, a change which at first injuriously affects the species."[2] Similarly Darwin writes:—"Let the external conditions of the country alter," and again, "take the case of a country undergoing some . . . change."[3]

The question at once arises, where and when do we find these changes occurring in, or coming to, any particular district, where some species with a large population happens to be? Is nature

[1] *Loc. cit.*, p. 484. [2] *Loc. cit.*, p. 483. [3] "Origin, etc.," p. 63.

dependent upon geological catastrophes for producing variations in plants and animals? Indeed, this would seem to be Darwin's view in his discussion on geologic time, in which he says:—"It is probable, as Sir W. Thomson insists, that the world at a very early period was subjected to more rapid and violent changes in its physical conditions than those now occurring; and such changes would have tended to induce changes at a corresponding rate in the organisms which then existed."[1]

But when we find that one species will change into another recognised species under our very eyes, if its environment be altered, why need one appeal to millions of years for aid? Dr Wallace, *e.g.*, notes how "*Arabis anchoretica* has tissue-papery leaves due to its growth in hollows in the rock. Seeds of this plant when cultivated at Kew produced the common species *A. alpina*. The same thing occurs with many plants as every cultivator knows."[2]

Darwin and Dr Wallace agree in requiring "rapid adaptation," but Darwin admits "that natural selection generally acts with extreme slowness."[3]

Now, if nature has to wait for catastrophes before some "changed conditions of life" come to her organisms, is not this something like trying to bring the mountain to Mahomet, instead of letting Mahomet walk to the mountain? Which is easier to do, to let plants and animals migrate to a place with a different climate and abundance or deficiency of altered food, rather than imagine the latter to come to them?

Migration is so obvious a process that Darwin cannot help alluding to it, as when he says:—"Among animals which unite for each birth and are highly locomotive, doubtful forms ranked by one zoologist as a species and by another as a variety, can rarely be found within the same country, but are common in separated areas."[4] They have not, therefore, arisen at one common spot.

A new climate and abundance of food are often supplied by domestication and cultivation, and the anticipated results follow, *viz.*, variation *ad libitum*, the consequences also being often hereditary as they are in nature.

Acquired Characters are Hereditary in Plants.—Dr Wallace writes:—"Climate and Food undoubtedly produce modifications in the individual, but it has not yet been proved that the modifications are hereditary. If this could be proved the whole discussion on the heredity of acquired characters would be settled in the affirmative."[5] But surely cultivation proves it every day? Our garden vegetables are all derived from wild plants, and they come true by seed.

[1] "Origin, etc.," p. 286. [2] *Natural Science*, vol. v. p. 182. [3] "Origin, etc.," p. 84.
[4] "Origin, etc.," p. 37. [5] "Darwinism," p. 489.

What more do we want to prove that acquired characters are hereditary? I do not understand what he means when he says:—"In every case these changes can be interpreted as . . . adaptations or individual, non-hereditary modifications in the case of plants."[1] That garden races are adaptations to their environment is obvious, and to say that they cannot be hereditary is, as it seems to me, to shut one's eyes gratuitously to the most conspicuous facts. The "Student" Parsnip was "fixed" in five years, *i.e.*, from 1847 to 1852, having been raised by Professor J. Buckman from seed of the wild plant, and it is still pronounced to be "the best in the trade"; its acquired characters have been, therefore, relatively fixed for half a century, though the plant's variability may never cease to exist, because no so-called "fixed race" is absolutely stable. Hence we constantly hear of Mr A's improved race of Mr B's pea, bean, or what not. Nevertheless, that the typical garden form is always reproduced, and that its sub-varieties or races come relatively true by seed, is all that is wanted to establish the truth of acquired characters being hereditary in plants.

Migration, essential.—With regard to the origin and fixation of varieties in nature a closer observation shows that, as a rule, contrary to the Darwinian view, new varieties of plants have not arisen among the parent types, but away from them. Thus, Sir J. D. Hooker, who in his knowledge of the geographical distribution of plants is *facile princeps*, says:—"As a general rule the best marked varieties occur on the confines of the geographical area which a species inhabits."[2] Darwin also quotes A. de Candolle's opinion that "plants which have very wide ranges generally present varieties; and this might have been expected (he writes), as they are exposed to diverse physical conditions."[3] Precisely so; but then this is due to migration together with adaptation to the new physical environments; for the "diverse physical conditions" do not come to the plants where the large populations have been supposed to grow. It is interesting to see that both Darwin and Dr Wallace, after asserting the importance of large populations among which new varieties are said to arise, are compelled by facts to admit precisely the contrary. Thus, both Dr Wallace and Darwin observe that the struggle for existence will be "most severe between individuals of the same species; for they frequent the same districts, require the same food, and are exposed to the same dangers." Such is the condition said to be required for natural selection; but now, on the contrary, he tells us, "as an effect of this principle [?] we seldom find closely allied species of animals or plants living together, but often in distinct though adjacent districts where the conditions

[1] *Loc. cit.*, p. 490. [2] "Introductory Essay to the Flora of Tasmania," p. v.
[3] "Origin, etc.," p. 43.

of life are somewhat different." If so, and this statement is quite in accordance with Sir J. D. Hooker's view already quoted, how could the varieties have arisen in the midst of the plant type? Similarly, Darwin says that mountain breeds always differ from lowland breeds; and "a mountainous country would probably affect the hind limbs from exercising them more, and possibly even the form of the pelvis," &c. What is all this but the formation of new varietal structures by a response to the direct or definite action of the environment? But, then, it is obvious from Darwin's remarks that the mountain breeds are not supposed to have arisen among the lowland forms or *vice versâ*; just as the submerged forms of *Ranunculus* could not have arisen among land buttercups or *vice versâ*. Consequently Darwin could not shut his eyes to the fact that "isolation is an important element in the modification of species." [1] Again, he says:—"Migration and isolation are necessary elements for the formation of new species." [2]

On the other hand, Dr Wallace says:—"Physical isolation, I believe with Darwin [?], to be of comparatively little importance, and to have very rarely been the chief agent in modification."

If migration and isolation, which are only to be secured on the confines of the geographical area of a species, as Sir J. D. Hooker says, are so important, then it becomes obvious that the centre of the parent population is not the place, as a rule, to look for the origin of a new variety, but as far away from it as possible. From this it follows that the less struggle for existence there be with the parent type, the better it is for the origination of new varieties; and it is best of all where there is no struggle at all.

Dr Wallace enquired of two experienced British botanists if there "are any cases of well-marked varieties, which occupy a considerable area to the exclusion of the parent species, and do not occupy any area, or only a very small one with the type." [3] One example of a *Rubus* was given him; but a more important question, however, as it seems to me, would be:—Is a sub-species or variety usually found within the area occupied by a large number of the parent type? Take, *e.g.*, *Hieracium*, a most variable genus; of this Sir J. D. Hooker writes:—"Variable as the genus is, the sequence of its forms is so natural as to have been recognised by all botanists. This sequence represents to a considerable extent the spread of the forms in altitude and area in the British Isles." [4] Now *Hieracium* is not a genus with gregarious species; for though the sub-species and varieties are very many, the relative quantity of each is not particularly great anywhere; and thus, so far from lending any

[1] "Origin, etc.," p. 81.
[2] "Origin, etc.," p. 82.
[3] *Loc. cit.*, p. 494.
[4] 'Students' Flora," p. 232.

countenance to Darwin's idea, that a species must have a numerous population to produce varieties, the rule seems rather to be that these two features do not necessarily coincide at all.

Supposed Aids to Natural Selection.—In order to secure the survival of the fittest, *i.e.*, a new variety among the parent form, it was perceived that two additional and hypothetical aids were necessary, *viz.*, (1) some degree of infertility with the parent, and (2) a rapidity of adaptation.

With regard to the first, all experience goes to prove that it does not exist; for when cultivators wish to fix a new race, as of cabbage, &c., they are obliged to grow it as far as possible away from the parent stock. Indeed, considering how freely species can be hybridised, the probability of an offspring refusing to be crossed by the same species is very small or nil. Neither Darwin nor Dr Wallace bring forward any examples of infertility with the parent among plants.

Secondly, a rapidity of adaptation is claimed hypothetically. This does often really exist, but it is a little uncertain whether these authors were aware of it. For when a plant finds itself in a new and markedly different environment, which strongly affects it, it then grows by self-adaptation in response to the new external influences: as when passing from water to land, or *vice versâ*; from the wild state to the artificial soil of a garden; from lowlands to alpine or subarctic localities, &c., as I have shown in "The Origin of Plant Structures."

The Persistence or non-retention of new varietal characters.—To come to what Dr Wallace regarded as the most important point in his paper. Four times does he mention it, only slightly altering the expression, *e.g.*, he says:—"No attempt has been made to show, even hypothetically, how, through the action of known causes, such characters [useless ones], when they do arise, can become first extended to every individual of a species, and then be totally obliterated as regards any portion of the species which may become modified so as to constitute a new species. Useful characters thus strictly limited are the necessary and logical results of modification through survival of the fittest. No agency has been shown to exist capable of producing useless characters similarly limited."[1] As illustrations to meet Dr Wallace's demand, it may be observed that the races of cultivated pears are spineless; yet they are derived from the wild *Pyrus communis*, which has useless abortive branches as spines. Similarly is it the case with some varieties of plums derived from *Prunus communis*.

With regard to the retention of injurious characters, the

[1] *Loc. cit.*, p. 491.

obstruction to self-fertilisation produced by the rostellum is common in orchids, and generally occurs in all the species of any particular genus. Yet it is obliterated in *Phajus blumei*, *Chysis aurea*, species of *Chrysoglossum*, *Arundinia speciosa*, and *Eria flavescens*, &c., so that these species set plenty of good seed by self-fertilisation, whereas 40,000 blossoms of *Dendrobium speciosum* set one pod. I have already had occasion to allude to the rudimentary organs of *Mercurialis*, *Erodium*, &c., which are retained in all the species alike.

Is Dr Wallace, therefore, justified in making the above assertion at all, or in demanding that either useful or useless characters should be limited? Why should either one or the other be obliterated when a new variation arises? The fact that a genus, which is the result of sufficient variation in a species (unless it be monotypic), does retain both useful and useless characters in some, many, or all of its species, shows that there are no grounds for his statement. Natural selection may demand it, but nature utterly refuses to be obedient to that theory.

Too great stress is laid upon a necessary fixity, as a proof of specific characters, by many writers. This is purely a relative matter. Cultivation has been suggested as a test of a species; but this is the very best means of inducing a wild plant to vary, as all cultivators know. The fixation of any variation is a matter of time. About five years may, perhaps, be regarded as the average period under cultivation in "fixing" races: but nothing is known about wild varieties. In either case the rule is that the environment must be constant.

Indefinite Variations, non-existent.—This is the second hypothetical source of new variations according to Darwinians.

With regard to all the offspring varying approximately alike and not "indiscriminately" (Romanes) or "indefinitely" (Darwin) when subjected to changed conditions of life, I wish to emphasise the fact most strongly that experiments show conclusively that if seedlings are subjected to a markedly different environment, when they grow up to maturity, the rule is, that all that do change, change in precisely the same way. They do not vary indefinitely among themselves; so that there is no material here—any more than with "individual differences"—for natural selection to act upon. Thus, in cultivating the wild parsnip or carrot, all the seedlings that change, do so by beginning to assume the same new characters—*viz.*, an increased size with a greater fleshiness in the root, larger dimensions of the leaves, reduction of hair, &c., with a corresponding alteration in the anatomical structures.

So, too, if the seeds of an amphibious plant as *Ranunculus heterophyllus* be sown in a garden border, all grow absolutely alike in adaptation to the aërial medium. Numerous other illustrations could be given.[1]

I think it must be from not being aware of the abundance of evidence of this sort, that the idea arose that all the offspring did not acquire the same characters when the external conditions were changed.

Dr Wallace doubts it because, he says, "the argument is, that the same causes will always produce the same or closely similar results. But this is only true when the same causes act upon identical materials and under identical conditions."[2] Dr Wallace is mistaken in supposing that nature pays any attention to "individual differences" which occur between any number of plants of the same kind. It is not a question of argument, but of facts. These differences are of no moment whatever when self-adaptation is required to take place. The external influences cause all the individuals to change alike in the same direction, and utterly ignore the various dimensions among the "individual differences" described above. The result is that the same facies is acquired by all the individuals, though a new set of individual differences may readily be found among the individuals of the new variety.

Secondly, besides doubting the occurrence of the same definite variations in the whole of the offspring subjected to new but similar external conditions, Dr Wallace adds:—"It must do more than this, for it must produce a variation so exceptionally stable that it constantly recurs in all the offspring of successive generations, even though those offsprings are subjected to considerable change of conditions."[3]

But the stability of a species, I repeat, is a purely relative matter and depends upon time. Some plants are very plastic, others are not so, some variations may become very (but never absolutely) rigidly fixed, while others may refuse to be reproduced by seed with any certainty at all. Not only is this true when the plant is propagated by seed but it is also true for vegetative multiplication. Tulips, &c., introduced from the East, though they have presumably been constant in form for unknown ages, yet often become unrecognisable in three years though propagated by bulbils only; apple trees, though propagated by grafts alone have given rise to numerous varieties; even different kinds of apples raised on stocks, but grown in the same States of N. America, respectively, often bear fruit of approximately the same form.[4] On the other hand the Jerusalem artichoke, asparagus, sea-kale and celery offer

[1] The reader is again referred to "The Origin of Plant Structures" for further details.

[2] *Loc. cit.*, p. 488.

[3] *Loc. cit.*, p. 489.

[4] "Bud Variation and Evolution," *Natural Science*, vol. vii., p. 103. An essay in Mr Bailey's work "The Survival of the Unlike," 1896.

but little variations to select from. Of common vegetables, parsnips, carrots, radishes, *Brassica oleracea*, &c., have supplied numerous varieties which come true by seed; though each may still furnish an improved "race."

Similarly, if a useless character be acquired among cultivated plants, not only may it occur in every individual but it may become hereditary and relatively fixed; just as in the examples of wild plants already mentioned. Thus, there is no special advantage in the mere variety of colouring of flowers as of pansies, nor in double flowers, nor in excess of neuter flowers of composites, nor in the abortive pedicels of the feather hyacinth, &c.

With regard to the fixation of characters, therefore, there is no absolute rule whatever, nor can we say why one plant is so plastic and another refractory.

Nature recognises no "must" in her processes.[1]

Darwinism, an Unverified and Unverifiable Deduction.—It is a common statement that Darwin placed the Doctrine of Evolution on a scientific basis when he pronounced the theory of "The Origin of Species by means of Natural Selection." It is against this statement that I would venture to protest most strongly. To take the latest example, Ludwig von Graff says:—"The selection theory of the celebrated Englishman, Darwin, first based the idea upon a scientific foundation. The obvious phenomena of heredity and of variability are the foundations of his bold system, the axles of life's mechanism; and the motive power of this mechanism is the struggle of all living things for the preservation and procreation of life."[2]

Darwin's theory, however, as stated in the title of his book, "The Origin of Species by means of Natural Selection," is a pure deduction; and deductions (*i.e.*, *à priori* reasoning), though useful as working hypotheses are not scientific or useless, until they have been verified by induction and experiments.

The theory was based on two primary deductions; out of these secondary ones followed. They were, first, that "Individual Differences" could supply materials for natural selection to act upon; secondly, that when offspring of any species varied under the action of new conditions of life, they generally varied indefinitely, so affording fresh material for natural selection. It has been shown that both of these fundamental assumptions are groundless.

As an illustration of his deductive method of reasoning, let us take the following typical passage which states Darwin's theory clearly and concisely:—

[1] Dr Weismann says:—"Doubt is the parent of progress;" yet in about a page and a half of *Nature* (June 11, 1896), in an epitome of his theory, he uses the word "Must" fourteen times!

[2] *Natural Science*, vol. ix., p. 193.

"It may metaphorically be said that natural selection is daily and hourly scrutinising, throughout the world, the slightest variations; rejecting those that are bad, preserving and adding up all that are good."[1] This, as far as the origin of species is concerned, is a pure assumption; and what I contend for is, that since observation and experiment show conclusively that variations can arise rapidly under one's very eyes, there is no need to assume any other process whatever than the protoplasmic response to environments. Thus, rhizomes are often recognised as being of specific or other diagnostic value, but when an aërial stem is made to grow underground, its new growth at once begins to assume the characters of an ordinary rhizome. Roots, stems and leaves normally living submerged have characters which are at once more or less assumed by a terrestrial plant if it be made to grow in water, and *vice versâ*; or if a water plant send a shoot into the air the change is abrupt at the level of the water. Plants in damp places are often very different as a whole from those in excessively dry situations. Reverse their positions and each at once begins to assume the characters of the other as soon as they grow in response to their surroundings. If lowland plants or their seeds be grown in high alpine regions they at once assume the facies of normal alpine plants. The markedly peculiar features of desert plants at once begin to break down, when a normally desert plant is grown in ordinary soil, just as the wild carrot or parsnip may quickly acquire the characteristic features of the cultivated form.

If Darwin had fully realised the significance of these and such like facts, he could hardly have continued the above passage with the following words:—"We see nothing of these slow changes in progress, until the hand of time has marked the lapse of ages; and then so imperfect is our view into long past geological ages, that we see only that the forms of life are now different from what they formerly were."[2] That all this is due to natural selection is simply an unverified deduction.

Self-adaptation, by Response to the Definite Action of Changed Conditions of Life, the True Origin of Species.—That plants vary by self-adaptation to a new environment is proved by inductive evidence and amply verified by experiment.

Let me repeat.—The struggle for life is incessant. Apart from ill-luck, which applies to all alike, the weaker in constitution are often expunged, while the stronger survive and the general distribution of plants in time and space is the result. This however, as Darwin insisted, is a quite different thing from the origin of species.

The origin of species is due, for the most part, or as a broad

[1] "Origin, etc.," p. 65.

[2] "Origin, etc.," p. 66.

general rule: first, to migration and isolation from the parent type, with as much freedom from the struggle for existence as possible; secondly, to self-adaptation by the inherent power of response in living protoplasm, excited by the physical influences of the new environment. The result is for the most part new structures in harmony with the new environment. If there be a thousand seedlings of one and the same plant which germinate and grow together, they will all put on, more or less, the same features under the same definite action of the same surroundings; though individual differences will still be found among them as before.

Conclusion.—Lastly, the answer to the question which heads this paper is that natural selection plays no part in originating new varieties, nor is it required as "means" or an aid in the origin of species; but is all-sufficient in the distribution of plants.

Now the above conclusion is practically admitted by Dr Wallace himself, in the following sentence:—"Should they [fixed varieties of plants] be found to occur more frequently in other countries [*i.e.*, 'Representative plants,' which are indeed innumerable] as varieties of birds, mammals, and reptiles, &c., occur in separate areas in North America — they may be usually explained as adaptations to very different climatic conditions, in which case the distinguishing characters will be utilitarian [or otherwise] and the local varieties will be really incipient species." The passage I have spaced represents precisely the views expressed in this paper. Darwin, too, admits the possibility of the origin of species without the aid of natural selection. His words are as follows:—"By the term definite action, I mean an action of such a nature that, when many individuals of the same variety are exposed during several generations to any change in their physical conditions of life, all, or nearly all the individuals are modified in the same manner. A new sub-variety would thus be produced without the aid of natural selection." [1]

Lastly, this was the conclusion of Mr Herbert Spencer, in 1852, seven years before Darwin and Dr Wallace superadded natural selection as an aid in the origin of species. He saw no necessity for anything beyond the natural power of change with adaptation; and I venture now to add my own testimony, based upon upwards of a quarter of a century's observations and experiments, which have convinced me that Mr Spencer was right and Darwin was wrong. His words are as follows:—"The supporters of the development hypothesis can show . . . that any existing species, animal or vegetable, when placed under conditions different from its previous ones, immediately begins to undergo certain changes of structure fitting it for the new conditions . . . that in the

[1] "Animals and Plants under Domestication," vol. ii., p. 271.

successive generations these changes continue until ultimately the new conditions become the natural ones. . . . They can show that throughout all organic nature there is at work a modifying influence of the kind they assign as the causes of specific differences; an influence which, though slow in its action, does in time, if the circumstances demand it, produce marked changes." [1]

All, therefore, I ask of my readers is to weigh well the evidence that has been again of late years brought forward in favour of adaptation in lieu of natural selection as the means by which varieties originate; and not to be biassed by, it may be, many years of conviction that Darwinism was all-sufficient. It is solely a question of evidence, and as the doctrine of evolution ultimately broke down men's faith in Creation by "Fiats" and the Argument of Design, so it is hoped that before this century closes, it will be seen that Darwin's deduction of "The Origin of Species by Means of Natural Selection" was a most unfortunate one, as it is quite incapable of verification; while the conclusion of Mr Herbert Spencer has been abundantly verified, both by inductive evidence and experimental proof. GEORGE HENSLOW.

[1] "Essay on The Development Hypothesis," 1852.

III

Reproductive Divergence: An Additional Factor in Evolution

SOME ten years ago the late G. J. Romanes propounded his theory of Physiological Selection,[1] which was founded on the fact that certain individuals of a species, though fertile with some, may be perfectly sterile with other individuals. Supposing such incompatibility to run through a whole race, then these varieties, separated by a physiological barrier from the rest of the members of the species, would be preserved, and might vary independently, and so become gradually split off from the parent species in respect of other characteristics as well.

This theory has not been generally received, and Wallace, in particular, has demonstrated[2] very clearly that in the form propounded by its author the theory cannot stand. Nevertheless, the theory served to draw attention to the importance of variations in the reproductive powers of organisms as a factor in evolution, and to emphasise certain unexplained difficulties in the theory of natural selection, more especially with reference to the sterility of first crosses between species, coupled with the fertility of those between varieties, the swamping effects of intercrossing, and the frequent inutility of specific characters.

In the present paper I wish to bring forward a theory which is also concerned with variations in the reproductive powers of organisms as an important factor in evolution, but which is essentially different from that propounded by Romanes. This theory may be enunciated as follows. Supposing that among the members of any species, those individuals, more alike, in respect of any characteristic, such as colour, form or size, are slightly more fertile *inter se* than less similar individuals, it necessarily follows that in the course of succeeding generations the members of this species will diverge more and more in respect of the characteristic in question, whereby ultimately the original species may be split up into two or more fresh species.

This principle I have ventured to call "Reproductive Divergence." It is best illustrated by a concrete example. Supposing

[1] *Journ. Linn. Soc.* (*Zool.*), vol. xix., p. 337, 1886.
[2] "Darwinism," p. 180.

that in the Lepidopterous *Ithania urolina*, an insect found in the Amazon valley, small individuals were slightly more fertile with other small individuals than with larger individuals, whilst these were also more fertile *inter se*, then it would follow that fewer individuals of intermediate size would be produced, and in course of time the species would be split up into a small and a large variety. These varieties would continue to diverge as long as the principle of "reproductive divergence" was acting, till at length they might become differentiated into two mutually sterile species. Supposing, on the other hand, this variation in fertility were correlated with slight differences of colour, then in course of time varieties differing in respect of colour would be produced, or if it were correlated with both size and colour, varieties differing in respect of both characteristics might be produced. As a matter of fact, this insect does actually occur as four distinct varieties, differing in colour, form and size,[1] though whether in consequence of the operation of reproductive divergence, it is of course impossible to say.

It will be observed that the theory enunciated is made up of two parts, the first of which can only be verified by experiment, whilst the second is the statement of a fact, which is capable of mathematical demonstration. This we will now proceed to afford.

Let a certain number of individuals of a species, say 900 males and 900 females, be divided up into three groups, according to their size. Let there be 300 small males, *S*, 300 medium sized ones, *M*, and 300 large ones, *L*. Let the 900 females be similarly divided up into the three groups, *s*, *m* and *l*. In order to maintain the number of individuals constant in each generation, let it be granted that any number of males and females breeding together give rise to the same number of males and females. Then if these 900 males and females be allowed to breed together, on an average 100 small males, *S*, will breed with 100 small females, *s*, and 100 male and female offspring, *Ss*, will arise. Similarly also there will be 100 male and female offspring, *Sm*, and 100 *Sl*. Again, with reference to the medium sized males, there will be 100 male and female, *Ms*, *Mm*, and *Ml* offspring; and with reference to the large sized males, 100 male and female, *Ls*, *Lm* and *Ll* offspring. Now let it be granted that the offspring *Sl* and *Ls* are of the same size as *Mm*, and that *Sm* and *Lm* are respectively of the same size as *Ms* and *Ml*. Then as the result of the chance breeding of the 900 males and females, we shall have the following numbers of individuals of each sex formed:—

100 *Ss*, 200 *Sm*, 300 *Mm*, 200 *Ml*, 100 *Ll*.

Now let us suppose that the comparative fertility of the various sized individuals is slightly changed, so that the principle of "repro-

[1] H. W. Bates, *Trans. Linn. Soc.*, 1862, p. 545.

ductive divergence" may come into operation. Let 100 individuals breeding with similar sized individuals, give birth to 120 offspring of either sex instead of 100, whilst 100 individuals breeding with moderately smaller or larger individuals (*i.e.*, *M* and *m* breeding with *s* and *S* or *l* and *L*) give birth to, on an average, only 95 offspring, and 100 individuals breeding with considerably smaller or larger individuals (*i.e.*, *S* or *L* breeding with *l* or *s*) give birth to only 80 offspring of either sex. Then it will be found that the 900 males and females breeding together will give birth to the following:—

120 *Ss*, 190 *Sm*, 280 *Mm*, 190 *Ml*, 120 *Ll*.

That is to say, whilst the largest and smallest individuals have increased in numbers by 20 per cent., the medium sized ones have decreased by 7 per cent., and the ones intermediate between these by 5 per cent. The fact that the medium sized individuals have decreased in number, in spite of the 100 *M* and *m* individuals which breed together having produced 120 *Mm* offspring, is of course due to the fact that only 160 *Mm* individuals are produced by the crossing of the 100 *S* and 100 *L* individuals with the 100 *l* and *s*.

In a similar manner, in succeeding generations, the numbers of individuals intermediate in size will gradually become smaller and smaller, whilst those of the extreme ones will increase. But, it may be said, even then the two varieties thus formed will not differ in size to a greater extent than the extreme individuals in the original 1800 taken. This is not the case. Thus supposing the three groups of individuals were respectively on an average 65·5, 68·5 and 71·5 inches in length, the extremes among the small individuals being 64 and 67 inches, those amongst the medium 67 and 70 inches, and those among the large 70 and 73 inches. Then suppose that by the principle of reproductive divergence the individuals were separated into two groups of an average of 64 and 73 inches in length. Then it follows that these groups would (approximately) contain individuals varying between 62·5 and 65·5 inches, and 71·5 and 74·5 inches respectively. That is to say, considerably smaller and considerably larger individuals would be formed than were originally present. Also if the principle of reproductive divergence continues to act amongst the two varieties of the original species formed, the individuals will continue diverging more and more in respect of this characteristic, with which the reproductive power of the organism is correlated. Also, if it be granted, that on an average, the more widely any two individuals differ in size, the greater is the relative degree of sterility between them, it follows that in course of time the individuals of the two varieties will become mutually sterile: or in other words, that from the original species two new species will have arisen.

Having demonstrated the correctness of the second part of the hypothesis enunciated, it remains to bring forward experimental evidence of the validity of the first part—*i.e.*, it is necessary to prove that in some cases more closely similar individuals of a species show greater mutual fertility than less similar; in other words, that there may be a partial sterility between varieties. On this point Darwin has collected a considerable amount of evidence in his "Variation of Animals and Plants under Domestication."[1] A few of the cases mentioned there may be now cited. Thus Gärtner found that a variety of dwarf maize, bearing yellow seed, showed a considerably diminished fertility with a tall maize having red seed, though both varieties were perfectly fertile when crossed *inter se*. Again, in the genus *Verbascum*, numerous experiments were made by Gärtner with the white and yellow varieties of *V. lychnitis* and *V. blattaria*, he finding that crosses between similarly coloured flowers yielded more seed than those between dissimilarly coloured flowers. These experiments have been repeated and extended by Scott with confirmatory results. Again, Girou de Buzareingues crossed three varieties of the gourd, and concluded that their mutual fertilisation is less easy in proportion to the difference which they present. Still again, the blue and red varieties of pimpernel, which are considered by most botanists as varieties, were found by Gärtner to be quite sterile when crossed.

With regard to members of the animal kingdom, there is very little evidence. Such as there is, is related only to domesticated animals, and can be at once objected to on the ground that it merely shows that the animals in question are descended from two or more distinct species. Thus Youatt[2] states that longhorn and shorthorn cattle, when crossed, show a diminished fertility. This statement has, however, been denied by Wilkinson.

The evidence determinable from certain anthropological data is, on the other hand, of more value. Thus Professor Broca has brought forward evidence[3] that some races of man show diminished fertility together. Again, according to statistics collected in Prussia from 1875 to 1890, it was found that Protestants, Catholics and Jews, marrying among themselves, had, on an average, respectively 4·35, 5·24 and 4·21 children. When, however, the husband was a Jew and the wife a Protestant or Catholic, the numbers of children were only 1·58 and 1·38 respectively; and when the wife was a Jewess and the husband a Protestant or Catholic, only 1·78 and 1·66 respectively. Whether this apparent partial sterility was due to differences of race or to social reasons it was impossible to say.[4] Still again, from the natality tables of Körosi,[5] which are calculated

[1] 2nd ed., vol. ii. p. 82.

[2] "Cattle," p. 202.

[3] "On the Phenomena of Hybridity in the Genus *Homo*." 1864.

[4] Quoted from Mayo Smith's "Statistics of Sociology," p. 115.

[5] "Phil. Trans.," 1895, B. 781.

from the marriage statistics of 81,000 couples in Buda-Pesth, it is possible to obtain evidence supporting our theory. Thus from these figures one may see that parents of similar ages are more fertile *inter se* than parents of dissimilar ages. With very young mothers the most fertile fathers are, on an average, from three to six years in advance as to age; but with increasing years of the mothers the ages of the fathers become less and less in excess, till at about thirty years of age they coincide. At greater ages they gradually become slightly in defect. Though this greater mutual fertility of individuals like in respect of age can be of no influence in modifying the species, or splitting it up into varieties, yet it gives us reasonable ground to suppose that the fertility may also be found on examination to be greater with individuals similar in respect of some other characteristic. In such a case there would be a tendency for two or more varieties to be formed, unless there were some other agency counteracting it.

It will be seen that the evidence adduced in favour of a partial sterility sometimes existing between varieties of a species is, in the case of animals at least, very meagre. The reason of this is not far to seek. Thus wild animals, when placed in confinement, will not, in the majority of cases, breed at all. Domesticated animals, on the other hand, do not afford evidence of much value, for the reason given above. Also, it is generally held that domestication of itself tends to increase fertility, and so would overcome any tendency to sterility of varieties.

In order to obtain evidence as to the existence of a diminished fertility between varieties, I have made a considerable number of observations on the effects of crossing the various colour varieties of the sea urchins, *Sphaerechinus granularis* and *Strongylocentrotus lividus*, and have found that from a given number of ova the number of blastulae and the number of larvae subsequently produced are appreciably smaller for crosses of dissimilar colour varieties than for those of similar ones. Also, the larvae produced are, on an average, about 5 per cent. smaller. As, however, it will be necessary to repeat these observations a large number of times before the proof of such a partial sterility can be considered quite unexceptionable, and as moreover I hope to be able to make similar series of observations among other classes of the Animal Kingdom, it would be premature at this point to refer to these investigations at any greater length.

It should be borne in mind that the theory of Reproductive Divergence does not require that there should be a partial sterility between the varieties of species in all cases, or in even the majority of cases. It merely premises that such sterility does exist in certain instances, and that in these the members of the species will gradually become more and more divergent in respect of one or more characteristics, unless of course other causes are at work counteracting its influence. Probably in the majority of cases, in most stable species in fact, there is no such variability of fertility between slightly differing individuals, and hence there is, from this cause, no tendency to the formation of more or less distinct varieties. Very probably, however, there is a latent possibility of such a variability of reproductive power arising in almost any species, when for instance some of its members are exposed to fresh environmental conditions, in consequence of migration or change of climate. If this is so, then a species will tend to split up into varieties just at the most opportune moment, the varieties thus formed becoming by the action of Natural Selection gradually more and more adapted to their surroundings, and so fresh species produced. That change of environmental conditions has a very great influence on the reproductive powers of both animals and plants is a well-known fact, and one on which Darwin has collected much valuable evidence.[1]

It now remains to be demonstrated how the theory of reproductive divergence can successfully account for some of the chief objections which have been brought against the theory of Natural Selection, objections indeed which have been of considerable weight in deciding many scientists against the doctrine of the all-sufficiency of Natural Selection as the cause of Evolution.

The fact of the very general infertility of crosses between species and their hybrid offspring, coupled with that of the fertility of crosses between varieties, and of their mongrel offspring, was recognised by Darwin as a formidable objection. Though this distinction between species and varieties is now recognised as not of such universality as it was formerly believed to be, yet it is still admitted to be a difficulty hitherto by no means adequately accounted for. The theory of reproductive divergence offers a most satisfactory and convincing explanation. Thus according to it, as we have seen, varieties and ultimately new species have, in many cases at least, been formed by the operation of a slight and accumulating sterility between unlike individuals, whereby two or more groups of individuals become more and more segregated, and so capable of undergoing independent variation. This divergence of species takes place quite independently of Natural Selection, but this principle can always be exerting its action at the same time, whereby the new or modified characteristics produced can, if useful to the species, be accumulated and rendered better adapted to the environmental conditions. Whether the very general sterility of crosses between species is due originally in most or in all cases to reproductive divergence, or whether it came into operation but seldom, it is not as yet possible to say. If extended series of experiments show that it is in fairly

[1] "Variation of Animals and Plants under Domestication," vol. ii., pp. 130-149.

frequent operation in those species having a tendency to split up into varieties, it may be concluded that it was, and is, an extremely important factor in the production of sterility of crosses between species. Thus, as has already been mentioned, it is not supposed that reproductive divergence comes into effect in fixed and stable species, but only in those which, probably in consequence of changes of conditions of environment, are in the course of splitting up into varieties and new species.

Connected with the fact of the general mutual fertility of varieties, is that of the swamping effects of intercrossing. Thus if varieties are perfectly fertile with the parent form, it is difficult to see how they can ever establish themselves as incipient species, unless they become separated from the parent form by a geographical or other barrier. If, however, these varieties have arisen in consequence of the operation of reproductive divergence, it is obvious that they can preserve their characteristics unobliterated, and continue to exist in the same region as the parent form.

One of the most important objections to the doctrine of the all-sufficiency of Natural Selection as a cause of evolution, is that of the very frequent inutility of specific characters. Some naturalists, especially Wallace, are inclined to maintain that all specific characters are of use, and that it is only due to our ignorance that they appear to us useless. It is a more generally received opinion, on the other hand, that these characters can frequently be of no useful purpose to the organism, and must therefore have originated by some other means than Natural Selection. Darwin himself was fully alive to this objection, and considered that such useless specific characters might owe their origin to the correlation of organs, or to the laws of growth, and to so-called spontaneous variability.[1] These seem but very inefficient causes for such frequently occurring effects, and hence there is a strong *prima facie* evidence that some other principle is at work. The principle of reproductive divergence offers a satisfactory solution of the problem. Thus, as we have seen, by means of it species are caused to diverge in respect of one or more characteristics, and so fresh or altered characteristics can be originated without the influence of Natural Selection. To take a concrete instance, one mentioned by Bateson.[2] The commonest forms of lady-birds are the small *Coccinella decempunctata*, and the larger *C. septempunctata*. The small insect is very variable in colour and the pattern of its colours, whilst the large is almost absolutely constant in these respects. This difference in specific characters may have originated by a common parent form having had its colour marking, and also the size of the individuals to a slight extent corre-

[1] "Origin of Species," 6th ed., p. 171.
[2] "Materials for the Study of Variation," p. 572.

lated with reproductive power. After the splitting up of this parent form into a large and a small species, in each of which the colour marking was invariable, the variations in fertility in the larger form, as correlated with colour marking, may have ceased, owing perhaps to the conditions of environment having changed from a variable to a more constant state, and the species would now become constant in this respect. The smaller form, on the other hand, may still be in the course of splitting up into two or more other species, differing in respect of colour marking, and maybe, of other characteristics.

Another not fully explained question with regard to the origin of species is that of the divergence of character. Why is it that in the course of evolution, species have widened out into diverse branches, and have not continued in merely linear series? This question of divergence has been examined somewhat fully by Gulick.[1] Darwin seeks to answer the question "from the simple circumstance that the more diversified the descendants from any one species become in structure, constitution, and habits, by so much will they be better enabled to seize on many and widely diversified places in the economy of nature, and so be enabled to increase in numbers."[2] As Romanes points out,[3] this argument is, however, assailable in one particular, *i.e.*, it ignores the fact of the swamping effects of intercrossing. Thus, in Darwin's own words, it is where specific forms "jostle each other most closely" in an overstocked area that Natural Selection will be enabled to act most favourably on any members which may depart from the common type. Now, any varieties formed under these conditions by the splitting up of a species will be almost inevitably swamped by their mutual intercrossing, unless there be some degree of sterility between them. Under these conditions, therefore, reproductive divergence can act at a great advantage, as not only can it originate varieties, but by the mere fact of so doing it ensures these varieties not being eliminated by the swamping effects of their mutual intercrossing.

It is unnecessary on this occasion to show how the theory of reproductive divergence may be applied to the other questions and difficulties connected with the theory of Natural Selection as an explanation of the mechanism of the origin of species. Suffice it to say that to some points in connection with Geographical Distribution, with the origin of rudimentary organs and other questions, it offers most material aid. The objections to the theory itself, as far as they present themselves to me, seem to be but few, and of but little weight. One of the most obvious is the frequently made statement, that crosses between varieties generally produce indi-

[1] *Journ. Linn. Soc.* (Zool.), vol. xx., p. 189.
[2] "The Origin of Species," p. 87.
[3] *Loc. cit.*, p. 385.

viduals of greater vigour and fitness than the parents. As far as I am aware, there is no evidence to show that this greater vigour is the result of the differences of morphological form, but rather that it is due to the individuals being descended from different stocks, whereby the evil effects of in-and-in breeding are avoided, or to being exposed to differences of environmental conditions, whereby they may perhaps be rendered physiologically unlike individuals to a slight degree, rather than morphologically unlike. That mere exposure to differences of environmental conditions may be sufficient to give rise to a vigorous race even when this is propagated by the closest in-and-in breeding, is shown by the case of the rabbits on the Island of Porto Santo, all of which are descended from a single pregnant individual.

Another objection which might be raised is, that in the case of both plants and animals it has frequently been found that varieties showing considerable differences of external form are perfectly fertile *inter se.* Even if this is the case, it is no argument against the theory of reproductive divergence, for it was specially mentioned that this is not supposed to be invariably in operation when a species is in course of splitting up into varieties. At the same time, it may reasonably be doubted whether this statement as to the perfect fertility of varieties is a fact, because a very slight degree of sterility would easily escape notice unless extensive series of breeding experiments were made, and careful records kept.

H. M. VERNON.

IV

On the Restoration of some Extinct Reptiles

THE exhibition of large diagrams in museum cases has met with the disapproval of many who are in a position to give an authoritative opinion; but, by way of justification of such a practice, it may be pointed out that it frequently happens in a museum that, since it is only possible in rare instances to have cases specially made to accommodate definite series of specimens, spaces will occur which are a source of much trouble to the curators; and diagrams, from their elasticity of size, can always be relied upon to fill what must otherwise be left blank. It is just such a difficulty that has to be confronted in planning out some of the cases at the Natural History Museum. The wall-cases, for instance, on the south side of the third Bay on the left-hand side of the Entrance Hall are devoted to the elucidation of the more important features which are made use of in the classification of reptiles, and contain stuffed specimens, casts, and skeletons, articulated and disarticulated, of representative members of each order. But the cases are ten feet in height, and the upper compartments are too far removed from the eye of the observer, and too badly lighted, to admit of the recognition of much detail in the specimens exhibited there. The framework of the back of the case, also, is too slight to bear heavy specimens, and it is here, if anywhere, that the exhibition of wall-diagrams is justified.

As complete skeletons of extinct reptiles of such a size as to fit conveniently into these wall-cases without crowding out the recent members of the class, or being lost among them by reason of their diminutive size, are almost impossible to obtain; and as the disjointed parts of the skeleton of these extinct forms are efficiently represented either by actual specimens or by casts in the table-case, it was, when recently planning out this wall-case, considered sufficient for the purposes of the Index Collection to represent the Ornithosauria, the Ichthyopterygia, the Sauropterygia, and the Anomodontia by bold diagrams of the whole skeleton of one selected species of each, drawn to such a size as to fill the four top spaces. The diagrams, which have now been completed and are exhibited in the cases, measure about 27 inches in height, and 41 inches in breadth. They are bold outline diagrams, in black lines on a white ground, executed by Miss G. M. Woodward with the artistic skill and excellence of technique which invariably characterise her work.

The species chosen to represent the Order Ornithosauria is *Dimorphodon macronyx*, from the Lower Lias of Lyme Regis (Fig 1). Owen's well-known restoration of this species ("Liassic Reptilia," *Mon. Pal. Soc.*, 1870, pl. 20., and "Hist. Brit. Foss. Rept.," 1849-84, vol. iv., pl. 17), naturally formed the basis of the diagram, but the shapes and proportions of the bones were taken from the actual specimens, of which the Geological Department of the museum can boast a good many.[1] The correctness of Owen's restoration of the pelvis was severely criticised by Seeley in 1891

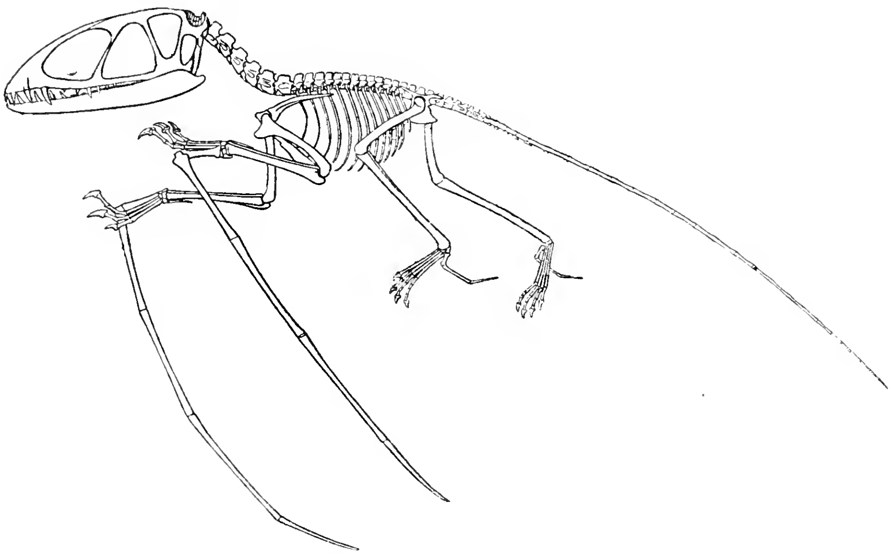


Fig. 1. *Dimorphodon macronyx*, from the Lower Lias of Lyme Regis. (x$\frac{1}{5}$).

(*Ann. and Mag. Nat. Hist.*, ser. 6, vol. vii., pp. 235-255), and recourse was had to figures 11 and 13 of this paper when drawing the pelvic region of the skeleton. The pteroid bone, or backwardly directed metacarpal of the rudimentary thumb, which is incorrectly shown on the ulnar side of the limb in Owen's figure, was introduced from the specimen (R. 1034) in the Geological Gallery, and the details of the caudal vertebrae from specimen (41346), figured by Owen in the "Liassic Reptilia" (pl. 19, fig. 4). Since the back part of the skull is crushed in the Natural History Museum specimens of *Dimorphodon*, the outlines of the quadrate bone and the supra-temporal and lateral temporal fossae were added from

[1] The more complete skeletons were described and figured by Buckland, Owen, and others, and references to the descriptions and figures are to be found in the *Brit. Mus. Cat. Foss. Reptilia*, part i., pp. 37-39.

Newton's figures of the skull of the allied genus *Scaphognathus* (*Phil. Trans.* 1888, B. pl. 77 and 78). There appears to be no palaeontological evidence to warrant the great length which Owen gave to the hindermost ribs in his restoration, and these have, therefore, been considerably shortened, so that the contour of the ventral abdominal wall now passes evenly from the ribs to the ischial bones of the pelvis. So little is yet known concerning the coracoids and sternum of Ornithosauria that, beyond representing the sternum as keeled and as articulating with the first few ribs, but little has here been attempted.

Of the four restorations which form the subject of the present article the greatest interest probably centres around that of *Ichthyosaurus*, inasmuch as the recent additions to our knowledge of this genus have rendered possible a very complete restoration. The species chosen is *Ichthyosaurus communis* from the Lower Lias of Lyme Regis, and the specimens which form the basis of the reconstruction are those in the Geological Gallery bearing the register numbers (41849) and (2000,1*). This is the same species as that of the well-known restoration of Owen's ("Anat. of Vert.," vol. i., 1866, p. 170). An illustrated summary of recent papers on the Ichthyopterygia has already appeared in the pages of this journal (Lydekker, *Nat. Sci.*, vol. i., 1892, pp. 514-521), and in this article is reproduced Fraas's figure of the wonderfully well-preserved specimen of *Ichthyosaurus quadriscissus*, showing the complete outline of the body and affording incontrovertible evidence of the presence of a bilobed tail with the vertebral column running down the ventral lobe, and the existence of a series of irregular integumentary fins along the back (Fraas, *Neues Jahrb. f. Mineral.*, 1892, Bd. 2, pp. 87-90). These details are reproduced in the present restoration (Fig. 2), and, while the proportionate size and the details of the paddle skeleton are taken from the specimens of *Ichthyosaurus communis* above specified, the postaxial flap of the paddle, not supported by skeletal parts, is added from Fraas's figure, from the museum specimen of *Ichthyosaurus intermedius* (R. 1664), described and figured by Lydekker (*Geol. Mag.*, dec. 3, vol. vi., 1889, pp. 388-390), and from Owen's figure of the paddle of *Ichthyosaurus communis* (?) ("Liassic Reptilia," part iii., 1881, pl. 28, fig. 5). The outline and details of the skull were introduced mainly from specimens (39492) and (R. 1164) of *Ichthyosaurus communis*, both of which exhibit a very complete side view of the skull. In none of the specimens of *Ichthyosaurus communis* at the Natural History Museum are the bones of the pectoral girdle undisturbed, so that in restoring this part of the skeleton the shapes of the constituent bones were taken from specimen (41848), but their mutual relations from the very complete girdle which the museum possesses of

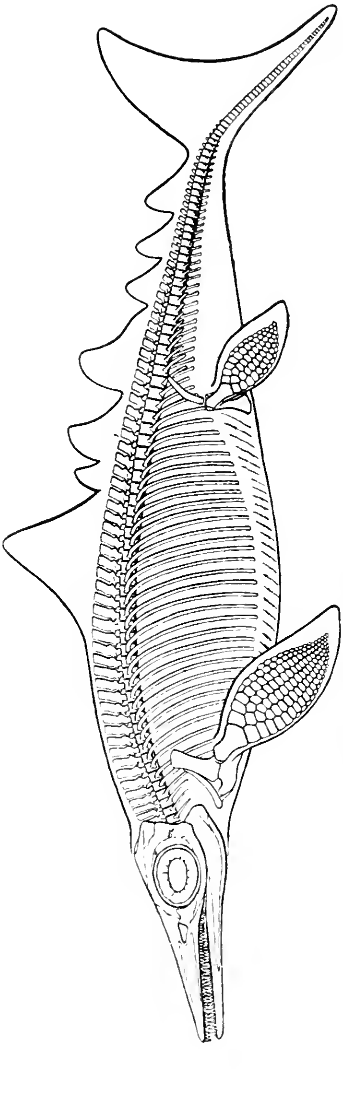


Fig. 2. *Ichthyosaurus communis*, from the Lower Lias of Lyme Regis. ($\times \frac{1}{17}$).

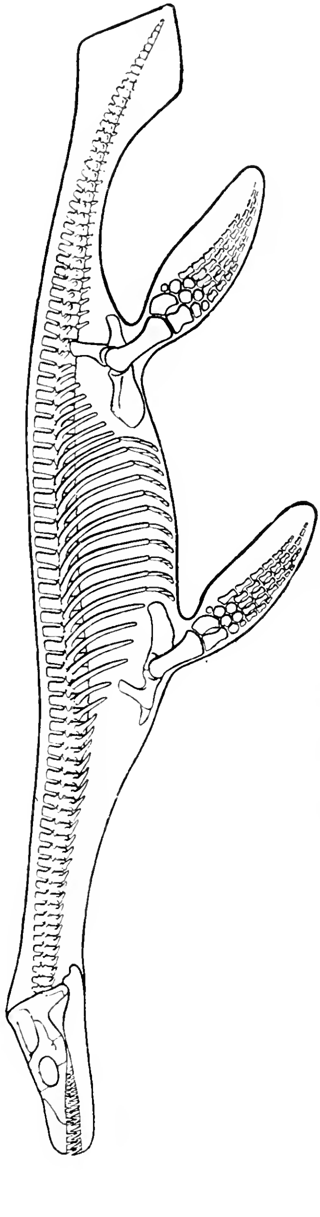


Fig. 3. *Plesiosaurus rostratus*, from the Lower Lias of Charmouth. ($\times \frac{1}{23}$).

Ophthalmosaurus icenicus (R. 2137), described and figured by Seeley (*Proc. Roy. Soc.*, vol. liv., 1893, fig. 1, p. 151). No such difficulty beset the restoration of the pelvis, since the parts are hardly at all displaced in specimen (41849).

The diagram of the Plesiosaur is mainly based on the splendid specimen of *Plesiosaurus rostratus* from the Lower Lias of Charmouth, Dorsetshire, exhibited in the Geological Gallery of the Museum, and bearing the register number (38525). This specimen was described and figured by Owen in his "Liassic Reptilia" (*Sauropterygia*, 1865, pl. 9), but it did not form the basis of his well-known text-book restoration of *Plesiosaurus* ("Anat. of Vert.," vol. i., 1866, p. 52), the species of which, according to Lydekker (*Brit. Mus. Cat. Foss. Rept.*, part ii., 1889, p. 121), is *macrocephalus*. The number of cervical vertebráe in *Plesiosaurus rostratus* is not definitely known. Owen put it down as twenty-four, but there were probably more, since in the specimen (38525) there are evidently some vertebrae missing after the seventeenth (see Lydekker, *loc. cit.*, p. 272). Judging from the shape and relations of the cervical ribs flexion of the neck must have been as difficult of achievement in *Plesiosaurus* as in our modern crocodiles, and so the vertebral column in the cervical region has been drawn nearly straight (Fig. 3), instead of being allowed the graceful sinuous curve which characterises Owen's figure. The outline of the body has been introduced from the figure given by Dames (*Abhandl. königl. Akad. Wiss.*, Berlin, 1895, ii., p. 79); and special attention may be called to the shape of the tail fin, and to the presence of an integumentary extension of the paddle behind the part supported by the internal skeleton. The transverse temporal ridge at the back of the skull would probably not have influenced the general contour of the body to the extent suggested by the diagram. This improbability should have been avoided by making the vertebral column articulate a little higher up the occiput, and by putting the cranial axis more in a line with the cervical vertebrae.

The cranium of the specimen above mentioned is considerably crushed; therefore, while preserving the proportions of the cranial bones of this species, the actual details were added from the more perfect skull (49202) of the allied species *P. macrocephalus*, described and figured by Andrews (*Quart. Journ. Geol. Soc.*, vol. lii., 1896, pp. 246-253, pl. 9). The skeleton of the paddles in the specimen of *Plesiosaurus rostratus* is extremely well preserved, and nothing more was necessary than to copy the outlines of the constituent bones; but as the bones of the pectoral and pelvic girdles are disturbed, a certain amount of restoration was here inevitable, and the assistance derived from the perfect girdles of *Muraenosaurus plicatus* (R. 2428) and *Cryptoclidus oxoniensis*

(R. 2416) and (R. 2616) and the descriptions and figures of these specimens by Andrews (*Ann. & Mag. Nat. Hist.*, ser. 6., vol. xvi., 1895, p. 429; *ibid.*, ser. 6., vol. xv., 1895, p. 333; *Geol. Mag.*, dec. 4., vol. iii., 1896, p. 145) should here be acknowledged.

Pariasaurus baini was chosen to represent the Anomodontia, chiefly because of the completeness of the skeleton exhibited in the Reptile Gallery of the Geological Department of the museum. This skeleton (R. 1971), from the Karoo formation (Trias), was discovered by Prof. H. G. Seeley, near Tamboer Fontein in Cape Colony, and was described and figured by him in the *Phil. Trans.* (1892, B., pp. 311-370, pls. 17-19, 21-23). The Anomodontia constitute such a heterogeneous collection of reptiles that it would be difficult to say what species might be considered to be most typical of the Order. But the completeness of this specimen of *Pariasaurus* certainly renders it more suitable for the purpose in hand than any other Anomodont yet known. The diagram (Fig. 4) is not a

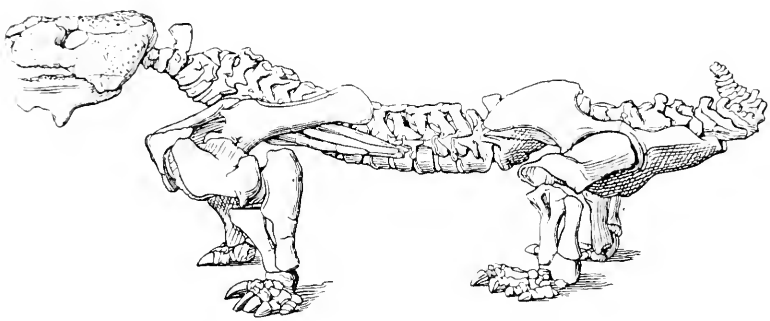


Fig. 4. *Pariasaurus baini*, from the Trias of Cape Colony. ($\times\frac{1}{23}$)

restoration in the same sense as the other three, because, in the first place, the completeness of the skeleton renders possible a very close adherence to nature, and, in the second, because, the whole of our knowledge of the species being derived from this one specimen, a reconstructed diagram would be less instructive than an outline drawing of the specimen boldly treated. The unimportant cracks in the bones shown in the large folding plate in the *Phil. Trans.* have been omitted, and a little diagrammatic cross-shading has been employed here to give the effect of distance, although it was not found necessary in the other three diagrams. The legs are shown articulating in the glenoid cavity and the acetabulum, as in the mounted specimen but not as in the plate; and the anterior cervical vertebrae, which during fossilisation were united into a block of extreme upward curvature, are given a more convenient disposition so as to articulate with the condyle of the skull, which is not the case in the plate, nor in the specimen as now mounted.

My grateful acknowledgments are due to Sir William Flower, K.C.B., for permission to reproduce these figures for publication, and to Mr A. Smith Woodward, assistant keeper of the Geological Department, for sundry hints and advice during the construction of the diagrams.

W. G. Ridewood.

V

The Facetted Pebbles of India

IT is now nearly forty years since the first account (1) of evidence of ice action in Palaeozoic times and within the tropics was published, and though the concept of a Permian glacial period is now one of the accepted results of geological research, the opposition to its acceptance is by no means dead. Some ten years ago this opposition received an access of strength by the arrival and exhibition in England of certain peculiar fragments of rock, first discovered by Dr Warth (2) in the Permian boulder beds of the Salt Range, which did not merely show a striation like that produced by glaciers, but bore several surfaces or facets which met in obtuse angles, and sometimes completely surrounded the stone. A number of these were sent home, unaccompanied by stones of other types, and an idea seems, perhaps not unnaturally, to have sprung up that these were the normal type of boulder, and not, as was the case, curiosities which were strange to geologists in India, and sent by them to their colleagues in Europe, with a view to enlightenment as to the mode of origin of a feature with which they were not acquainted as a result of ice action.

Specimens were exhibited at the Geological Society (3), the British Association (4), and elsewhere, and the general opinion may be expressed in the words of a letter by Dr W. T. Blanford to the *Geological Magazine* (5), that "the great difficulty in accounting for the origin of these facetted blocks is that whilst the smoothed surfaces are in every respect similar to those on stones worn by glacial action, no fragments from moraines, from boulder-clay, or from other glacial deposits, are known to exhibit the peculiar facetting characteristic of the present specimens."

Such was the general opinion held by most, if not all, of those who saw the specimens, and in the museum at Zurich one of these very facetted stones may be seen, with an endorsement on the label, by Professor Heim, to the effect that he had seen nothing like it in recent glacial deposits.

In these circumstances, the facetted stones being supposed to be the evidence on which was based the claim for a glacial origin of the beds in which they were found, it was natural that the opposition to the claim should be strengthened. In reality, however, the supposition that these facetted pebbles were in some way the result of ice action was based on the fact that they were found in beds which, on quite independent grounds, were believed to be of glacial origin, and this belief would have been in no way affected if the facetted stones had been shown to owe their peculiar form to any other agency than ice.

All this while, however, there was on record the description of boulders of precisely similar character in glacial boulder clays of Post-Tertiary age. In 1879 Professor Credner published an account of the scratched stones found in the neighbourhood of Leipzig, (6) in which he mentions three types; the first being those on which a flat surface had been ground away on one side; the second comprising those on which two or more such surfaces are found meeting in obtuse angles; the third, those which show no facets, but are of a rounded or sub-angular form, and bear grooves and scratches scattered over their surface. It would be impossible to give a better classification of the stones found in the boulder beds of the Salt Range, and the closeness of resemblance is only enhanced when Professor Credner's detailed description is read.

This account appears to have been overlooked by all those who saw the Salt Range specimens, for which small blame can be laid, as the volume of glacial literature is so vast that the greater part must remain unread—even by those who devote themselves specially to this subject—and the paper might have remained unnoticed in this connection had it not been accidentally stumbled on while a very different line of research was being pursued. Struck with the light it threw on the origin of these curious pebbles I wrote to Professor Credner asking for further particulars, and in reply was informed that in the collection of the Saxon Geological Survey there are a large number of ice-worn stones showing two or more facets, meeting at an angle, and that in some these facets were distributed round the whole circumference of the stone. He also informs me that after a comparison of the specimens in Leipzig with the figures and descriptions of Drs Warth (2) and Noetling (7), he considers that their nature as glaciated fragments of the same character as those of the "gründ-moräne" of the northern ice-sheet is beyond doubt.

From this it is evident that we have, in Post-Tertiary glacial deposits, ice-worn fragments showing all the peculiarities of those found in the Permian boulder beds of the Salt Range, and with this the last objection to accepting their glacial origin should disappear.

R. D. OLDHAM.

REFERENCES.

1. **Blanford, H. F. and W.T., and Theobald, W.**—On the geological structure and relations of the Talcheer coalfield in the district of Cuttack. *Mem. Geol. Surv. Ind.*, i., pt. i., p. 33 (1859).

2. **Warth, H.**—A facetted pebble from the boulder bed (speckled sandstone) of Mount Chel in the Salt Range in the Punjab. *Rec. Geol. Surv. Ind.*, vol. xxi., p. 34 (1888).
3. **Blanford, W. T.**—On additional evidence of the occurrence of glacial conditions in the Palaeozoic era, &c. *Quart. Journ. Geol. Soc.*, vol. xlii., pp. 249-263 (1886).
4. **Wynne, A. B.**—A facetted and striated pebble from the Olive group conglomerate of the Chiel Hill in the Salt Range, Punjab, India. *Rep. Brit. Assoc.*, lvi., pt. ii., 631 (1887). *Geol. Mag.*, 3rd decade, vol. iii., p. 492 (1886).
5. **Blanford, W. T.**—On a smoothed and striated boulder from the Punjab Salt Range. *Geol. Mag.*, 3rd decade, vol. iii., p. 574 (1886).
6. **Credner, H.**—Ueber gletscherschliffe auf Porphyrkuppen bei Leipzig, und über geritzte einheimische Geschiebe. *Zeitschr. deutsch. Geol. Ges.*, vol. xxxi., pp. 21-24 (1879).
7. **Noetling, F.**—Beiträge zur Kenntniss der glacialen Schichten permischen Alters in der Salt-Range, Punjab (Indien). *Neues Jahrbuch*, 1896, vol. ii., pp. 61-86.

SOME NEW BOOKS

A French Treatise on Zoology

Traité de Zoologie Concrète. Vol. I. La Cellule et les Protozoaires. By Yves Delage and Edgard Hérouard. Pp. xxx. 584, with 870 col. figs. Paris: Schleicher Frères, 1896. Price 25 francs.

This is the first instalment of a work which, if it finishes as it has begun, will be of the greatest value, since it combines completeness and erudition with a method of treatment at once highly original and well adapted to the end in view.

The primary object of the authors is to smooth the path of the student and to help him in his difficulties, and in their preface they are at pains to explain how it is intended to bring about this result. Every one knows how difficult it is, when commencing the study of a group of animals with the help only of an ordinary text-book of comparative anatomy, to apply the more or less vague generalities of which such works are composed to the case of a particular form. The usual method of describing a group of animals in the text-books or treatises on zoology is to commence with a chapter or chapters in which the comparative anatomy of the group is described organ by organ in a purely abstract manner—that is to say, without reference to the remaining organs of the body. This is followed by a systematic portion in which the families or genera are catalogued and distinguished by means of their external characters. The great defect of this mode of treatment is the want of any proper link between the abstract and the concrete, between the general and the particular. The beginner who is as yet unfamiliar with the group in question finds an extreme difficulty in forming a clear idea of how a particular form is organised in its entirety, since he has to combine in his mind a brief summary of its external characters with the rather vague mental image of its anatomy which he constructs by wading through the comparative chapters and picking out such portions as may apply to the form under consideration. Hence text-books of this class, though extremely valuable to the advanced student or teacher as works of reference, are confusing to the learner, who requires above all things something real and concrete, upon which to found his general notions.

It is not every student who has the time or opportunity to obtain the empirical basis so necessary for a clear grasp of the main principles, by consulting the special memoirs or monographs dealing with the forms he is studying, and in order to help him out of the difficulty a large class of practical text-books of zoology has sprung up in recent years, in which particular forms are chosen as typical examples of the larger systematic groups and described in great detail. In this way a division of labour has come about whereby the treatise of comparative anatomy is supplemented and elucidated by the concrete examples of the practical text-book. It is, however, only to a limited extent that such co-operation is possible or practicable. The number of types which can be described within the limits of a practical text-book must be necessarily few by comparison with the ground covered by the more abstract treatises, and illustrative only of the greater systematic divisions. The design of our authors is an ambitious one. It is nothing less than to effect a compromise, so to speak, between the abstract and the particular, and to impart a general knowledge founded upon judiciously constructed, concrete examples.

A complete knowledge of a natural group of animals might be supposed attainable only by a separate description of each of the species in it. But allied species and even genera only differ amongst themselves by secondary characters, and it is not until we come to families or orders that we find anatomical characters of sufficient importance to warrant detailed treatment in the limited compass of a text-book. Hence for each such systematic division the authors propose to commence with the description of a generalised type, in which the characters of the subgroup—in most cases a suborder—shall be found combined, and then to proceed to point out how the various forms comprised in the subgroup differ severally from the essential type. But such generalised and fundamental types are to be found but rarely in nature. The authors have therefore invented and constructed a morphological type for each suborder, a fundamental form "which summarises in itself that which is common to all the actual forms of the group, or which is presented as a simple initial form, from which the others would be derived by progressive complications." In this way it is possible to present general notions in a concrete form. It might be objected that the morphological types are not real but represent to a certain extent ideal abstractions. In answer to this it is pointed out that the term concrete does not mean real. "A type may be concrete even though it is ideal What does it matter to a student when he reads a precise description with the indication of all the organs and of their relations, whether the being thus described really exists in nature or whether it represents only the mean, we might almost say the composite portrait, of a small group of real beings? The idea he will obtain of the being described, and later of the entire group, will be none the less precise and none the less accurate."

For the reasons that have just been set forth, Messrs Delage and Hérouard call their work a treatise of concrete zoology, as opposed to the more abstract zoology of the ordinary text-books. They claim, and we think justly, to have helped the student over one of his greatest difficulties, though as they acknowledge, the Protozoa are scarcely a fair test for the efficiency of the method on account of their simple structure, and we are begged to suspend our final judgment until the appearance of the volumes to follow. At the same time the great store of information brought together in a most painstaking and laborious manner renders the work very useful to others than beginners, chiefly on account of the simple and methodical arrangement that has been adopted, and the consequent ease with which any required facts can be hunted down.

The present volume contains two parts, the first dealing with the cell and its functions, the second with the Protozoa. In the first part the authors give a review of general cytology, and deal with the vexed questions of protoplasmic and nuclear structures. Here, as they admit, they are often on very controversial ground, and experts would find much to criticise and to dispute in the opinions put forward. It cannot, however, be laid to the charge of our authors that they have neglected or passed over other views, though their criticisms upon them are occasionally perhaps rather one-sided; for having in view once more the exigencies of the student they have divided the work into two parts, one printed in large type composing the main text, the other in small type contained in the footnotes. In the former the objects are described in a simple and straightforward manner from the point of view taken by the authors, while to the footnotes are relegated the more controversial subjects as well as details concerning the less important or doubtful genera and similar matters. In this way the work is rendered extremely complete, and while on the one hand the student is treated to a clear and continuous, if at times dogmatic, *exposé* of the subject, he is enabled, on the other hand, to greatly extend his knowledge, if he wish, by means of the references and discussions in the footnotes.

The portion of the work dealing with the Protozoa contains a mass of information which it would be impossible to criticise in detail. We must, however, take exception to one innovation which has been introduced into this work, namely, the manner in which the authors have changed the names of the groups, in the attempt to introduce one uniform system of terminations for the equivalent taxonomic subdivisions. The results have been in some cases almost disastrous; we can hardly recognise such familiar groups as the Flagellata and Ciliata when we see them written as "Flagellia" and "Ciliae" respectively. In science a very good excuse is always necessary before the alteration of well-established names can be permitted. In the present case it is again solicitude for the student which is responsible for this well-meant but, we think, injudicious reform. It is supposed, for instance, that to make the names of classes end in *ia*, and subclasses in *iae*, in all cases, will tend to clearness. Not only, however, is this alteration of names rather confusing, especially to the beginner, but it involves the assumption, which can scarcely be maintained, that the various categories known as classes, subclasses, orders, and so forth, are of the same taxonomic value in all groups. The fact alone, however, that in the classifications of different authors, different names are given to equivalent divisions, is a sufficient refutation of this view, for where one author has a subclass divided into orders, another may have an order divided into suborders. It is, therefore, rather premature to coin a uniform termination for subclasses or orders until the value of these categories is more fixed. But further, Messrs Delage and Hérouard have given new names in their scheme to just those taxonomic categories for which, being of lesser and therefore of more definite value, the almost universal custom of naturalists has already established a uniform terminology. Nearly everywhere now names of families are made to terminate in *idae* and subfamilies in *inae*; yet our authors choose to employ the termination *idae* for suborders and *inae* for families. The alteration in this way of an already uniform and established system of nomenclature seems to us quite unjustifiable.

In the classification of the Protozoa we notice a certain number of new names, some of which are coined for the purposes of new classifications, while others are older groups renamed. The Sporozoa are divided into two subclasses (1) Amoebogeniae (nov.) with amoeboid sporozoites, and containing the Myxosporidia, and (2) Rhabdogeniae (nov.), with sporozoites of definite form, including the remainder of the class. The Rhabdogeniae are further divided into the two orders, Dolichocystida (nov.), comprising the Sarcosporidia, and Brachycystida (nov.), which includes the remaining forms, namely, the Gregarinidae, Coccidiidae, Haemosporididae (*Drepanidium*, etc.) and Gymnosporididae (*Haemamoeba* and others). In the Ciliata Stein's four orders are maintained. The Holotricha are divided into Gymnostomidae, corresponding to Bütschli's Gymnostoma, and the Hymenostomidae (nov.), corresponding to Bütschli's Trichostoma Aspirotricha. The Peritricha are divided into Scaiotrichidae (nov.), comprising Bütschli's Licnophorina and Spirochonina, and Dexiotrichidae (nov.) = Vorticellina. In the former the adoral zone of cilia has a sinistral (Σκαιός) twist, in the latter a dextral (Δεξιός).

The work is illustrated throughout by excellent diagrammatic figures, for the most part coloured, some even in as many as four colours. None of the familiar "vieux clichés" which persistently haunt one text-book after another, are permitted to intrude between these covers, all the figures being either specially constructed diagrams or else copied or modified from the original figures in the special memoirs. In short, the whole book is characterised throughout to a marked degree by one most precious quality, that of clearness and lucidity both in description and illustration. In conclusion, we congratulate Messrs Delage and Hérouard most heartily on the first results of their labours, and wish them all success in the great task which they have before them. E. A. M.

Goniatites in the British Museum

Catalogue of the Fossil Cephalopoda in the British Museum (Natural History). Part iii. Containing the Bactritidae and part of the suborder Ammonoidea. By Arthur H. Foord and George Charles Crick. Pp. xxxiv., 303. Published by order of the Trustees. London, 1897. Price, 12s. 6d.

The first part of this Catalogue, published in 1888, and the second published in 1891, dealt with the Nautiloidea and were written by Dr Foord alone. That gentleman's removal to Dublin made some help imperative, and Mr Crick, the assistant in charge of the Cephalopoda in the Geological Department of the British Museum, has proved a worthy collaborator. The experience gained from previous work, combined with this fortunate co-operation, has brought the present volume, which treats of the older Ammonoidea, very near our ideal of what such a museum-catalogue should be. Some of these publications of the Natural History Museum have been important contributions to knowledge, but have left us still in the dark as to the precise extent or value of the Museum collections; others have displayed a marvellous zeal in the hunting up of ancient literature and the compilation of synonymies, but have not greatly assisted the student; others again have been dry lists of specimens, jotted down in haste and repented of at leisure, but having at least this merit, that they told us what material the Museum contained.

The present volume seems to us to combine the advantages, without the defects, of those predecessors to which we have referred. The descriptions of the species are most carefully drawn up, each being based, where possible, on examination of the type-specimen itself, and following a uniform plan, which greatly facilitates comparison. A useful diagram explains the terms employed. The difficulty of describing the all-important suture-line has been avoided by giving a tracing made from an actual specimen, if possible the type. There are also woodcuts of specimens, many of them from original drawings by Miss G. M. Woodward. The references to literature, in the form of lists of synonyma, are carefully done, but occupy a disproportionate space. When a species has never received more than one specific name, *e.g.*, *Prolecanites becheri*, it seems unnecessary to trace this through all the obvious genera, such as *Ammonites* and *Goniatites*, to which it has been referred by older authors, including the compilers of text-books and nomenclators. The information is useful, but might be put in less compass. Finally, this is a true catalogue: every specimen in the Museum is mentioned in such a way that it can be identified, and the number under which it is entered in the Museum lists or registers is printed. Thus the foreign student can gauge precisely the wealth of the collection, can tell whether what he wants to see is contained in it, and on reaching the Museum can ask for the definite specimen he requires.

One or two improvements may be suggested for future volumes of this and other catalogues. The statements of locality are misleading: under each species comes a series of statements made with reference to the species in general, including the usual size attained. After "*Size*" follow "*Form. and Loc.*" These, however, refer not to the species, but to the particular specimens in the Museum. It would be better to give the general geological and geographical distribution of the species, and to refer to definite localities under the individual specimens, as is already done in cases where more than one locality is represented. It would be well to draw more forcible attention to the type-specimens, *e.g.*, by broad-faced type, also to distinguish cotypes, paratypes, and the rest. It is good to know the names of donors, especially when they are such men as J. E. Lee and John Rofe; but it would also be good in other cases to know the names of those from whom specimens have been purchased, since these must often have been geologists of repute, whose statements of locality and the like would be of more value than those of an ordinary dealer or inefficient collector. It is sad to see how many specimens are entered with "*History unknown*," and of how many others "*Transferred from Mus. Pract. Geol.*" the necessary details are not recorded; but this is no fault of Messrs Foord & Crick.

A catalogue is not a text-book; nevertheless the Catalogues of the British Museum have come to be looked for by us outsiders as likely to introduce some improved system, and to unravel the tangle of conflicting authorities. We look for some pronouncement on debated points, and for some clue through the maze that perplexes us. In these respects the present work leaves us unsatisfied. There is no exposition of the principles of the classification adopted; there are no keys; and there is little to indicate the relations of the species to one another. The authors suffer from an excess of caution: they tell us what Hyatt has written, what Haug thinks, what is the opinion of Branco, and what one will find in Zittel; but what their own views are, wild horses will not drag from them. It is the duty of people with such advantages as have our authors, not only to have opinions but to express them. It is not enough to tell us of so interesting a form as *Clymenia* that its derivation "is at present enigmatical"; it has been that for half-a-century. But one doubts occasionally whether even the authors know their own minds. There is a vast deal of quotation as to the systematic position of *Bactrites*, but where it is placed after all, we cannot understand. In Part I. of the Catalogue, Dr Foord inserted it among the Nautiloidea; in Part II. he said that he would refer it to the Ammonoidea; and now in Part III. it is hung up in the air, as though it were an Archicephalopod or a Schematic Mollusc. Again, among the quotations bearing on this, we find a passage from Hyatt and some of his figures; but we find no quotation of the destructive criticism of this passage published by Mr Crick himself, in conjunction with Mr Bather, in *Natural Science* for December 1894 (vol. v., p. 425). It is less strange, but quite as inexcusable, that there should be no reference to the important papers by J. M. Clarke in the *American Geologist*. There is always some excellent excuse for the suppression of evidence, and we shall no doubt learn that this is all for the good of the Government—or its officials.

It is curious, in a volume dealing with the *Goniatites*, to find no family Goniatitidae and no genus *Goniatites*. The type-species of *Goniatites* is the *Nautilites sphaericus* of Martin, a perfectly well-known form, which appears in this book as a *Glyphioceras*. There seems no room for doubt that *Glyphioceras* must rank as a synonym of *Goniatites*, since the latter has some sixty years' priority.

With the few exceptions mentioned, the volume is brought well up to date, and the care with which it has been compiled augurs well for the continuation of the series.

Popular Natural History

The Concise Knowledge Library—Natural History. Edited by Alfred H. Miles. 8vo, pp. xvi. and 771, with 530 original illustrations. London: Hutchinson & Co., 1897. Price, 5s.

This volume is the first of a projected series, the purpose of which is shown in the title, and in the editorial preface. The volumes are intended to be "concise and popular . . . at once accurate in statement, handy in form, and ready of reference"; and the results hoped for are, "that much time may be saved to busy people and much help afforded to students." The plan is excellent, but the execution is scarcely so successful as one would expect from the names which figure on the title page. Mr Lydekker is responsible for the mammals, reptiles, amphibians, fishes, and cyclostomes; Dr R. Bowdler Sharpe for the birds; Mr Garstang for amphioxus and balanoglossus; Mr W. F. Kirby for the arthropods; Mr B. B. Woodward for the molluscs; Mr Bather for the lamp shells and starfishes; Mr Kirkpatrick for the moss animals; Mr Pocock for the "worms"; and Mr and Mrs Bernard for the coelenterates and the protozoa. There is, of course, a good deal of excellent work in the book, which is a marvel of cheapness; but some of the sections dealing with vertebrata read as if they were made up of popular newspaper articles, hurriedly, and not very skilfully, welded together. The best part of the book is that dealing with the lowest vertebrates and the invertebrates. The sections on lamp shells and starfishes deserve special mention; and students of the bryozoa will be grateful to Mr Kirkpatrick for appending to his section a classification and bibliography. In a second edition it would be well to adopt the same zoo-geographical regions for mammals and birds; and the puzzling sentence on p. 122—"the teats of the female elephants are placed between the hind legs, and the young calf sucks with its mouth, and not with its trunk"—should be deleted. Stricter supervision, too, should be exercised over the illustrations. Fig. 86 (p. 156) bears the inscription *Tragelaphus angasi*, about which no word occurs in the text; the inscription of Fig. 82 (p. 349) does not refer to the bird figured; the illustration of the bearded reedling (p. 368) bears the generic name *Calamophilus*, while *Panurus* is given in the text, though it does not appear in the index. The misprints, of which there are considerably more than are justly chargeable to the printer, should be carefully sought for and corrected. *Alunda, Teirao, Phasiandae, Paro, Syrrhoptes, Scolopaeinae, Nyctierax nycticrax, Trypanns, Anthrophysa* (and many others) are likely to prove hindrances rather than helps; and some readers may stumble at "catenanan formation." "Pellage," too, is an unusual form in English books; while "Leydecker" and "Brydden" conceal familiar names.

Moths

A Handbook to the Order Lepidoptera. By W. F. Kirby, F.L.S., F.E.S. Vol. V. Moths. Part 3. 8vo, pp. 332, plates 32. (Allen's Naturalist's Library.) London: W. H. Allen & Co., 1897. Price, 6s.

With praiseworthy celerity, Mr Kirby has brought his handbook of lepidoptera to a conclusion. It is unfortunate that his account of the noctuids, the geometers, and the whole of the so-called "microlepidoptera" has had to be compressed into the volume now before us. The space is quite inadequate for a due treatment of these groups, especially as the author continues to devote a quarter or half a page to the synonymy and references of each species which he selects for description. Although a large number of moths are described and figured, the families are necessarily much more cursorily treated than those dealt with in the preceding volumes. For example, among the noctuids we find only one British species, each of such large genera as *Acronycta*, *Leucania*, and *Agrotis*, and not a single representative of *Hadena*; and turning to the geometers, the large and important genera *Eupithecia* and *Cidaria* are altogether omitted. As for the "microlepidoptera," Mr Kirby states in his preface that he has found it "impossible to do more than describe and figure a selection of species belonging to various families."

It is a considerable disappointment to find that in his classification of the noctuids and geometers, Mr Kirby closely follows the arrangement proposed forty years ago by Guenée, instead of availing himself of the work of those modern writers who have critically studied the structure of these moths. There is probably hardly a serious student of the noctuids who would not closely associate the genera which Guenée distributed between his two "families," Apameidae and Hadenidae; yet Mr Kirby treats these assemblages as sub-families, and separates them widely in his series. It is sincerely to be hoped that in the coming volumes of his great catalogue of the lepidoptera Mr Kirby will adopt a more modern classification. Otherwise the value of his work will be seriously diminished.

It could not be expected that much space would be devoted to the habits of the moths which are mentioned, but a few notes of considerable interest on this subject are given by Mr Kirby. He has rescued from a long obscurity an account published in 1830 by the Rev. L. Guilding on the aquatic larva of a West Indian pearl-moth (*Petrophila fluviatilis*). He also calls attention to de Riville's account, published nearly one hundred and fifty years ago, of a Mediterranean *Antispila*, whose caterpillar mines in vine leaves.

A large number of species are figured in the coloured plates, a good proportion of them for the first time. Though the effect of some of the colouring is rough, and there is a want of uniformity in the setting of the specimen, these figures will be helpful for the determination of species. Mr Kirby's wide knowledge of insects and their literature must needs make his writings useful to naturalists, even if they do not care to adopt all the changes in well-known names which he believes to be necessary. G. H. C.

A Bibliographical Enigma

Manuel de Géographie botanique. Par Osear Drude. Traduit par Georges Poirault et revu et augmenté par l'auteur. Livraisons, 14-16. 8vo, pp. 513-552, with 4 maps. Paris: Klincksieck, 1897.

We are glad to receive this, the completion of a useful translation and edition of Drude's work on plant geography. The previous parts have been noticed as they appeared; the last consists chiefly of an exhaustive, and so far we have tested it, accurate index, a list of additions and corrections, and four folding maps. The price of the book as a whole is 18 francs. While commending the work, we must call attention to a serious omission, from a bibliographical point of view. The title page, just issued, bears date 1897; but the first part appeared in June 1893, and the remainder at various dates between 1893 and 1897. As the covers of the individual parts all bear the same date, viz., 1893, and as there is no reference to successive dates in the text, its issue in parts will be lost sight of, and the whole will seem to have appeared in the present year. Thus another puzzle will be added to the future bibliographer's list—a list already far too long. To lessen this evil as far as possible we append the dates of notice in *Natural Science* of the individual parts. As *Natural Science* is wont to be prompt, this gives a very fair indication of the dates of publication :—

Part 1, noticed in vol. iii., p. 152 (Aug. 1893).
Parts 2 and 3, noticed in vol. iv., p. 464 (June 1893).
„ 4 and 5, „ vol. vii., p. 214 (Sept. 1895).
„ 6 and 7, „ „ p. 282 (Oct. 1895).
„ 8 to 10, „ vol. viii., p. 62 (Jan. 1896).
„ 11 to 13, „ „ p. 393 (Dec. 1896).

New Serials

We learn from *Science* that the Italian Società Positivista has established a bi-monthly journal, *Il Pensiero Moderna*, published at Via Collegio Romano 26 (Rome?), and edited by Prof. Sergi. The object of the society is to demonstrate the importance of science for modern life.

The *American X-ray Journal* is edited and published by Dr Heber Roberts, St Louis, Mo., and is intended for the medical profession.

We may mention here the *Bulletin* of the Geological and Natural History Survey of the Chicago Academy of Sciences. The first is a monograph of the lichens of Chicago and the neighbourhood by W. W. Calkins.

Further Literature Received

First Principles of Natural Philosophy, A. E. Dolbear : Ginn, Boston. The Choniostomatidae, H. T. Hansen : Host, Copenhagen. Euclid (Books i.-iv.), and The Tutorial Trigonometry : Clive. System der Bakterien, W. Migula : Fischer, Jena. Untersuchungen über das Erfrieren der Pflanzen, H. Molisch : Fischer. Allgemeine Physiologie, M. Verworn, ed. 2 : Fischer. Open-Air Studies in Botany, C. Lloyd Praeger : Griffin. Elementary Biology, T. Jeffrey Parker, ed. 3 : Macmillan. Twenty-seventh Ann. Rep. Entom. Soc., Ontario, 1896. U.S. Dept. Agriculture, Technical Series, No. 6. Ann. Rep. Manchester Museum, 1896-97. Journ. Inst., Jamaica, vol. ii., No. 4. First Ann. Rep. Geol. Commiss., Cape of Good Hope, 1896.

Crustacea of Norway, G. O. Sars. vol. ii., pts. v., vi. : *Bergens Mus.* The Asparagus Beetles, F. H. Chittenden : *Year-Book U.S. Dept. Agric.* Insect Control in California, C. L. Marlatt : *ibid.* The Use of Steam Apparatus for Spraying, L. O. Howard : *ibid.* The Protective Value of Action, volitional or otherwise, in Protective Mimicry, F. M. Webster : *Journ. New York Entom. Soc.* Biological Effects of Civilization on the Insect Fauna of Ohio, F. M. Webster : *Ann. Rep. Ohio State Acad. Sci.*

Jersey Weekly Press, August 7 ; Amer. Geol., August ; Amer. Journ. Sci., August ; Amer. Nat., August ; l'Anthropologie, May-June ; Botan. Gazette, June-July ; Feuille des jeunes Nat., August ; Irish Nat., August ; Literary Digest, July 10, 17, 24, 31, August 7 ; Naturae Novit., No. 12, June ; La Naturaleza (Madrid), Nos. 20-22 ; Naturalist, August ; Nature, July 22, 29, August 5, 12 ; Naturen, June, July ; Photogram, July, August ; Review of Reviews, July, and do. Australia, May ; Revista Quind. Psichologia, &c., vol. i., fasc. 1-7 ; Rev. Scient., July 17, 24, August 7, 14 ; Science, July 9, 16, 23, 30 ; Sci. Amer., July 10, 17, 24, 31, August 7 ; Scot. Geogr. Mag., August ; Scot. Med. and Surg. Journ., August ; Proc. Biol. Soc., Washington, vol. xi., pp. 213-230 (July 15).

OBITUARIES

Sir John Charles Bucknill, one of the first editors of *Brain* and editor of the *Journal of Mental Science* for nine years, was, at the time of his death recently, acting in the capacity of Censor, Councillor and Lumleian Lecturer in the Royal College of Physicians. In 1866 he was elected Fellow of the Royal Society, and in July 1894 he was knighted. He produced a large number of psychological works, making insanity and similar subjects a specialty. He became especially popular through his psychological essays on the "Mad folk of Shakespere."

Mr Samuel Laing, who died on August 8th at the advanced age of 87 years, was formerly chairman of the Brighton Railway and had a lifelong connection with railway interests. He devoted his leisure to scientific pursuits, and his principal original work was the exploration of the prehistoric refuse heaps of Caithness, which he described, with the aid of Prof. Huxley, in 1866. During recent years he successfully devoted himself to the popularisation of science, his best known works being entitled "Modern Science and Modern Thought" and "Human Origins."

The death of Captain Bertram Lutley Sclater at Zanzibar on July 24th will excite widespread sympathy among English naturalists for his father, Dr P. L. Sclater, as well as deep regret at the loss of an officer whose career was full of promise. His main work was road-making in British Central and British East Africa; during which he accomplished many careful surveys. His maps form a valuable addition to our knowledge of the geography of those countries, in the future development of which the work which cost him his life will play an important part.

Another geographer whose death cannot pass unnoticed in *Natural Science* was the late Ney Elias, a man whose work, though popularly very little known, was of such importance as to place him among the greatest English travellers of this century. His first paper, "Notes of a Journey to the New Course of the Yellow River," is one of the classics of physical geography. His exploration of western Mongolia during a journey from Pekin to Nijni Novgorod is one of the six great feats in Asiatic travel. In 1885 he settled the vexed question as to the sources of the Oxus, and later on made numerous less famous journeys in the Indian borderlands. His shyness was excessive, and he had no ambition for notoriety. His great feats are recorded in technical geographical papers, but these will live. His reputation as a traveller will probably be greater in a century's time than it is to-day. But in the meanwhile it would be very useful if his papers were collected and republished with some sketch of his life.

THE death is announced of THEOPHILE CHUDZINSKI at Paris on June 18th, aged 55. By birth a Pole, he studied at Moscow until the insurrection of 1863 caused him to give up his studies and join in the movement. This was followed by an incarceration of several months in Austria, but escaping he made his way to Belgium and subsequently to France, where he spent the rest of his life. It was in pursuing his anatomical researches that he was first noticed by Broca at Paris, who later gave him a post in the Laboratoire d'Anthropologie des Hautes-Etudes. For several years Chudzinski assisted his master until the latter's death, when he devoted himself to anatomical works, particularly to the study of the brain and the anatomical resemblances and differences between that of man and of the anthropoid apes. A large number of anthropological and anatomical works were the result of his minute researches. These, although edited in Paris, were not published in French.

LUCIEN BIART, who died recently, was a talented author, and although he chose to veil his scientific knowledge in the form of novels, that knowledge was incontestable. A great love of travel took him in 1845 to Mexico, where he studied archaeology and ethnography. In addition to his novels, the chief of which are "Le Roi des Prairies," "Entre deux Océans," etc., he wrote a volume on the red races for the *Bibliothèque ethnologique*, as well as a monograph on the Aztecs.

The deaths are also announced of:—PAUL SCHUTZENBERGER, the physiological chemist of the College de France, Paris, aged 67; P. C. PLUGGE, Professor of Pharmacology and Toxicology at Göttingen; ARMINIO NOBILE, Professor of Geodesy, and author of many valuable papers on astronomy, at Rome; Professor OERTEL of Munich, distinguished for his researches on the etiology of diphtheria; ALFRED MOQUART, Professor of Anatomy at Brussels, on June 5, aged 42 years; MARTIN WILCKENS of the Agricultural School of Vienna, on June 10, aged 64; Count VICTOR TREVISAN DI SAN LEON, the cryptogamist, in Milan, on April 8, aged 79 years; ROBERT DOUGLAS, known for his work in arboriculture and forestry, on June 1, at Waukegan, Ill., aged 84; G. OSSOWSKI, the geologist, on April 16, at Tomsk; P. B. L. VERLOT, botanist, at Verrières-les-Buisson; Rev. ROBERT HUNTER, botanist, on Feb. 25, at Epping Forest, aged 74; SAMUEL JAMES AUGUSTUS SALTER, botanist, on Feb. 28, at Basingstoke, aged 72; Geheimrat HEYDENREICH, student of Lepidoptera, on May 18, at Osnabruck; the coleopterologist, DANIEL MÜLLER, on May 22, at Barcelona; the oologist, C. Q. ASCHAN, schoolmaster at Kuopio, Finland; Dr ANDERS JOHAN MALMGREN, a well-known ichthyologist and student of Annelida, of Ulcaborg, Finland; Dr WÖLFERT and a mechanic named KNABE, who fell while sailing at a height of 1000 feet in a navigable balloon, at Tempelhof, near Berlin; FERDINAND BÉCLARD, palaeontologist at the Brussels Museum, who was in the midst of important studies of Devonian brachiopods; R. ALLAN WIGHT, the economic-entomologist of Paerva, near Auckland, N.Z., on the 22nd December 1896, aged 73 years; MICHAEL ANGELO CONSOLE, professor in Palermo University, and well-known as a cactus-hunter, on May 13, aged 85; PETER VON TUNNER, of the mining district of Leoben, on June 8, aged 89 years; DOMINIK HOFER, the veterinarian of Munich University, on June 13, aged 80; Dr JULES JULLIEN of Havre, the zoologist (Bryozoa); CHARLES F. WELLS and J. W. JONES, who were exploring the West-Australian deserts, killed by the natives in June; FRIEDRICH C. STRAUB, the botanist, at Liberia, on March 21, aged 26; ALFRED SUTTON, of the well-known firm of J. Sutton & Sons, Reading, on August 9, aged 80.

NEWS

The following appointments are announced :—
Henry Charles Williamson, as Naturalist to the Fishery Board for Scotland ; W. W. Watts, of the Geological Survey, as assistant-professor of geology in the Mason College, Birmingham ; Miss Bertha Stoneman as professor of botany in the Huguenot College for Women at Cape Town ; Dr Edward Fischer to succeed his father, Prof. L. Fischer (retired), as professor of botany in the University of Bern ; Dr Julius Paoletti of Padua to be professor of natural history at the Melfi Technical Institute ; Dr Pio Bolson as second assistant in the Botanical Garden of Padua ; Dr M. Raciborski of Cracow and Dr Zehntner of Pasoeroean (Java) to be professors of botany and entomology at the experimental station for sugar production at Kagok Tegal (Java) ; Hugo Münsterberg of Freiburg as professor of psychology in Harvard University ; Dr Antonio Crocichia as professor of biology in the Catholic University of Washington ; Dr H. Fling to the chair of biology, F. A. Mitchell to the chair of geography, and Dr F. D. Sherman to the chair of psychology at the Oshkosh Normal School ; Ernest B. Forbes as assistant state entomologist in Minnesota ; Dr C. E. Beecher as professor of historical geology at Yale University ; Dr L. V. Pirsson as professor of physical geology in the Lawrence Scientific School ; Dr Geo. B. Shattuck as assistant in geology at Johns Hopkins University ; Oliver L. Fassig as instructor in climatology, and Dr Charles R. Bardeen as assistant in anatomy at the same University ; Dr Albert Schneider as professor of botany, pharmacognosy and materia medica in the School of Pharmacy of the North-Western University, Chicago ; E. B. Copeland, of the University of Wisconsin, to be assistant-professor of botany in the University of Indiana ; Dr G. J. Pierce to be assistant-professor of botany in Stanford University ; Henry Kraemer to the chair of botany and microscopy in the Philadelphia College of Pharmacy ; Cleveland Abbe, jun., as a fellow in geology of Johns Hopkins University.

Sir Frederick McCoy has retired from his professorship in the University of Melbourne.

Mr A. W. Bennett has succeeded Prof. Jeffrey Bell as editor of the Journal of the Royal Microscopical Society.

Mr C. Davies Sherborn has resigned the secretaryship of the Geologists' Association of London, and will be succeeded in October by Mr Percy Emary.

Mr C. W. Andrews should have arrived at Christmas Island before this number appears. He left Batavia about the 21st July on Mr Ross's schooner.

The Irish Field Club Union held their annual excursion in July around the north coast of Antrim, making Ballycastle Bay their headquarters.

Mr R. T. Günther is on his way to Lake Urumiya on the Persian frontier, to study the fauna of that lake. We regret to hear that he has had a temporary breakdown in health, and hope he will soon recover and proceed on his way.

We learn from the *Revue Scientifique* that it is proposed to found an experimental station in Madagascar, for the introduction of European cereals and the improvement of local vegetable produce.

Lady Humphry, widow of the late Sir George Humphry, Professor of Surgery at Cambridge, has presented her husband's library to the surgical department of the university.

On July 8th, the Geographical Institute of Lisbon, founded in commemoration of the 400th anniversary of Vasco da Gama's departure for the Indies, was opened by the Geological Society of Portugal.

The German Botanical Society begins its annual meeting at Brunswick on September 21 at the same time as the German Association of Naturalists and Physicians. There will be an exhibition of scientific apparatus.

Prof. Gustav Born, of Breslau, has received the Sömmering prize from the Senckenberg Society of Natural History at Frankfurt for his investigations on the growth of the larvae of amphibia.

In the absence of Prof. Bütschli, Prof. V. Carus presided over the annual meeting of the German Zoological Society at Kiel, June 9-11. There were present thirty-seven members and thirteen guests. The next meeting will be held at Heidelberg at Whitsuntide, 1898.

Dr Henry Woodward, keeper of the department of geology in the British Museum (Natural History), has been permitted by the Treasury to retain his office for another two years. According to the rules of retirement in the Civil Service, his term of service would have expired next November.

The Darwin statue at Shrewsbury was duly unveiled, and stands in front of the Free Library. It is the work of a Shrewsbury man, Mr Horace Mountford, is said to be an excellent likeness, and is the gift of the Shropshire Horticultural Society.

Professors D. T. Macdougal and Campbell, representing a Commission from the American Universities, have visited Jamaica with a view of founding there a botanical research laboratory. Other Commissioners have gone to Trinidad. On their return to the United States they will compare notes as to the best locality and come to a decision.

Mr George Murray and Mr V. H. Blackman have returned from their trip to Panama, after a successful and profitable voyage. They have obtained a large quantity of plankton containing many new specimens, which will shortly be worked out, and have made numerous interesting observations on living forms. They spent two or three days in Jamaica on the way.

According to *Science*, Mr R. W. Porter and Mr A. V. Shand, who are with Lieut. Peary, expect to pass the winter in Baffin Land for the purpose of ethnological and zoological studies and collections. In the summer of 1898 they hope to travel further north and to return to Aberdeen on a whaling ship from Cumberland Sound.

The first meeting of the Jersey Natural Science Association was held on August 5, Dr A. C. Godfray in the chair. The attendance was small but enthusiastic, and included many well-known names. We wish the Association every success, but hope they will not find the usual trouble arising from the proposed library and museum.

Among those visiting Russia during the meeting of the Seventh International Geological Congress at St Petersburg are :—Dr John Ball, Mr L. Belinfante, Mr F. A. Bather, Prof. J. F. Blake, Mr J. H. Cooke, Mr P. Emary, Mr L. Fletcher, Sir Archibald Geikie, Mr Upfield Green, Mr G. F. Harris, Dr Frazer Hume, Prof. M'Kenny Hughes, Mr Philip Lake, Mr D. A. Louis, Mr Henry Louis, Prof. Sollas, Dr P. L. Sclater, Mr G. A. Stonier, Mr J. J. H. Teall, Prof. H. G. Seeley, Mr H. Bauerman, and Dr Wheelton Hind.

The British Museum (Natural History) has acquired the Savin collection of vertebrate remains from the Norfolk forest-bed and other deposits of that coast. A collection of gault fossils from the 300 feet level of the shaft of the Dover coal-field has also been received, and we understand that the whole of the remains from this very interesting and important shaft will be preserved for the national collections, as a typical reference series for the underground geology of the S.E. of England.

The new Botanical Garden of New York will be on an imposing scale, rivalling the new Zoological Garden which Dr Sclater recently described in these pages (*Natural Science*, vol. xi., p. 36). The coniferous trees will occupy thirty acres, the deciduous trees more than seventy acres; the space for the herbaceous plants will be not less than eight acres, while the bog-plants alone will cover five acres. The area of the lakes and ponds will be six acres. The museum will have a frontage of 300 feet, with two wings, each 200 feet in length.

We learn from *Science* that an important change has been effected in the administration of the U.S. National Museum. Acting upon the advice of Hon. Chas. D. Walcott, at present assistant-secretary of the Smithsonian Institute, three sections have been formed—the section of anthropology, with Dr W. H. Holmes, of the Field Columbian Museum of Chicago, as head curator; that of geology, with head curator Dr George Merrill; and of biology, with Dr Frederick W. True as curator.

Harvard University has received under the will of Mr A. W. Thayer $30,000 as an endowment fund to assist poor students. University College, Liverpool, receives £7000 as bequest from Mrs Gee for the advancement of the medical department. It has been decided to institute a Robert Gee fellowship in anatomy of £100 a year and four entrance scholarships of £25 each for one year. Yale University receives land valued at $25,000 by the will of Dr J. T. Atwater of Poughkeepsie; and the Ohio State University an estate left by the late Mr Henry F. Page.

The following have received awards from the Academy of Sciences at Berlin to assist them in their researches:—Prof. Engler, 2000M. (for African botany); Dr R. Hesse, 500M. (eyes of lower marine animals); Prof. H. Hürthle, 850M. (muscles); Prof. Cohen, 1500M. (meteorites); Dr G. Lindau, 900M. (lichens); Prof. R. Bonnet, 800M. (for a work on blood-vessels); Dr L. Wulff, 1500M. (artificial crystals); Dr Lühe, 2000M. (fauna of North African salt lakes); Prof. F. Frech, 1500M. (geology); and Dr G. Brandes, 300M. (Nemertina).

The *Lancet* announces that Prof. Engelmann, the successor of Dr du Bois-Reymond as Professor of Physiology at Berlin, is about to make some alterations in the Institute. Of the four departments, those for microscopical and biological work and for chemical physiology will continue with their present directors. Prof. Engelmann intends to enlarge the department for special physiology, and to share the work of direction with Dr Hermann Munk. The department for physical physiology will for the future be known as the Department for the Physiology of the Sensory Nerves. Prof. König, director of the last-mentioned department, will lecture upon the sensory organs during the last four weeks of summer, and Prof. Thierfelder, of the department for chemical physiology, during the first four weeks of winter, on physiological chemistry.

The Fifty-Eighth Annual Report of the Royal Botanic Society shows a much more favourable prospect. The lease of the Gardens in Regents Park has been renewed for a further term of twenty-one years. The Council has decided to open a school of practical gardening, granting certificates to gardeners, and the material for practice is assuredly ready to hand. This new school has been officially recognised by the Technical Education Board, who have voted £100 to the Society in aid of the scheme. The Council further intends to establish an institute for the teaching of botany and for promoting original research; but as this is a bold and ambitious scheme, outside aid will be necessary to give it a practical effect.

M. Henri de la Vaulx, now travelling in Patagonia, has written to the Société de Géographie de Paris from Rawson, the capital of Chubut. He has visited the Monsonero Indians, where he has found a *tolderia* 18 leagues south of Keurskeule. The cacique, Sayhueke, received him with great cordiality, and he witnessed a *komaruko*, a religious *fête*. M. de la Vaulx has made an ethnographical collection and taken some photographs. On the shore of Lake Colhue the explorer discovered ancient stone sepulchres, in which he found a skeleton almost perfect, as well as ten skulls of Telhuelche Indians. There were a large number of arrowheads, knives, and stone *boleadoros*. These discoveries will prove of great importance to the study of the ancient peoples of Chubut.

In connection with the South-Eastern Union of Scientific Societies, a section for geological photographs has just been established. Its objects are:—(1) To stimulate interest in the observation and recording of geological phenomena; (2) to form annually a set of lantern slides dealing with some part of the geology of the south-east of England, and to circulate these, with an explanatory lecture, among the affiliated societies during the winter session; (3) to form a permanent collection of geological slides and photographs; (4) to contribute to the national collection of geological photographs now being formed at the Jermyn Street Museum under the auspices of the British Association. Particulars as to the work may be obtained from Mr H. E. Turner, the hon. secretary, Bank Street, Ashford, Kent.

The Bill which the Duke of Devonshire presented to the House of Lords concerning the University of London has been withdrawn for the Session. It proposes to appoint the following Commissioners:—Lord Davey (chairman), the Bishop of London, Lord Lister, Sir William Roberts, M.D., Sir Owen Roberts, Professor Jebb, and Mr E. H. Busk, whose powers continue till Dec. 31, 1898, and whose duty is "to make statutes and regulations for the University of London in general accordance with the scheme of the report" presented by the previous Commissioners, with any "modification which may appear to them expedient after considering the changes which have taken place in London education of a university type since the date of the report, &c." The Bill provides that "after the expiration of the powers of the Commissioners, the Senate of the University shall have power to make statutes and regulations for altering or supplementing any of the statutes or regulations made by the Commissioners." The Senate, consisting of the Chancellor and fifty-five other members, to be nominated by the Crown and certain learned and public bodies, "shall be the supreme governing body and executive of the University. All University property shall be administered by the Senate, and the Senate shall have the entire conduct of the University and all its affairs and functions, provided always that no religious test shall be adopted, and no applicant for a University appointment shall be at any disadvantage on the ground of religious opinions; no procedure to a higher degree shall be allowed without examination or other adequate test, nor shall any honorary or *ad eundem* degree be conferred unless the Senate, in exceptional cases, think fit to confer such a degree on a teacher of the University; no disability shall be imposed on the ground of sex."

CORRESPONDENCE

WOMEN WITH BEARDS

The idea of the woman of the future having a beard, noticed in *Natural Science*, vol. xi., p. 2, as set forth by Dr A. Brandt, is scarcely new. If it is not definitely stated by Darwin in his "Descent of Man," at any rate it is an obvious conclusion from what he has to say concerning the appearance of distinctive masculine characters, such as horns, sometimes in the male sex only, sometimes in both sexes. The appearance of the beard in *Homo* is quite analogous to that of horns in other animals; and just as horns have apparently been acquired by the females of certain species by what may be called "inherited transference" from the males, so will beards be obtained in time by the future females of *Homo*.

I thought I had actually drawn attention to this matter of beards in "Some Laws of Heredity" (*Proc. Cottswold Field Club*, vol. x., 1892), but I cannot find it. I had it and the case of horns, etc., in my mind when I wrote therein (p. 275), "A marked character of the male sex . . . being transmitted in accordance with the law of earlier inheritance, ultimately appears early in life in the male. Then the character tends to appear in the female sex also, though why it does so is not clear." Also the transference may be from female to male, which would appear to be the case with rudimentary mammæ in the male of *Homo*.

Let me point out another biological aspect of the case:—Facial hairiness is exhibited more by the unmarried than by the married women. It seems that each woman receives from her male parent latent beard-characters. If she have children she certainly transmits such characters to them. If she has no offspring it seems that the characters tend to develop in her own person. So it will be in the old woman, and not in the "new woman" of the future, that the beard will be most prominent—a startling retribution that the most masculine characters should appear in those who are the greatest old maids. S. S. Buckman.

Cheltenham.

CHEMISTRY IN MUSEUMS

In his notice of the "Report of the Proceedings of the Museums Association," 1896 (*Natural Science*, vol. xi., p. 132), Dr R. H. Traquair, after referring to my paper on "Chemistry in Museums" as carrying "the educational theory of museums to a pitch of absurdity," goes on to say: "A collection of metals, salts, &c., is no doubt a desirable feature in connection with the chemical department of a school or college, but you will learn chemistry only in the laboratory, and certainly not in a museum." The phrase "pitch of absurdity" is too often on the lips and on the pen points of scientific men, and coming from the quarter whence it does is only a too effective means of killing suggestions which might possibly lead to beneficial improvements. As to the sentence I have quoted, one might generalise in the same heedless fashion about any of the sciences which museums seek to illustrate. But it is not intended in these institutions to supply a complete course of study in any branch of science, but to place such illustrations of them before the public as will be helpful to those interested in the study. Hundreds of cases of stuffed birds and mammals will not teach the science of zoology; all the dried plants and wood sections in the museums of Europe will not teach the science of botany, nor can we learn palaeontology by looking at a fossil *Glyptolaemus* in a museum case. These sciences also can only be "learnt in the laboratory." Would Dr Traquair on that account refuse the stuffed bird or mammal, the dried plant and the fossil, a place in the museum? Do not the many mineralogical cases which litter the floors of museums contain simply "a collection of metals, salts, &c."? and these teach, if they teach anything at all, a very little of the science of chemistry. Is Dr Traquair of opinion that a good artificial crystal of common salt is of less educational value than an indifferent natural crystal of Halite?

At the time when my paper was written I was fresh from the laboratory of a technical college, and thought I saw a way to help not only the laboratory student, but also that larger class which is interested in science but cannot obtain access to the laboratories. I did not, be it observed, advocate the formation of a new museum department; I merely asked for the re-arrangement and extension of a department already existing in some museums. In the Edinburgh Museum of Science and Art, for instance, there is, as far as case-room is concerned, a very fair section devoted to the illustration of chemical science, but the arrangement followed is so antiquated, and the specimens are so few—many of the cases are almost empty—and so little representative, that the whole thing is distinctly farcical.

It seems to me that men like Dr Traquair, looking down from a great height on the unscientific rabble, are too much in the habit of dividing the population into three classes—scientific men, who have little use for museums; science students, who have their manuals and their laboratories; and the general public, who have to be amused with stuffed animals and big crystals. They forget that large and increasing class, who are the main support of such papers as *Knowledge*, *Science Gossip*, and even *Natural Science*, who look, and have a right to look, to museums to illustrate their reading. It is for this class that museums must cater more and more. The museum of the future, if it is to be of any educational value whatever, must become the laboratory, the technical college, the university, of those who have to earn a livelihood by their hands. Consequently the formation of such a collection as I advocated, were it only in view of its importance as an adjunct to the evening lecturer, is an absolute necessity.

KELVINGROVE MUSEUM, GLASGOW. GEORGE W. ORD.

SEVERAL correspondents have noted an unfortunate misprint which escaped correction in the June number (vol. x., p. 427, line 34), where "a cross between *Myxine* and the cod" ought to read "a cross between the megrim and the cod."

NOTICE.

TO CONTRIBUTORS.—All Communications to be addressed to the EDITOR of NATURAL SCIENCE, at 67 St James' Street, London, S.W. Correspondence and Notes intended for any particular month should be sent in not later than the 10th of the preceding month.

TO THE TRADE.—NATURAL SCIENCE is published on the 25th of each month; all advertisements should be in the Publishers' hands not later than the 20th.

TO OUR SUBSCRIBERS AND OTHERS.—There are now published TEN VOLUMES OF NATURAL SCIENCE. Nos. 1, 8, 11, 12, 13, 20, 23, 24 being OUT OF PRINT, can only be supplied in the set of first Four Volumes. All other Nos. can still be supplied at ONE SHILLING each.

Price of Set of Vols. I., II., III., IV.	£2	10	0
,, ,, ,, V., VI., VII., VIII.	1	4	0
,, ,, ,, I.—VIII.	3	10	0

One Shilling each Number of any Bookseller.

Annual Subscription, payable in advance to J. M. DENT & Co., 67 St James's Street, London, S.W., Thirteen Shillings ($3·50), post free.

NATURAL SCIENCE

A Monthly Review of Scientific Progress

No. 68—Vol. XI—OCTOBER 1897

NOTES AND COMMENTS

The Training of the Biologist

The most striking feature of late in the study of animals, at least in this country, is the marked tendency of the orthodox school of teachers to break through the narrow bounds which have confined them since 'Biology' replaced the old-fashioned natural history and comparative anatomy. It is, indeed, strange that while the methods of Darwin have had such an immense influence upon the lines of advanced work and research, they should have had so little effect upon the curriculum of elementary teaching. As Prof. Miall well said in his recent address to Section D of the British Association, "the animals set before the young zoologist are all dead; it is much if they are not pickled as well. When he studies their development, he works chiefly or altogether upon continuous sections, embryos mounted in balsam and wax models. He is rarely encouraged to observe live tadpoles or third-day chicks with beating hearts. As for what Gilbert White calls the life and conversation of animals, how they defend themselves, feed, and make love, this is commonly passed over as a matter of curious but not very important information; it is not reputed scientific, or at least not eminently scientific." Finally, as to the inter-relationships of animals, the average graduate of the orthodox university school is in a state approaching blissful ignorance. He is usually led, if not actually taught, to look down with scorn upon the 'systematist.' He imagines he has mastered the whole of the principles of Biology before he has acquired the most elementary notions of generic and specific characters and the phenomena of variation.

There have been two noteworthy utterances on this subject during the past month, that of Prof. Miall in his Presidential Address already referred to, and that of Mr Walter Garstang in the last number of the *Quarterly Journal of Microscopical Science* (vol. xl., p. 211). Both urge that the time has arrived for some reform in the methods of elementary training, and we commend their plea to the careful consideration of all teachers interested in the future progress of biological science.

Mr Garstang gives a practical illustration of the importance of the study of living animals in his interesting paper "on some modifications of structure subservient to respiration in Decapod Crustacea which burrow in sand." It is to this that his general remarks are prefaced, as follows:—

"A good deal of scepticism has been expressed in recent years by various writers as to the utility of the more trivial features which distinguish the genera and species of animals from one another. I do not think that such scepticism can excite much surprise if one remembers that the vast majority of 'biologists' are almost exclusively engaged in the study of comparative anatomy and embryology. The amount of attention paid to these branches of biology has long been utterly out of proportion to the scant attention devoted to the scientific study of the habits of animals and of the function of the organs and parts composing their bodies. With isolated and noteworthy exceptions, the only naturalists who seriously add to our knowledge of the latter subjects are those who travel in distant countries, and who are thus thrown into close relations with animals in their native haunts. Yet all the time there are thousands of forms living on our own coasts and almost at our very doors of whose detailed habits and life-conditions we know practically nothing. I venture to think that the time has come for consideration whether the subject of bionomics (in Prof. Lankester's sense of the word) should not receive more adequate recognition than it does at present in the curriculum of our universities. That such recognition would almost immediately produce effects in a rapid extension of our knowledge is certain; and the subject is invested with so much intrinsic interest, as well as with such important bearings on the problems of evolution, that I believe such recognition would also have the effect of attracting many students to the pursuit of morphology who at present avoid it as a region of mere comparative anatomy. . . .

"It must in any event, however, remain clear that the great problems which Darwin left us as his heritage, after so greatly illuminating them, are not to be solved by the exclusively morphographical researches which occupy the time and zeal of the great majority of naturalists to-day. Even in the best of hands such researches are capable of obscuring even the simple facts of structure which they profess to elucidate; while the study of the functional relations of parts, side by side with the anatomical elucidation of the parts themselves, provides not only the data for generalisations of intrinsic importance, but assistance of an invaluable character in the field of morphological criticism."

The Historical Method in Teaching Biology

Prof. Miall, in his Presidential Address, not only emphasises the importance of the study of living animals in a manner that will scarcely bear abstracting, he also adds another important suggestion, that too little attention is bestowed by biological teachers upon the historical development of the subject. Many students attend the lectures and demonstrations simply because they are compelled to do so by the college curriculum or by the exigencies of a certificate. Those who happen to have no preliminary inclination to the subject thus find many of the statements of bare facts dull, uninteresting, and useless for mental discipline. Suppose that that well-worn topic, the Alternation of Generations, is being treated. As Prof. Miall remarks, "the lecturer defines his terms and quotes his examples; we have *Salpa* and *Aurelia* and the Fern, and as many more as time allows. How can he expect to interest anybody in a featureless narrative, which gives no fact with its natural circumstances, but mashes the whole into pemmican? What student goes away with the thought that it would be good and pleasant to add to the heap of known facts? The heap seems needlessly big already. And yet every item in that dull mass was once deeply interesting, moving all naturalists and many who were not naturalists to wonder and delight. The Alternation of Generations worked upon men's minds in its day like Swammerdam's discovery of the butterfly within the caterpillar, or Trembley's discovery of the budding *Hydra*, which when cut in two made two new animals, or Bonnet's discovery that an *Aphis* could bring forth living young without having ever met another individual of its own species. All these wonders of nature have now been condensed into glue. But we can at any time rouse in the minds of our students some little of the old interest, if we will only tell the tale as it was told for the first time."

Of course, there are many practical difficulties in carrying out this suggestion. It entails much reading of ancient literature, which the ordinary teacher rarely sees. It trespasses upon the allotted lecture hours, already too short for the material to be treated. At the same time, if it succeeded in infusing a little more philosophy into our medical students and others, who are too apt to look upon the preliminary biological course as drudgery, it would well repay the additional labour involved in preparation.

The Times on Archaeology

The foregoing matters are of more or less professional interest. So also are the geological questions—the pre-Cambrian problems of

Canadian geology—treated by Dr George M. Dawson in his admirable address to Section C at Toronto. Sir John Evans' address as President of the British Association, however, is one to interest even the least scientific of the general public. It is a valuable expression of opinion of one of the foremost minds upon the question of the Antiquity of Man, and recent attempts in Europe to carry the human period backwards much further than the Palaeolithic gravels. As might be expected, Sir John Evans' opinions are conservative. The engraved Pliocene shell, *Pectunculus*, is dismissed with ridicule. For the asserted Indian Miocene man he accepts the explanation ably advanced in *Natural Science* by Mr R. D. Oldham; the form of the fractured flints of the Cromer Forest Bed he attributes to natural fractures; he wishes for more evidence as to the age of the beds which yielded *Pithecanthropus erectus*, and the claims advanced in favour of 'Eolithic' man from the high level gravels near Sevenoaks, he considers, as he did in 1890, to be still unproved. In all these points we regard Sir John Evans' scepticism as healthy; and as he is unquestionably one of the best living authorities on stone implements, his opinion must carry great weight. The asserted pre-Glacial man of East Anglia, based on implements supposed to have been found beneath the glacial deposits, the recent excavations by Mr Clement Reid seem to have conclusively disproved. And in the other cases referred to the evidence is either wholly discredited or still inconclusive.

The most remarkable expression of opinion called forth by this learned and calmly scientific exposition of the facts was an astounding leading article in *The Times*. Most of the scientific members of the staff of that paper seem to have gone to Canada, and the reactionary journalists apparently resolved to make the most of their opportunity. Accordingly, we read concerning archaeology, in the first leading article of August 19th, that—

"All its speculations upon neolithic and palaeölithic man are founded upon a single observation, as yet completely unrelated, save by the loosest conjecture, with any other portion of human knowledge. That observation is that flints, chipped or polished in a manner for which natural agencies do not seem to account, have been found in certain deposits at widely-separated points on the surface of the globe. That they were chipped by man as we know him is a mere conjecture. How they came to be so widely distributed is a question that baffles even the licence of surmise. Geology does not attempt to fix within a thousand centuries the age of the beds in which they are found; and geological speculations themselves rest upon assumptions which may be plausible where all real knowledge is wanting, but which can never be scientifically verified. Attentive perusal of Sir John Evans' address itself suffices to show that archaeology is in no sense a science, but rather a recondite and remote branch of historical speculation."

This extract is long, but it is worth reprinting, since it is a sad reminder of how slowly knowledge of the elementary facts of science really spreads. After this, it is perhaps unnecessary to consider any of *The Times'* later criticisms of the President's address or of his proposed Imperial Ethnographic Bureau. It was, however, unfortunate that Sir John Evans should have prejudiced his proposal by suggesting that the work might be undertaken by the Imperial Institute.

The Mammals of the Lost Antarctic Continent

As soon as space permits, we hope to publish some further interesting contributions to our knowledge of primaeval man and the question of his antiquity. This month we go a little further back in the history of the mammalia, and print a translation of an important address to the New University of La Plata by Dr Florentino Ameghino, which is liable to be overlooked in its separate form in the original Spanish. We do not pretend to endorse his conclusions; we look upon some of them, indeed, as visionary speculations. But during the past ten years the brothers Ameghino have done more than anyone else—not even excepting the eminent Director of the Museum La Plata (Dr F. P. Moreno)—to elucidate the geology and the mammalian fossils; and Dr Florentino Ameghino, who is an accomplished zoologist and comparative anatomist, commands a respectful hearing, if only on account of the remarkable contributions he has made to our knowledge of the Tertiary mammals and birds. We have already referred to the progress of his researches on several occasions in *Natural Science*.

It is well known that, according to our present information, the chief types of the higher mammals all suddenly appear both in Europe and North America at the dawn of the Tertiary period. We are acquainted with old land surfaces of the late Secondary period in both countries, but hitherto we have not found a trace of the ancestors of the higher Tertiary mammals on any of them. Dr Ameghino now claims to have discovered these long-lost ancestors of the Cretaceous period in Patagonia. He believes in the theory of an Antarctic Continent, which split up at the beginning of the Tertiary period into South America, New Zealand, Australia, South Africa, and less important islands. Here he considers that the Mesozoic ancestors of the mammals were evolved. He believes that they first wandered into the Euro-Asiatic Continent at the end of the Cretaceous period from South Africa, which then became directly connected with the lands of the northern hemisphere. These mammals passed directly from the Euro-Asiatic Continent by a land-bridge into North America. Then the isthmus of Panama was formed, and many of the later Tertiary mammals were able to wander back to the land of their primeval ancestors in the direction of Patagonia.

The theory is a pretty one, and we only wish the facts supporting it were more convincing; for some theory of this kind would explain many mysteries in the distribution of animals. For our own part, we cannot recognise the very antique and ancestral features which Dr Ameghino perceives in his '*Pyrotherium*-fauna' from Patagonia; but we must await the promised memoir in which the remarkable new mammals in question are to be fully described.

The Geology of Patagonia

The interest aroused in the age of the tertiary deposits of Patagonia will be still further fostered by a forthcoming paper by Mr J. B. Hatcher of Princeton University, who visited the district in 1896. Mr Hatcher has already recorded a few notes in the *American Journal of Science* for September. In south latitude 51° 31′ he discovered, near Cape Fairweather, a series of marine beds with a fairly abundant invertebrate fauna, overlying the Santacruzian formation, which in that locality are well-developed and full of fossil mammals. These Fairweather beds, as Mr Hatcher has named them, have been deposited upon an eroded surface of the Santacruzian formation, and consist of some 30 to 40 feet (as at present observed). The lower part is fine-grained, incoherent sandstone, the upper a coarse, loose, but in places an extremely hard conglomerate, which passes insensibly into the overlying Patagonian shingle formation, from which it can only be distinguished by the fossils it contains. The marine invertebrata, according to Prof. Pilsbury, point to a Pliocene age, but they do not promise to be of much service in determining the vexed question of the age of the Santacruzian beds. Mr Hatcher at present believes that the Fairweather beds are the equivalent of those beds discovered by Darwin in North-Eastern Tierra del Fuego, and provisionally referred by him to the Santacruzian beds discovered by Fitzroy at the mouth of the Gallegos river, and he has, in support of his view, fragments of crabs' legs very similar to those which occur in the bluffs of San Sebastian Bay. The general dip of the strata also lends colour to his deductions. We shall await with interest the more detailed report which Mr Hatcher promises.

The Sirenian Mammals

The want of all definite knowledge of the ancestry of the Tertiary land-mammals is strange. Our absolute ignorance of the origin of the marine mammals like the whales, dolphins, and sea-cows is still stranger. Marine deposits of the Cretaceous and early Eocene periods are recognised nearly all over the world, but not a trace of the Cetacea and Sirenia has been found in them. So far as known, these curious types appear fully evolved at the top of the Eocene.

Nor does embryology help us much. It has shed a little light upon the nature of the Cetacea; we might therefore expect some information from this source concerning the Sirenia. Thus far, however, the results are small, and Prof. Willy Kükenthal's new memoir on the Sirenia (in Semon's "Zoologische Forschungsreisen in Australien und dem Malayischen Archipel," vol. iv., lief. 1), which is one of the most important monographs issued during the past month, does not contribute much to the solution of the great problem. The available material, it is true, is small—only four stages of *Halicore* and six stages of *Manatus*—and only three chapters (external form, integument, and dentition) are published. It is a most important contribution to the facts of the case, and for this alone we must at present remain grateful.

The Origin of the Irish Fauna

Not only is it almost impossible as yet to fathom the mysteries connected with the dawn of the present order of things in the distribution of life on the various land-areas; it is very difficult to discover the routes of the migration and distribution of organisms even during comparatively modern periods. During the last few years, however, much attention has been paid to the relations of the existing faunas and progress made in the determination of their affinities. Among others, many Irish naturalists have discussed in a very interesting manner the relations of the fauna of their island, and have arrived at various conclusions, some of which may prove to be of permanent value.

Quite recently Dr R. F. Scharff, Keeper of the Natural History Collections in the Dublin Museum, has returned to the subject, and published an interesting paper in the *Proceedings* of the Royal Irish Academy (ser. 3, vol. iv., No. 3, 1897, pp. 427-514). The contribution is lengthy, and it is written in a somewhat disconnected style; and how the author gets from his premises to his conclusions is not always apparent. Dr Scharff argues that part of the Irish fauna lived in Ireland in pre-glacial times; that the lower continental boulder-clay is Pliocene; and that it is a marine formation deposited in a great sea which covered a large tract of Russia and Central Asia; that the Siberian mammals migrated into Western Europe to the south of this sea; and that the British Pleistocene fauna and flora do not indicate former Arctic conditions in this country. The range of subjects discussed in this memoir is considerable. The conclusions are startling, but only when considered apart from the statements on which they are based. "The occurrence in almost all the English boulder-clays of marine shells" is an example of Dr Scharff's sensational statements. The paper cannot be discussed in a short notice. Our chief fear is that Dr Scharff's speculations will prejudice the use of zoological distribution in geological investigation.

Franz Josef Land

Captain Robertson, the enterprising commander of the whaler *Balaena*, has given an interesting description of his voyage this summer to Franz Josef Land. His geographical discoveries are interesting. He found some new islands on the south coast, but his most important achievement was returning westward from Franz Josef Land along the 79th parallel of latitude. He thus passed over the site of the two famous islands reported by Johannesen and Andreassen in 1884; but he found no trace of them. The Norwegian seamen must, therefore, have been out in their reckoning. The sea this summer was exceptionally free from ice, and the polar pack had receded far to the north. Captain Robertson thinks that in such a ship as the *Balaena* the whole Franz Josef Land archipelago could be charted in a single summer. This opinion renders the results of the Jackson-Harmsworth expedition all the more disappointing so far as can be judged from the accounts already published. But now that the expedition has returned we may hope for a final account of its work by the members themselves. Perhaps this may remove the somewhat widespread prejudice roused by the unjust publication of private letters and the injudicious advertisement of the London agents. The expedition is said to have cost some £49,000. We hope Mr Harmsworth is satisfied.

Mimicry and Protective Colouration

The questions of protective colouration and mimicry have a perennial interest for naturalists and the general public. Now that the conclusions of Trimen, Bates, and Wallace are being dogmatically taught in magazine articles and popular books, it is only to be expected that they should begin to be discredited by some of the younger school of biologists, and several of the works attacking the 'orthodox' theories have been noticed in *Natural Science*. In defence of the old positions we notice a short paper by Mr F. M. Webster of Ohio, in the Report of the Entomological Society of Ontario for 1896 (pp. 80-86). The author minimises the value of experiments, tending to show that insects with 'warning' colours are not always distasteful. He points out that the fact that Prof. Plateau enjoyed feeding on the caterpillars of the magpie moth does not prove them palatable to more usual enemies. No insect is so familiar an example of 'warning' colour as the North American danaid butterfly, *Anosia archippus*. Mr Webster narrates an instance of a number of these butterflies being eaten by mice in Texas, and tells how he himself observed a colony of brightly-coloured cabbage bugs (*Margantia histrionica*) devoured by the same rodents. But he believes that mice must be very exceptional enemies to these species, and that the bright colours may be of 'warning' value to animals that eat insects habitually.

The same author in another paper (*Journal New York Entom. Soc.*, 1897, pp. 67-77) deals with the mental or instinctive factors in protective resemblance. It is well known that in addition to the form and colour of the insect, a special attitude or a position on some particular background of leaf or twig is essential to the perfection of the illusion. Mr Webster believes that such habits have not been developed without the action of some conscious will and intelligence on the part of the creatures concerned. He compares the young twig-like caterpillar to the human infant who has inherited none of the accumulated knowledge of his ancestors, though he has inherited an aptitude for learning.

The Effect of Civilisation on the North American Insect Fauna

Another subject of general interest, lately dealt with by Mr Webster (*Fifth Annual Report of the Ohio State Academy of Science*), is the effect of civilisation on the insect fauna of North America. In few other parts of the world has so rapid a change been made in the natural aspect of the country by the advent of the pioneer and the farmer, and the transformation of swamps and forests into cultivated fields has led to the extinction of many native species of insects. Some species, however, have adapted themselves to the changed conditions, while a considerable introduction of Tropical and European forms has been a direct result of the advance of civilisation. Mr Webster rightly lays stress on the importance of systematic observations on the natural history of new countries, wherever possible, before the balance of nature has been disturbed by the advent of the white man.

Aspidiotus

The San José scale (*Aspidiotus perniciosus*) is a subject of perennial interest to the American entomologists. Last year the U.S. Department of Agriculture issued a pamphlet by Messrs Howard and Marlatt on the spread of the insect in the States, and now from the same Department we receive a bulletin (No. 6 Technical Series) on the systematic position of the scale, and the structural points which distinguish it from its allies. This work is from the pen of Mr T. D. A. Cockerell, and it will prove of great value to the student of the coccids, as it contains not only full descriptions and figures of *A. perniciosus* and the species nearly related to it, but a geographical list of all the described species of *Aspidiotus*, with a short summary of their characters.

Care of the Brood in Holothurians

Prof. Hubert Ludwig of Bonn writes to the *Zoologischer Anzeiger* to say that he is able to record an antarctic *Chirodota*, in which the care of the brood is well marked. The species is *Chirodota contorta*, was described in 1874, and forms an abundant part of the material obtained by the Hamburg-Magellan Collecting Expedition. Prof. Ludwig says that in this species he has discovered a form of care of the brood previously unknown among echinoderms. In the female (the sexes are separate in the species) the genital canals themselves become receptacles for the brood, and the entire development is passed through within them. The young at 3 mm. in length are born through the genital aperture; they have then seven tentacles, and the 'wheels' and 'hooks' are already well developed. Further details of this discovery of Prof. Ludwig's will appear in his forthcoming memoir on Antarctic Holothuria.

Solifugae

When revising a genus or describing a whole series of new genera and species, the average describer looks upon all details of habit or economy as beneath his notice. Not so Mr R. I. Pocock, who frequently appends to his papers notes as interesting to the general reader as important to the cabinet naturalist. In the September number of the *Annals and Magazine of Natural History* Mr Pocock deals with the group Solifugae, which contains arachnida of the genera *Galeodes*, *Solpuga*, etc., coming from tropical Africa. After a revision of the family and a description of the genera and species we find a note on the sound produced by a Natal species of *Solpuga*.

Mr G. H. K. Marshall, the observer, seems inclined to attribute the sound produced to "trituration of the creature's powerful jaws against the hard ground in which they seem to prefer to dig their holes, the operation being performed with the jaws, and the sound ceasing when the spider stops digging." Although Mr Marshall kept them alive he failed to detect any stridulation, though they made a considerable noise by energetically biting at the sides of the boxes, one of them nearly succeeding in escaping by gnawing its way through at one spot. A further note is to the effect that the Solfugae succumb more rapidly to the cyanide bottle than the ordinary spiders or scorpions; and Mr Pocock, in quoting Mrs Monteiro, to the effect that a large black scorpion was confined eight hours in a strong poison bottle before it succumbed, states that this is no doubt due to the fact of the richer development of the respiratory system in Solpuga. A further note of Mr Marshall's corroborates Hutton's observation as to the use of the terminal organ on the palpus. This is a gelatinous fan-shaped sucker with which the animal has the power of picking up objects, probably prey, and conveying them to its mandibles. The principal food of the Solpugae, according to Mr Marshall, are termites, "a small species which makes no mound, but builds mud tunnels along the surface of the ground among dead leaves, sticks, etc. When the Solpuga comes across such tunnelling it examines along it carefully, then suddenly breaks through the mud and extracts a termite, the presence of which it detects, I suppose, by either hearing or touch."

The evidence as to the poisonous nature of these animals varies. A Kaffir boy declared them very poisonous, sometimes fatally so, and a bite supposed to be from *S. darlingii* did not subside till the fourth day; on the other hand, Mr J. M. Hutchinson of Estcourt, Natal, finds the bite of *S. hostilis* "to be quite harmless, the forceps being unable to pierce the tenderest skin."

The British Pleistocene Mollusca

In 1890 a valuable summary of the Pleistocene (non-marine) mollusca of the London district was published in the *Proceedings* of the Geologists' Association by Mr B. B. Woodward. This paper treated the material from a geographical point of view, describing the geology of the localities where the shells were found, and concluded with a valuable table of distribution, in which were distinguished the living and extinct species. It was hoped that Mr Woodward would extend his researches into other districts, and we have now to welcome a second paper by Mr A. S. Kennard and himself, to which Mr W. M. Webb has contributed, on the Post-Pliocene (non-marine) mollusca of Essex (*Essex Naturalist*, x., pp. 87-109). This paper is treated in practically the same way as the former, with the addition of a bibliography, and deals with that important series of shells obtained by the late John Brown of Stanway at Copford, as well as series from many other deposits. The mere mention of Grays, Ilford, and Clacton, as some of these other deposits, will show at once the special interest of the paper to London geologists and conchologists.

Among the more interesting notes given to us is the confirmation of the rarity of *Helix hortensis* in a fossil state; the absence of *H. pomatia;* the occurrence of *H. aspera* in the Lea Valley; the elimination of *Eulota fruticum* from the recorded fauna of Copford; the restriction of the distribution of *Pomatias elegans;* and the observation as to the increased size of *Helicella caperata* since Pleistocene times.

We have now a great advance in our knowledge of the geological history of the non-marine mollusca of our home district; and though some of the names in Messrs Kennard and Woodward's list may be a little startling to the uninitiated, we are glad to see a possible termination of the confused nomenclature which has prevailed for so many years.

Triassic Cephalopoda

The description of Triassic Cephalopods occupies an important part of the recently-issued volume of the *Denkschr. d. k. Akad. Wissensch., Wien.* Franz v. Hauer, who has been contributing to the literature of Triassic Cephalopods for more than thirty years, and although now considerably past his threescore years and ten, furnishes a paper on the Trias Cephalopods of Bosnia. This author has already described Triassic Cephalopods from this region, but he now records from a new locality both Nautiloids and Ammonoids, among the latter being the new genus *Bosnites.* Dr E. von Mojsisovics, so well known for his work on the Trias Cephalopods, contributes a very important paper on the Upper Triassic Cephalopod-fauna of the Himalaya. It is based not only upon the older collections made by Strachey, Stoliczka, and Griesbach, but also upon the rich collection obtained by Messrs Griesbach, Middlemiss, and Dr Diener during their expedition into the Central Himalaya in the year 1892. As was to be expected, many new species are described and not a few new genera are proposed. The author points out that there is a marked contrast between the Upper Triassic fauna of the Indian province and the homotaxial fauna of the Mediterranean province, but he believes there was a sea connection between the two regions during Upper Triassic times, and is of opinion that an examination of the intervening districts will probably render the provincial character of these two regions less apparent.

Lobster Fishery

Mr James Hornell has contributed two long letters to the *Jersey Times* and the *Jersey Evening Post* relative to the Lobster Fishery of the Channel Islands. There is a marked and general decrease in the size of the catches, and some arrangements are needful for regulating and preserving the supply. Mr Hornell is of the opinion that the geographical position of the Channel Islands precludes—in view of the powerful currents sweeping their coasts—any useful purpose being served by the hatching and liberation of fry, wherever fry are surface swimmers for any considerable length of time. He does not forget in his argument that the currents may reverse their direction at regular times, but urges the importance of a detailed and exact investigation of current action around the islands before costly means are undertaken for stocking purposes. And at the same time he casts doubts from his own observation on the accepted idea that the young lobster is a pelagic animal, because he has found that in some experiments he has made that while he lost most of the fry by the surface pipe of his aquarium, those of the age of three days seemed inclined to sink to the bottom and abandon a surface life. Again, Mr Hornell has never once taken lobster fry in his almost continuous towings with fine muslin nets on the south coast of Jersey, while the fry of crabs, prawns, and *Squilla* occur in countless thousands. His method for the improvement and protection of the Lobster Fishery would be to rigidly enforce the protection of the berried female and all lobsters under nine inches, rather than to commence a nursery, both costly and difficult to manage. At the same time, if experiment were to prove the non-pelagic nature of the fry, then culture and liberation might be useful, in addition to the protective regulations referred to above.

Pre-Cambrian (?) Radiolaria in Australia

Prof. Edgeworth David and Mr Walter Howchin announce in the *Proceedings* of the Linnean Society of New South Wales, pt. 4, 1896, the discovery of Radiolaria in rocks of supposed Pre-Cambrian age in the neighbourhood of Hallett's Cove, about fifteen miles S.S.W. from Adelaide. The fossils occur in a dark, greenish-grey silicious limestone, and in a fine-grained laminated grey clay-shale, but they are very obscure and badly preserved.

Although no other fossils have been found at Brighton and Crystal Brook in the rocks in which the Radiolaria occur, there is a rich and abundant fauna in the Cambrian series of the district; but in a subsequent note to this paper the authors state that they have found a great number of *Archaeocyathinae* at Normanville in limestone, which "appears to be conformable to strata which most resemble those in which the radiolarian casts have been observed." Hence they are inclined to believe that the Radiolaria may be in Lower Cambrian or passage beds rather than Pre-Cambrian.

Freezing of Plants

Messrs Gustav Fischer, of Jena, have just issued, in book form (73 pages 8vo), an account of some researches by Prof. Hans Molisch on the freezing of plants. The author, by means of an arrangement which he describes in the first chapter, has observed under the microscope the changes which occur in freezing not only in plant-cells and tissues, but also in colloidal substances, emulsions, coloured liquids, and salt-solutions. For observation the microscope is placed in a triple box. The outer case is of wood, then comes a hollow-walled zinc chamber, inside which is fitted the instrument, the tube projecting through the top. Sawdust is placed between the outer wall and the zinc chamber, and the hollow walls of the latter contain the freezing mixture. A wide zinc tube allows light to pass from the outside to the reflector. Several figures are given, showing the appearance of non-living substances as freezing. In all cases particles of ice are formed by separation of the water, while the gum, particles of latex or concentrated salt-solution occupy the intervening spaces. Three figures (p. 17) of an amoeba, alive, frozen, and thawed respectively, are of interest. In the frozen state the organism forms a lump of ice intersected with a highly complicated network, consisting of protoplasm very poor in water, concentrated cell-sap and air-bubbles. When thawed there is a much less sharply defined reticulum of dead protoplasm, the lacunae in which are the spaces which in the frozen state were filled with ice. *Spirogyra* cells (p. 22, fig. 10) in freezing lose about half their diameter by withdrawal of water, which then freezes on the outside; on thawing the cells swell to their original size, but protoplasm, chlorophyll band and nucleus form a disorganised central axis between which and the walls is contained the water.

After experimenting for five winters with hundreds of objects, the author comes to the conclusion that, as a rule, it is immaterial to the preservation of the life of the object whether thawing is rapid or slow, and that death by freezing is the result of an excessive loss of water, through ice formation, if the protoplasm by which its structure ("architektur") is destroyed, and that all the facts of the case can be easily and naturally explained from this point of view.

Funafuti

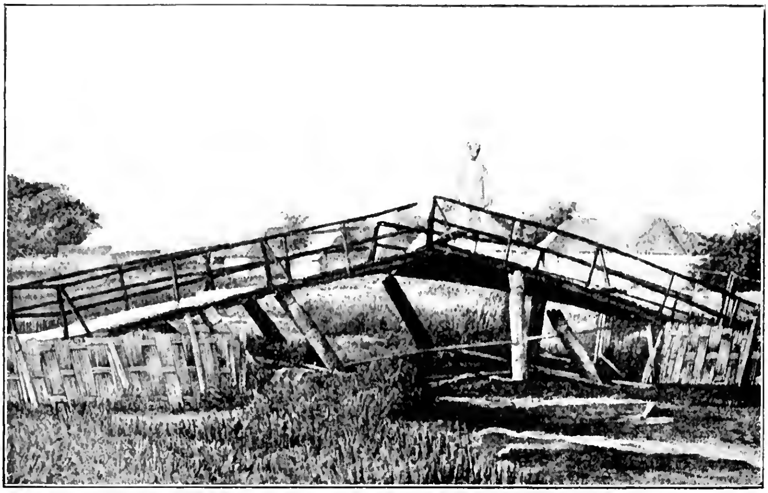
The third part of the Memoir of the Australian Museum on the Atoll of Funafuti contains further interesting additions to knowledge of the zoology of that island. Mr E. R. Waite has described the collection of mammals, reptiles and fishes made by Mr Hedley. The most interesting part of Mr Waite's memoir is an account of the habits of the fruit-eating Pacific rat, for which, following Thomas, he adopts Peale's name of *Inus exulans.* An interesting fact is recorded in reference to the edibility of fishes: at the time the expedition was on Funafuti the natives would only eat fish caught in the lagoon, all those from the reefs being condemned. The native explanation is that the pumice which was then being washed on to the beach rendered the fish poisonous; but as the pumice is harmless, Mr Hedley concludes that some marine organism arrived with it which rendered the fish unwholesome. Mr Waite quotes a remark of Wyatt Gills that good food fish become poisonous by eating the worms of the genus *Nereis.* The two species of Enteropneusta collected are described by Mr J. P. Hill; one of the two is a new species (*Ptychodera hedleyi*). Mr Whitelegge's account of the Alcyonaria includes a description of four new species and a redescription of several previously very imperfectly known. We regret to find that some remarks concerning the publication of this Memoir, made in reference to Part II. (*Natural Science*, July 1897, Vol. xi., p. 5) were based on a misunderstanding.

The Great Indian Earthquake

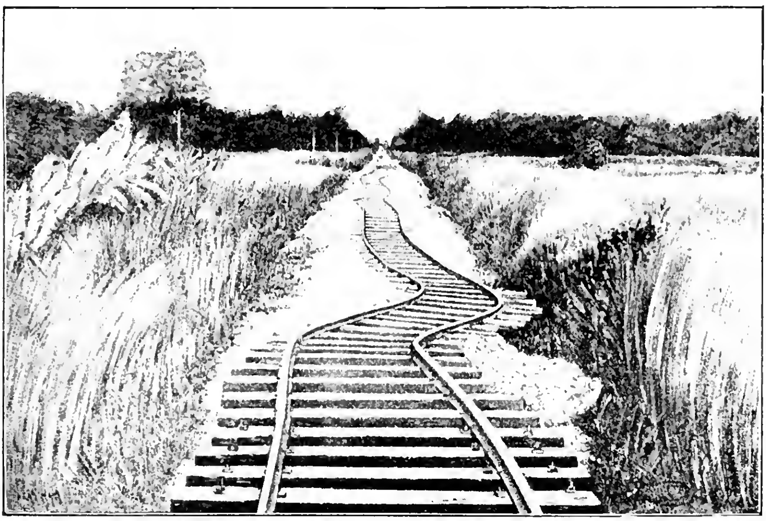
At five o'clock in the afternoon of June 12, 1897, Calcutta and north-eastern India were startled by an earthquake which is regarded as having exceeded even the famous Lisbon earthquake in the area affected. The Geological Survey of India immediately set to work to collect the data for a complete investigation. An immense amount of information has already been obtained, which it will take considerable time to digest. Sufficient has, however, been done to enable Mr R. D. Oldham to contribute a preliminary note to the Records of the Geological Survey. The area affected by the shock included more than a million and a quarter square miles, while its effects appear to have been felt even in Edinburgh and Rome. The shock was most destructive in Assam: at Shillong in the Khasi Hills it is said that hardly one stone has been left standing on another. Heaps of road metal have been scattered into layers a few inches deep. All masonry has been shattered into pieces, so that the roofs fell bodily down on to heaps of ruins. A cylinder seismometer had fortunately been erected at Shillong in 1882, and this was thrown to the north-east: it enables the velocity of the wave to be calculated. The range of motion is estimated at 7·4 inches. So Mr Oldham concludes that "the violence of the shock at Shillong was at least equal to a backward and forward shake of 7 inches repeated sixty times a minute." All telegraphic communication was of course destroyed, and the accompanying illustration (reproduced from a plate of the Indian Survey Records) shows the effect on the railway lines produced by the movement of the surface soil. The rate of transmission of the shock was over 100 miles a minute. The fuller account promised will be awaited with much interest, for it will probably yield suggestive information as to whether the Himalayan movements are still in progress. It is fortunate that the work will be carried on under the supervision of Mr Oldham, who is keenly interested in all the broader problems connected with seismic movements.

Geology in New South Wales

The most noteworthy point in the recently issued Report of the Geological Survey of New South Wales is the discovery of Devonian plant remains and Lower Silurian graptolites by Mr Joseph Carne. This is the first identification of Lower Silurian Rocks in the Colony, and they are of special interest in that they contain in the neighbouring Colony of Victoria the famous saddle reefs of Bendigo. The graptolites were found in a black slate in the Parish of Lawson, Wellesley Co., and occur as shiny films. Mr W. S. Dun identifies them as *Didymograptus furcatus* (Hall), *D. extensus* (Hall), *Dicranograptus*, *Diplograptus*, and *Phyllograptus*. The Devonian plants comprise a Pecopterid fern and a *Sphenopteris*. They came from Genoa River, Co. Auckland.



BRIDGE ACROSS NALLAH AT HALDIBARI



LINE BETWEEN HALDIBARI AND MOGHAL HAT

I

The Fundamental Principles of Heredity

IN the recent elaboration of the Theory of Descent, as first fully published by Charles Darwin, two schools of thought have arisen. The one, though professing discipleship pure and simple, has laid extreme stress on the principle of Natural Selection, which owes so much to Darwin, but has rejected his belief in the internal tendencies of races to vary in adaptation to changed surroundings; while the other has attributed the greater share in the transformation of species to the latter factor, and sent Natural Selection in the background. The two most illustrious leaders of scientific thought have been August Weismann on the one side and Herbert Spencer on the other. Their debates have long since obtained an audience among the cultured laity; but while the arguments are well known, some of the most important facts have been rather taken for granted than fully stated and clearly co-ordinated even in the scientific press. I allude especially to the coarser relations of the actual mechanism of reproduction and of the act of transmission from one generation to the next of the form which clothes on or assumes the parental characters. Such an exposition as we have to make cannot be limited to the higher organisms which are familiar to us in our daily life, for these are complex elaborations; while the primitive types, though still existing abundantly, are only to be studied with the microscope. It is in this field, hidden if not buried, that we must first labour, if we wish to rightly understand the foundations of the wonderful superstructure of the higher Organic Kingdoms. We shall endeavour to use as few unfamiliar terms as possible, bearing in mind that the reader has no Handy Atlas to help him in following the exploration of this foreign country, with its outlandish names.

Only two centuries ago the microscope revealed to mankind an immense world of minute living creatures as well as the details of the structure of the familiar Animal and Plants. Naturally enough the early observers, or 'philosophers,' as they were then called, inferred that these strange small creatures must have as complex a structure as our own. They proceeded zealously to search for, and sometimes to proclaim, the existence therein of brain, heart, blood-vessels, etc., just like those of ordinary bird, beast, or fish.[1] Since then we have learned that the ultimate units of structure of the familiar organisms are identical in character with the entire organism of one of such microscopic being; and the search we have referred to would be now regarded as equivalent to seeking in a limestone pebble the pillars and buttresses, the vaults and domes of a great cathedral in miniature. Such units of structure are called 'cells,' an ill-chosen term indeed, whose signification, however, as a nucleated unit of protoplasm, is familiar to everyone. The lower organisms consist of single cells or of aggregates of similar cells; the higher ones consist of complicated arrangements of those dissimilar aggregates of cells which we call tissues. The former we call Protists, distinguishing between Protozoa and Protophytes according as the mode of existence is animal or plant-like; the higher animals and plants we term Metazoa and Metaphytes respectively, the appropriate conjoint term, 'Metists,' not having been coined by any recognised authority.

Throughout the higher groups the act of reproduction[2] of the race consists in the separation from the complex organism of single reproductive cells, which may either independently grow up into the original form, or else one with another fuse to produce a new cell which grows up. Again in most Plants and many Animals multicellular portions of the body may become detached, and finally develop into complete organisms; this we shall call 'propagation,' not 'reproduction.' In either case the parent body continues to exist, alive or dead, after the detachment of these cells or groups of cells. In Protists, matters are very different; for here, when the cell individual has attained its full size, it usually divides into two new cells, and itself is no more, alive or dead. We call the original cell a 'mother cell,' the new ones 'daughter-cells,' by a convenient metaphor; but we must remember that the devoted mother here absolutely merges her very existence into that of her offspring, a self-denying type of maternity often imagined but never realised among ourselves. Thus as Weismann first explicitly stated, the Protists may escape personal death by the sacrifice of their individual life; he therefore terms them 'immortal.' It is with cellular pedigree, according to the mode of parentage we have just explained, that we shall mostly have to deal in this paper.

The modes of reproduction among Protists are many and various.

[1] Thus Baker writes in the middle of the last century: "Search we further and examine the Animalcules—many Sorts whereof it would be impossible for an human Eye unassisted to discern; those breathing Atoms, so small they are almost all Workmanship! in them too we shall discover the same Organs of Body, Multiplicity of Parts, Variety of Motions, Diversity of Figures, and Particular Ways of Living as in the larger Animals.—How amazingly curious must the Internal Structure of these Creatures be! The Heart, the Stomach, the Entrails and the Brain. How minute and fine the Bones, Joints, Muscles and Tendons! How exquisitely delicate beyond all Conception the Arteries, Veins and Nerves!" ("The Microscope Made Easy," by Henry Baker, ed. v., 1767.)

[2] In the limited sense, distinguished from 'propagation,' as defined immediately.

The most familiar is the simple halving of the cell each time it has attained double its original bulk (Herbert Spencer's 'limit of growth'), a process termed in Hibernian phrase 'multiplication by simple division.' Sometimes, however, the first division is followed immediately by another, and so on, so as to produce with little delay grandchildren or great-grandchildren, &c.; this process is called 'brood-division,' or, when the progeny do not immediately separate, 'segmentation.' Again the progeny of brood-divisions may assemble in groups, usually in pairs, which fuse to form a new or 'coupled-cell'; this process is called 'conjugation,' or, if the 'pairing-cells' are dissimilar, 'fertilisation.' We must bear in mind that conjugation processes are not, strictly speaking, processes of multiplication; for the act of pairing halves the total number of cells for the time being, one replacing two: the two literally become one flesh.

We very often find these three reproductive processes recurring in cycles, *e.g.*, a succession of simple divisions at the limit of growth is wound up by brood-formation, and the brood-cells conjugate; the coupled-cell then initiates a fresh cycle. But the order of the processes varies in different cases, and sometimes even different modes of brood division may alternate. Thus a common Gregarine, parasitic in the Earthworm, shows the following: after conjugation the coupled-cell undergoes repeated brood divisions so as to form many hundred of brood cells; each of these matures into an oat-shaped body surrounded by a hard shell. After a time the oat-shaped cell divides again by brood formation into eight sickle-shaped cells, which finally leave the oat-shaped case and migrate into the living cells of the worm.

In many cases the separation of the daughter- or brood-cells is not complete, and they remain associated in more or less close union. Such an assemblage of cells of common origin is called a biological 'colony' in the strict sense, the term 'social aggregate' being used for an assemblage formed like a human colony by the flocking together of originally isolated organisms. Protist colonies may be formed in three ways, the third being only a combination of the first two:

(1) Cell division, alternating with intervals of growth, gives rise to daughter-cells which remain united together.

(2) Brood division (segmentation) produces a number of cells which remain united together.

(3) A colony first formed by segmentation continues to enlarge by the division after growth of its several cells, the daughter-cells still remaining connected.

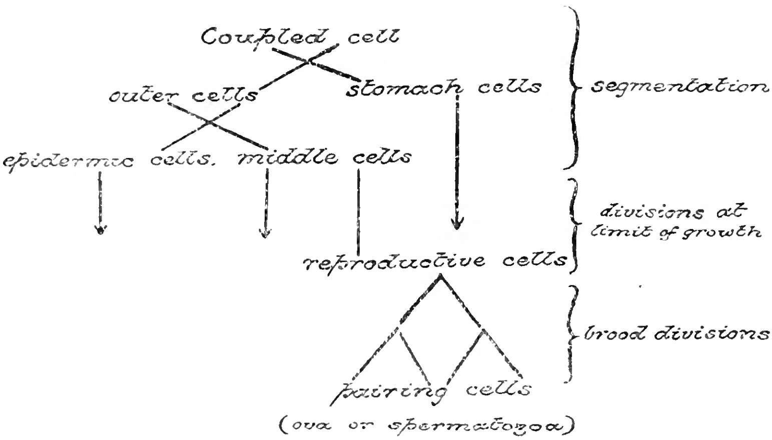
Colonies of the first and third type may be propagated by the separation of a part of the colony; if the separated part consist of a single cell this merges into true reproduction.

In the most primitive colonial Protist, all the cells of a colony are practically alike; and the colony ultimately breaks up into its individual cells, which reproduce in one or other of the ways described above. But in some cases the colonial habit has induced differentiation among the cells. There is a striking example of this in *Protospongia haeckelii* (a small organism found in pond-water by Savile Kent), which consists of a large mass of cells united by a gelatinous secretion. Those at the outside of the mass are provided with a waving lash, the base of which is surrounded by a funnel or collar of protoplasm. These cells take in the food particles brought into contact with them by the waving of the lashes in the surrounding water; while the cells at the centre of the colony appear to be only indirectly nourished by the food, which is digested and transmitted to them from the collared-cells. Our knowledge of the life-cycle of the organism is still very incomplete, but it appears certain that only the central-cells can truly act as reproductive cells by segmentation, while outer-cells may possibly separate to propagate the race also by the slower process of nutrition and growth, followed at intervals by simple division. We might almost regard this as a Metazoon with two tissues—the outer one nutritive, the inner reproductive, and ascribe the specialisation to the relative position of the two layers: the outer one is favourably situated for obtaining food from the ambient water; while the inner, debarred from all activity by its position, and fed and sheltered from the stress of contact with the unkind world by the outer layer, devotes its energies to the reproduction of the species.

Indeed, this organism, as its name implies, is, as it were, a forerunner of the Sponges, and probably represents a last survivor of their ancestral type. For a simple Sponge is a sack attached by the bottom and widely open above, with the wall pierced by numerous pores. This wall consists of three layers, an outer epidermic layer, an intermediate layer, and an inner or stomach layer, the cells of the last possessing lash and collar. The lashes of the stomach-cells produce a constant current of sea water through the sack, which passes in through the pores and out through the mouth, and brings with it the food particles which the stomach-cells alone can take up, the two other layers being nourished by them. In this case it seems that only such fragments of the Sponge as contain all three layers can propagate it; and in nature, indeed, hollow outgrowths of the sack are formed as branches, and may even be detached as buds. But only the intermediate layer, sheltered as it is on every side, differentiates certain cells as reproductive-cells. These by brood divisions produce male and female pairing-cells; and the coupled-cell after fertilisation grows up into a fresh Sponge. We have here a very marked advance on the primitive colonial

Protists; for here the colonial organism can only be propagated by the co-operation of all three kinds of cells. The individual cell is no longer a Jack-of-all-trades, but it has been so specialised that it needs the association and co-operation of cells specialised in other directions to form a complete self-sufficing organism; and each kind of cell can by growth and division only reproduce its own type and tissue; but not the complete organism of which it formed a part. This has been aptly termed by Orpen Bower a process of sterilisation.

We have noted the richer endowment of certain of the intermediate-cells. We must now follow up the fate of the coupled-cell (fertilised egg, oosperm). This divides afresh repeatedly, and by its segmentation gives rise to a hollow spherical colony, one hemisphere being composed of smooth cells, while the other is provided with lashes. The latter now sinks into the former so as to give the colony the form of a lined skull-cap. The lining is composed



of collared-cells, which are the stomach-cells; the outer layer of cells again divides into two layers, the epidermic and middle cells respectively. This is essentially the processes of reproduction and early embryonic found growth in all Higher Animals, save that the middle layer may be formed from the inturned cells instead of, or as well as, the outer ones, and that the reproductive cells may be formed in different layers in different classes. The annexed genealogical table, starting with the coupled-cell and ending with the pairing or sexual cells, represents the cellular pedigree in a Sponge.[1]

From the above it is clear that the coupled-cells, though they are descended from middle-cells only, yet produce by their divisions offspring that ultimately become cells of kinds which are different, and have never been in the line of their direct descent. We might compare this with a race of which the older and the younger

[1] In this and the tables to follow we use the signs X to indicate segmentation, ΛΛ to indicate brood divisions, and II to indicate divisions alternating with growth.

members of a family were always sterile and different in character and endowments from the intermediate, fertile children, but where every fertile couple produced among its progeny some resembling the parents, others with the endowments and characters of the sterile uncles and aunts;[1] we must, however, bear in mind that any comparison of a strict cellular pedigree with the genealogical table of the members of a Metazoan race is only an analogy.

While the main features of reproduction in the Higher Animals run on the same general lines as the Sponges, certain of them may present differences; and especially, as above noted, the relation of the middle and the reproductive cells to those of the two original germ layers respectively, varies in different groups.

Propagation by budding in the higher animals, and regeneration, or the repair of injuries, are essentially two different aspects of the same phenomenon. In both cases the cells of one or more tissues multiply rapidly, and revert more or less closely to the state they possessed in the developing embryo. In some cases these 'embryonic cells' can only give rise to tissues like those they respectively sprung from, or, at least, to tissues belonging to the same layer; but in the lowest Worms the middle-cells are capable of thus forming other layers. In the Vertebrata the regenerative functions are strictly limited; thus, if the surface of the skin is completely removed over an ulcer or burn, the new epidermis only grows over by gradual extension of the living epidermis at the edges, not by its direct growth upon the raw. This is the rationale of the modern practice of 'skin grafts,' which implanted at intervals over the surface of a healing wound give so many centres for the new overgrowth of epidermis to start from, thus accelerating the process of 'skinning over.'

Most tissues of the Higher Animals retain sufficient 'vitality' to be able to enter at once on processes of regeneration of their own individual kind in cases of wounds; and in the Newts, for instance, even a complete structure like a limb or an eye can be renewed after amputation. The epiderm of Vertebrates retains in its deepest layer an almost indefinite power of growth and reproduction, the cells next the true skin forming a continuous stratum, each cell of which is constantly growing and dividing, the upper cell at each division becoming horny, to be ultimately cast off as other horny cells are formed beneath it, while the lower retains the original power of growth and division. This layer is absolutely comparable to the layer of cells that forms cork in most green plants. The periosteum or layer of cells overlying the bone has similar but less active powers.

[1] The case we have suggested for comparison is actually found in social Insects with their 'sterile castes' in each generation.

Reviewing the facts, we find that

(i.) In Protista, each cell retains the power of reproducing in its offspring its own characters or those of a direct ancestral cell, which we may term the law of direct cellular transmission uninterrupted or alternating, according as only one or several alternating modes of cellular reproduction constitute the genetic cycle.

(ii.) In Metazoa, the power of reproducing a complete organism is confined to certain reproductive cells, which must beget in their progeny cells like those which are only related to them collaterally; this we call the law of collateral cellular transmission.

(iii.) The remaining cells of the Metazoan can seldom or never revert closely enough to a primitive type to produce all those other tissues of which they are collaterals, though their propagative power may be very great. This limitation of reproductive power we may call the law of specialised sterility.

(iv.) In most cases of animal budding or repair we find that the several tissues co-operate to produce a complete organism; this we call the law of co-operative propagation.

MARCUS HARTOG.

(*To be continued.*)

II

The Place of Isolation in Organic Evolution

ALTHOUGH most writers on evolution mention the subject of isolation, very few attach much importance to it, Professor Cope even considering it as a function of natural selection,[1] which is putting the cart before the horse. This neglect of isolation is probably due to the term 'selection' having been used in such a variety of ways and having been made to include almost every process in evolution, even the origination of variations. But such indiscriminate use of a word which has a very definite meaning is objectionable, for it confuses in our minds several totally different things. To me it seems self-evident that all the known factors of organic evolution should be arranged under two heads: (1) the origin of variations capable of being transmitted by amphimixis or by environment, or by use and disuse, or by any other means; and (2) the preservation of variations by isolation or segregation, as it has also been called. Possibly 'internal tendency,' 'kinetogenesis,' or 'action of the environment' may be other causes which tend to preserve variations, but they have not yet been clearly established as such, while there is strong evidence in favour of isolation being the chief, if not the only, cause of the preservation of variations. The subject of this paper is to point out the important part which isolation must play in evolution.

Professor Y. Delboeuf has shown [2] that if in any species a number of individuals, bearing a ratio not infinitely small to the entire number of births, are in every generation born with a particular variation, which is neither beneficial nor injurious, and if it is not counteracted by reversion, then the proportion of the new variation to the original form will increase until it approaches infinitely near to equality. Now the effect of the isolation of a few individuals is to largely increase the ratio of any new variation which may appear among them to the total number of births, and thus to largely increase the chances of its preservation. On the other hand, every variation which arises in a few individuals, and which is subject to the free intercrossing of a large number of other individuals, will tend to disappear. Intercrossing is probably favourable to the production of variations, although it is unfavour-

[1] "Primary Factors of Organic Evolution," p. 386.
[2] Quoted in Murphy's "Habit and Intelligence," p. 241.

able to preserving them; for while cross fertilisation (amphimixis) may stimulate variation, it also prevents the variations from progressing by their mutual interference, and thus it tends to keep a species constant, but ready to vary when circumstances require it to do so.[1] Self-fertilisation, on the other hand, may be unfavourable to the production of variations, but when one does appear it has a good chance of being established.

The general belief that breeding in-and-in is injurious has led to the conclusion that a large and healthy progeny cannot arise from a few parents if they are kept quite apart from all others. But that cross-fertilisation is not necessary for the rapid increase and continued health of the descendants from a few common ancestors is proved by the successful naturalisation of many animals in New Zealand from very limited stocks. The honey-bee was introduced by the Reverend Mr Cotton, chaplain to Bishop Selwyn, who procured a few hives from Sydney; and, in 1866, wild bees were common in the forests of the North Island. Seven Chinese pheasants were introduced in 1851, and six more in 1856—more than half being cocks; and pheasant shooting near Auckland commenced in 1865. A few black swans were introduced by Sir Walter Buller in 1864. A few cirl buntings were turned out near Dunedin about 1868. A very few silver-gray rabbits were released at Kaikoura; and, I believe, only three Tasmanian opossums were turned out in the forests of Southland; yet all these species are now abundant and healthy. The herd of deer in the Wairarapa (Wellington) has sprung from one stag and two hinds turned out in 1863; and the herds in other parts of New Zealand have all started from very few progenitors. Also many of the self-introduced insects—as the English lady-bird (*Coccinella undecimpunctata*), the drone-fly (*Eristalis tenax*), the horse-bot (*Gastrophilus equi*), and *Lucilia caesar*, could only have been introduced in small numbers, for each has spread from a single centre; but yet they have been very successful. It is true that several failures to naturalise animals could also be given, but these failures do not invalidate the evidence supplied by successful naturalisation, and it is evident that, when the surroundings are favourable, it is quite possible for a few individuals to give rise to a new and vigorous species which might in time become dominant.

Isolation must therefore be a true cause of the preservation of variations, and also it must be an important one. The artificial selection of animals and plants by man might just as well be called artificial isolation. The breeder, or the horticulturist, certainly selects the variation he wishes to preserve, but he also isolates it; and it is the isolation which causes the variation to be preserved—

[1] See Professor H. R. Orr's "Development and Heredity," p. 234.

selection only securing that the variation is a good one. The natural selection of Darwin works in the same way: that is, it isolates beneficial variations by killing off the others. It is not so much natural selection as natural elimination; for, as Professor Lloyd Morgan has pointed out,[1] it is not by the survival of the fittest, but by the elimination of the least fit, that new species are made. Isolation by elimination must always tend to preserve variations which are useful to the competing individuals, while isolation by other means may preserve not only useful, but also indifferent and even injurious, variations, as in the case of domesticated animals and cultivated plants.

Isolation by Selection—or natural selection in a restricted sense—implies an outside agency as selector for whose benefit the isolation is made. These cases of true natural selection are limited to selection by insects. Several kinds of beetles, domesticated by ants, have become blind, and some of them are unable to feed themselves. These variations are evidently injurious to the domesticated animals, but useful to the ants, as they prevent the beetles from running away. The sticky secretion of aphides must also be injurious, if we may judge by the eagerness with which they allow the ants to remove it; and we must, therefore, suppose it to be due to selection by the ants. But how the selection was made we do not know.

The structural growths which, in many flowers, necessitate the visits of special insects to fertilise them, are also probably due to natural selection in its narrow sense, for it is very doubtful whether they are useful to the plants. In the first place the plants which have the most elaborate apparatus for securing fertilisation by certain insects only are uniformly rare; while self-fertilising and anemophilous plants are abundant.[2] Secondly, very few annual plants, which must set seeds every year, have complicated flowers, and some of these—such as the annual peas and beans—are also self-fertilising. Thirdly, many perennial plants with elaborate flowers have resorted to other means to secure fertilisation in case insects fail to visit them. Fourthly, the great number and abundance of plants, whose inconspicuous gamopetalous flowers show that they have reverted from insect fertilisation, is a sufficient proof that they have not suffered any harm by doing so. We must therefore conclude that the elaborate flowers found in many of the so-called entomophilous plants are quite unnecessary for their well-being, and, indeed, must be sometimes harmful, for they render fertilisation

[1] "Animal Life and Intelligence," 2nd ed., p. 791.

[2] Henslow, "On the Self-Fertilisation of Plants," *Trans. Linn. Soc.*, 2nd series (Botany), vol. i., p. 317.

uncertain and irregular. There is still another reason for coming to the same conclusion. If it be good for a plant to have its flowers fertilised by pollen from other plants, then the grouping of flowers into a head or spike must be injurious, because it almost insures that the flowers shall be fertilised by pollen from other flowers of the same inflorescence, which Darwin says does little or no good; and yet plants with capitate flowers are numerous and prosperous.

That cross-fertilisation is useful is proved by the fact that all the higher animals are bisexual; but that it is not essential is also proved by the existence of large numbers of asexual organisms, and by the fact that the highest plants are hermaphrodite. The first phanerogams were, no doubt, diclinous and anemophilous. As the number of species increased the individuals of each species would diminish, and, consequently, fertilisation by the wind would become more uncertain; and, probably, it is for this reason that the higher angiosperms became hermaphrodite. But this hermaphroditism was tempered by dichogamy, the origin of which is not connected with insect fertilisation. The visits of insects came later. They commenced to cultivate, as it were, the flowers for their own use, each species trying to preserve the honey for its exclusive benefit; for it is evident that all these floral arrangements—including colour, capitate flowers, scent, etc.—are very useful to the anthophilous insects, for whom the honey is preserved. But these flowers, elaborated by insects for their own benefit, have secured complete isolation for the plants to which they belong, and the variations have therefore been preserved, whether they were useful or indifferent, or even when they were injurious, as in the reduction of stigmatic surface in the orchids, the abortion of one half of each anther in *Salvia*, and the asexual condition of the ray-florets in some of the Compositae. All the changes, however, are useful to those insects which alone can fertilise the flowers, and Dr Hermann Müller thinks that different kinds of insects have evolved different kinds of flowers suited to their tastes. In fact, these flowers have been cultivated by moths and bees, just as ants have domesticated some beetles and aphides. The plants that have escaped from their cultivators have run wild again, like rabbits in Australia and New Zealand.

Isolation by Elimination—or natural selection in Darwin's sense—must always have a utilitarian cause, because the elimination is for the benefit of the remainder—that is, for the selected. It may be a struggle for food, or it may be a struggle for protection against enemies, or it may be a struggle to secure the persistence of the species; but in all cases it must be a struggle with death as the penalty for being vanquished, because, without elimination by death, there can be no selection and no isolation. It is only the struggle for food which is brought about by the rapid increase of the members of a species; the struggle for protection and the struggle for perpetuating the species do not at all depend upon the doctrine of Malthus. On the contrary, the more individuals there are of a species, the less the necessity for securing special means of protection, and the less is the risk of the species dying out. But in all cases the power of natural selection increases as the structures which influence the struggle get more perfect and as competition gets keener. It can hardly come into play in the early stages of a variation, or where competition is checked by geographical isolation; but it has increased in importance with the age of the earth, and is now the dominant factor in the evolution of species among the higher animals and plants.

Geographical Isolation. The rapid increase of the individuals of a species not only leads to competition for food, and thus to isolation by elimination, but it also leads to emigration and change of habits, and thus to geographical isolation. This subject has been fully discussed, especially by Moritz Wagner,[1] the Reverend J. T. Gulick,[2] and Professor Romanes,[3] and I will merely give a new illustration of the principle. There are twelve different kinds of albatrosses belonging to three genera which roam over the Southern Ocean, most of them mixing freely together—nine or ten occurring in the Tasman Sea—but each having its own separate breeding-places, to which it retires every year. Now, as these birds have no enemies, and as their specific characteristics are not connected with the struggle for food, we cannot suppose that each species was formed by competition on the ocean, and that each subsequently chose a separate breeding-ground, or—in other words—that the development of their specific characters preceded their isolation. Evidently isolation preceded, and caused the preservation of, the variations, which in time became of specific importance. The three species of the North Pacific must also have originated in the same way. It should be noticed that these species are nearly, if not quite, as well characterised as those species which have been developed by natural selection; the intermediate varieties having died out, although there can have been no elimination by competition. And as all live under the same conditions, the variations can hardly be due to the action of the environment. Geographical isolation must often have been the means of preserving, not only indifferent characters, but also the incipient stages of useful ones, which have been subsequently developed by elimination.

[1] See Gulick in *Journ. Linn. Soc.* (Zool.), vol. xx., p. 193.
[2] *Journ. Linn. Soc.* (Zool.), vol. xi., p. 496, and vol. xx., p. 222.
[3] *Journ. Linn. Soc.* (Zool.), vol. xix., p. 348.

Physiological Isolation. This was first brought forward by Professor G. J. Romanes.[1] By it is meant those cases where the individuals of a species mix together during the breeding season, but, for some reason or other, certain individuals are restrained from having sexual intercourse with others. The simplest case is that of a sexual reproduction which insures that each individual is isolated from all others, and, consequently, any variations that may arise are preserved, unless counteracted by reversion. Probably this is the cause of the immense variety found among the Bacteria, Diatoms, Fungi, Radiolarians, and Foraminifera ; and perhaps it is the reason why Bacteria are so readily modified when placed under new conditions by cultivation. Self-fertilisation is nearly as efficient; but a cross may occasionally occur. Ferns and many other plants, as well as many Coelenterates, are thus isolated and able to preserve indifferent variations.

Partial sterility with the parent form (the physiological selection of Professor Romanes); the selective association of Dr A. R. Wallace; and change in the season of flowering or of pairing, all appear to be true causes of physiological isolation. I have lately given an example of the process of species manufacture by the last process in the case of some petrels on the Kermadec Islands.[2] Two varieties of *Aestrelata neglecta*—the mutton-bird and winter mutton-bird of the settlers—breed on the same island, but at different times of the year. The first has the neck and breast, and sometimes the whole under surface, gray; while the winter mutton-bird has only a gray band on the breast, the rest of the under surface being white. Here physiological isolation is bringing about much the same result as geographical isolation has done in the case of the albatrosses, for —as with them—we must suppose that the change in the time of pairing preceded the change of plumage.

Sexual selection is better considered as a form of physiological isolation than of natural selection, for there is no elimination of the males; they are not killed off, but can, after defeat, try again to obtain a partner. Some males secure the females either by greater strength or by superior weapons of offence, or by superior means of capturing them, while others are selected or rejected by the females; and in the case of birds, the latter mode of selection seems to explain the preservation of many beautiful variations in plumage. Dr Wallace supposes that these beautiful variations in plumage have been produced by the greater vigour of certain males, which is probably true; but no amount of vigour in the male would, by itself, secure the preservation of these variations without isolation, and this has been due to sexual selection. It is possible that the females select the males for their vigour and not for their beauty,

[1] *Journ. Linn. Soc.* (Zool.), vol. xix., p. 350.

[2] *Proc. Zool. Soc.*, 1893, p. 753.

although there is much evidence to the contrary; but, in either case, isolation by sexual selection is necessary for the preservation of any variations the males may possess.

From this examination of the place of isolation in organic evolution I conclude that species generally originate by the preservation of individual variations by means of geographical or physiological isolation, which may be brought about in many ways; and that, in most, but by no means all, cases they are improved and developed by competition and natural selection; and this competition they find only in large and well-populated areas.

F. W. Hutton.

III

The Relation of Acquired Modifications to Heredity

TO most evolutionists it must be evident that a distinct change is coming over the controversy on use-inheritance. Not only are the views expressed less positive, but there also seems some likelihood of a compromise. A letter to *Nature*, last April, by Professor Baldwin, was, I think, expressive of the feelings of evolutionists generally.

Bateson has remarked[1] that "The study of variation thus offers a means whereby we may hope to see the process of evolution." This position does not seem to have received the attention which, I think, it deserves; and it is with the hope of helping to turn attention to these points that I offer two suggestions on this subject.

Ever since Galton put forward his "Theory of Heredity," the problem of use-inheritance has been coming more and more to the front, and became almost the main point at issue after the publication of Weismann's Essays; so that we have now the curious anomaly of evolutionists of the highest eminence occupying all grades between the extremes of Professor Henslow, who denies the action of natural selection altogether in the formation of species, and Professor Weismann, who nearly as emphatically denies the action of use-inheritance.

This is the more extraordinary when it is remembered that the intermediate position, occupied by Romanes and Lloyd Morgan, has practically disappeared, owing to the death of Romanes and the secession of Lloyd Morgan to the Neo-Darwinian position.

The difficult if not impossible task of finding any really satisfactory test case that is capable of only one explanation is no doubt largely responsible for the divergence of opinion. But I believe there is also another cause for this divergence, namely, that the vast field which the subject covers compels all but the most powerful minds to limit themselves to a portion only of the subject. It will be seen on reflection that most of the Lamarckians have mainly studied either the lower forms of life generally, have been more or less exclusive botanists or palaeontologists, or have devoted their attention to less important structures or easily variable species; while on the other hand the Neo-Darwinians have studied large living groups of animals or the more

[1] "Materials for the Study of Variation."

highly-developed and more differentiated organisms in animal or vegetable kingdoms. While such men as Darwin and, to a less extent, Romanes have occupied a more general position, the numerous almost unconscious impressions that come to an investigator in any branch of science, the little details of practical experience which are rarely if at all jotted down, even by the most painstaking recorder, give, I believe, a general and correct, though usually unconscious, colouring to all his work, original or otherwise, and are largely influential in determining his convictions.

It is to this colouring from different surroundings that I attribute the positions taken up by the various evolutionists, and I think, had it been possible for the Neo-Lamarckians and Neo-Darwinians to have exchanged positions at the commencement of their scientific studies, that both sets of investigators would have materially altered their opinions.

The conclusions drawn, both on theoretical and practical grounds, from the study of the principal works of both sides, seem to me to support the following propositions:—

(1) That the simpler the organism, the greater the power of use-inheritance.

(2) That the higher stages of evolution entail increased differentiation, and therefore increased difficulty of direct adaptation to environment, and therefore increased dependence on natural selection.

(3) That high specialisation must be accompanied by a correspondingly increased stability, and, therefore, increased difficulties in the action of use-inheritance, on account of the increased dependence of those specialised parts on each other.

(4) Lastly that, with the increase of natural selection, the variations must become increasingly adaptive.

The second point has special reference to the theories of heredity. To anyone who considers for a moment the immense importance assigned to automatic, unconscious, and reflex actions in Psychology, Physiology, and Pathology, and the large amount which has been written on habit and its effect on the organism, it must seem remarkable that so little importance has been given to it in evolution and heredity. Erasmus Darwin considered that as the embryo was made up of two portions which had formerly belonged to its parents, it was reasonable to suppose that it would to a large extent retain the habits of those parents.

More lately, Professor Ewald Hering has extended this idea to what he aptly defines as unconscious memory. His explanation of heredity lies between the physiological and morphological schools.

He supposes first that under appropriate circumstances a small amount of the original substance may be capable of governing the course of the future organism, just as the mathematician may construct from a small portion of a curve its whole extent. And, secondly,—

"If in a parental organism, by long habit or constant practice, something grows to be second nature, so as to permeate, be it ever so feebly, its germinal cells, and if the germinal cells commence an independent life, they will aggrandise and grow till they form a new being, but their single parts still remain the substance of the parental being."

The objections to this theory lie, I think, in the fact that direct communication with the reproductive organs becomes with increasing specialisation increasingly difficult, and therefore heredity and reproduction would cease when a certain point in the specialisation was reached.

Nevertheless, some kind of provisional theory such as the following would, I believe, explain better than any other theory most of the phenomena of inheritance:—

(1) That in every cell there are certain reproductive units which are necessary to the development of that particular cell.

(2) That these reproductive units having a very complicated structure (being composed of specialised protoplasm), are capable of modification when acted on by external forces.

(3) That the various impressions made upon the cell would of necessity be made upon these units also, and that this impression will be proportional to the length of time and intensity of the impression made.

(4) That as specialisation of tissue occurs, each reproductive unit will tend to reproduce its own history, past impressions becoming with each successive addition more and more blurred.

(5) That the stronger and more numerous the past impressions, the more difficult will it become for present impressions to affect them, hence progressively diminished power of use-inheritance.

(6) That the reproductive units have the power of self multiplication when in the latent condition, and that this multiplication will be difficult in proportion to their specialisation and complexity. Hence latent germs would tend to be carried on from one generation to another, and increase the general stability of the organism.

(7) That when not required in cell development they will tend to pass into the system of the organism, and that when suitable conditions arise they will tend to reproduce the cells from which they are derived.

In the earlier forms of life these units will diffuse themselves throughout the organism (Protozoa), but as differentiation occurs these units will tend to become localised at one or more places (Hydrozoa). Of these places one will become more important either from habit or position, and this will become fixed and subsequently specialised (ovary or testis). The cell differentiation will at last become so great that it will stop all reproduction of parts except at the specialised centre. Partial renewal of limbs, etc., in the earlier vertebrates becoming rarer and ceasing altogether as we ascend to the higher vertebrates.

(8) That these reproductive units having once started a phenomenon in any given direction, the direction will tend to be kept up and continued by physiological laws.

(9) That each unit would tend from habit to occupy in a new organism a position similar to that which it occupied in the parent.

This theory would explain the constancy of type, as there would be a continually increasing balance in favour of heredity. It would satisfactorily explain the recapitulation theory of embryology. It would account for the recognised antagonism existing in both plants and animals between the reproductive and bodily growth, and it would afford an explanation of growth in abnormal situations.

In conclusion, I think it will be found that we are brought back to a closer study of the causes of variations as the only satisfactory means of solving the fundamental problem of use-inheritance.

J. Lionel Tayler.

IV

A Carcinological Campaign

DURING the last few months there has been remarkable activity in discussing and describing new and peculiar forms of the smaller crustacea.

At Liverpool last autumn Mr A. O. Walker (15) announced his new Cumacean genus *Leuconopsis,* in which the male has on the second joint of the third foot a pair of curved blade-like processes, the feature unique, the function not yet explained.

In the *Transactions* of the Royal Irish Academy, Mr W. T. Calman (3) has enriched the caridea or true shrimps with a new family, Bresiliidae, established for a specimen taken at a depth of 750 fathoms off the south-west coast of Ireland. In the *Transactions* of the Royal Society of Edinburgh, Mr Calman (2) has re-described and re-figured the *Anaspides Tasmaniae* of G. M. Thomson, with a view of discussing the systematic position of this extremely interesting crustacean. It is found in Tasmania in pools at an elevation of 4000 feet. It is in structure at present quite unique. This combination of uncommon form with uncommon habitat led its learned discoverer to say that "owing to long isolation it has undergone very profound modification." But it may equally well be supposed that its isolation has enabled it to retain characters which in other crustaceans have been profoundly modified. Reasons are given by Mr Thomson for the opinion that the ancestral forms of *Anaspides* found their way from the sea into the streams and lakes of Tasmania as far back as Mesozoic times. Its thoracic limbs being divided into walking and swimming branches, it has reasonably been grouped with the Schizopoda or "cleft-foot" shrimps, and in some respects it seems to come nearest the Euphausid family, so distinguished for luminous organs. To such organs I fancied that the minute group of 'ocelli' on the back of the head, which Mr Calman has pointed out, might perhaps belong, but the guess has found no favour, although visual ocelli can scarcely be needed to supplement the stalked eyes. In the segments of the trunk the animal is rather like an amphipod, which it also resembles in having simple branchial vesicles. But these are in pairs. Mr Calman speaks of this latter circumstance as without parallel in adult malacostraca, overlooking, it would seem, the 'accessory branchiae' in certain amphipoda to which the late Professor Wrześniowski first called attention. In appearance *Anaspides* not only has seven thoracic segments distinct as in the Amphipoda, but also a segment immediately in front of these distinct. Here, however, Mr Calman maintains that the appearance is delusive, and that we have only to do with the well-known cervical groove of the carapace. He may be right. He may be wrong. The suggestion is certainly very ingenious. It would be inconvenient here to follow him into the details of so technical a question, or through the important comparison which he institutes between *Anaspides* and the palaeozoic crustacea, *Palaeocaris, Gampsonyx*, and *Acanthotelson*. To all seeming, however, *Acanthotelson* is much nearer to the isopod genus *Apseudes* than to a schizopod, and the figures of Packard's restoration would have been better omitted, since they do not agree either with the original figures of the fossils or with the description given in the text. Meek's figures (*Geological Survey of Illinois*, vol. III., p. 549, etc., 1868) probably give all the information that can be depended upon.

Professor G. O. Sars (12) is bringing out in rapid succession the parts of his Isopoda of Norway, always with the fulness of satisfying illustration and exact description for which his work is celebrated, throwing a flood of light upon groups, such, for example, as the minute species of *Munna*, which before were puzzling and obscure. In his account of the Anthuridae he does not notice, and has perhaps forgotten, the view taken by Dohrn and Gerstaecker, and later brought into prominence by Dr Charles Chilton (3), that in this family the longer branch of the tail-feet or uropods is not the inner branch, as authors have generally supposed, but in accordance rather with homology than appearance, the outer branch. Dr Chilton also doubts whether this longer branch is ever really two-jointed, though it is open to maintain that it is sometimes actually and always virtually so. These are points on which the Norwegian professor's expressly declared opinion would be of much value. For the correct name of the very common Isòpod, generally known as *Idotea tricuspidata* Desmarest, Professor Sars selects '*Idothea baltica* (Pallas).' As the synonymy of this species was exhaustively investigated by Harger in 1878, by Miers in 1881, and by Dollfus in 1895, it is amusing to note that, in the name finally adopted by each, they all differ from Sars and each one from the other. Harger was unable to consult Pallas' work. He therefore acknowledges that Meinert (1877) may have rightly regarded *Oniscus balthicus* Pallas as the earliest name of the species. The generic name *Idotea* came into the world with one letter missing, and this same much victimised letter is found as a superfluity in the specific name *balthicus*, so that *Idotea balthica* (Pallas) will be the form upheld by those of us who think the spelling used by our scientific forefathers worth preserving.

For number of remarkable novelties the palm is carried off by M. Jules Bonnier. He describes (1) six new genera and forty-five new species of sessile-eyed crustaceans, obtained by Prof. Koehler on board the "Caudan" in the Bay of Biscay. The depths ranged from 200 to 1700 metres. Out of 52 species taken 39 proved to be totally blind. The new Cumacean genus *Procampylaspis,* like Mr Walker's *Leuconopsis,* displays an unexpected character, the 'finger' or terminal joint of the second maxillipeds being cut into strong unequal teeth, giving the appendage what might almost be called an unnatural appearance. The rapid movement of modern science is exemplified in the circumstance that M. Bonnier's new anthurid, *Calathura affinis,* is scarcely published before it has to be transferred, as it evidently must be, to Sars' new genus *Leptanthura.* To the family Arcturidae M. Bonnier contributes a new species, *Astacilla Giardi,* which is remarkable not only for a quite abnormal appendage on the breast of the male, but also because the male is slenderly drawn out to a length thrice that of the female. The exiguity of the creature recalls the vermiform male of an anthurid discovered by Professor Haswell wriggling into serpula-tubes in Australia. Another of M. Bonnier's striking results is the discovery of a crustacean parasite upon a Cumacean species. But this novelty also has been already transferred to a new genus by Dr H. J. Hansen, who, in a work noticed elsewhere, has described no less than seven new species of such parasites.

Miss Mary J. Rathbun (8, 9) concerns herself only with the Brachyura, but, as in more than one of her recently described new species, the full-grown crab is less than the fifth of an inch in length, these species at least may be classed among the smaller crustaceans. On the other hand, M. Adrien Dollfus (6) speaks of a new woodlouse, *Porcellio eximius,* from the north of Africa, as "cette magnifique espèce." It has the outer branch of the uropods in the male half as long as the body, as though it were a kind of peacock among woodlice, proud of its tail. Possibly these prolonged appendages enable their owner to execute strategic movements to the rear with caution and tact.

Miss Harriet Richardson (10, 11) has this year described two new species of *Sphaeroma,* and given figures of one of them. The first is notable for its habitat, having been taken not from the sea but from a warm spring in New Mexico. The second is notable for its objectionable habits, having been found boring the piers on St John's river at Palatka, Florida. The mischievous little creature has powerful jaws, and in eight years reduced timber of 16 inches diameter to less than half that measurement.

In his Pelagic Entomostraca of the Caspian Sea, Sars (13) discusses eighteen species, of which thirteen are new. Six belong to the new genus *Cercopăgis*, meaning "sling-tail." Were these animals twelve feet long instead of a twelfth of an inch they would rank among the most striking objects in zoology. The eye is enormous. The thread-like caudal process is sometimes half an inch long, fully six times the length of the body. Near the end this lash is "bent in a peculiar sling-like manner, the opposite edges of the sling armed with a double row of recurved denticles." Furthermore, out of a kind of gastric sympathy, the intestinal tube forms also a sling-like flexure or loop. In the female the incubatory pouch rises abruptly from the back and inclines forward, this monstrous sack of young ones being sometimes as large as the body which supports it.

In the *Proceedings* of the Biological Society of Washington (vol. xi., pp. 153-167, June 9, 1897), Miss Rathbun gives 'A Revision of the Nomenclature of the Brachyura.' It appears to be thoroughly sound in principle, and is certainly based on wide and accurate knowledge. Only, in a few points of detail, one may be permitted to question the results arrived at, and to defend, for instance, the name *Carcinus* for the shore-crab, *Thelphusa* for the river-crab, *Macrocheira* for the giant-crab of Japan, since the reasons for displacing these familiar names seem to be at least not imperative. Owners of Herbst's *Naturgeschichte der Krabben und Krebse* and of Leach's *Malacostraca Podophthalma Britanniae*, will find in Miss Rathbun's paper exceedingly useful tables, establishing the dates of the numerous parts of those works, the publication of which extended in the one case over two-and-twenty years, and in the other over no less than sixty.

The excellent plan of printing the very day of publication on cover and title-page is followed in Miss Rathbun's paper. Therefore, for her new generic name *Ucides*, in place of Latreille's pre-occupied *Uca*, we know precisely that the date is June 9, 1897. But of Dr Ortmann's *Oedipleura*, also a new name for *Uca* of Latreille, we can say nothing positively. Some supplementary notes of correction at the end of his valuable Carcinologische Studien are dated "Princeton University, New Jersey, d. 29 Mai 1897." This date was probably written on the proof copy. The paper was printed and published in Jena. It is for the publishers to tell us the exact date of publication. Until they do an expectant world cannot know for certain which has the priority, *Ucides* or *Oedipleura*.

It should be understood that the above remarks touch only a small part of the papers mentioned, and also that they leave unnoticed contributions by many other well-known writers, highly worthy of attention, though the forms discussed may not happen to be quite so eccentric as those to which allusion has here been made.

REFERENCES.

1. **Bonnier, J.**—"Résultats scientifiques de la Campagne du 'Caudan' dans le Golfe de Gascogne—Août-Septembre 1895—Edriophthalmes." *Ann. Univ. Lyon*, pp. 527-689, pls. 28-40. 1896 (? 1897).
2. **Calman, W. T.**—"On Deep-Sea Crustacea from the South-West of Ireland." *Trans. Roy. Irish Acad.*, vol. xxxi., pt. i. (Reprint), pp. 1-22, pls. i., ii. 1896.
————————.—"On the genus Anaspides and its Affinities with certain Fossil Crustacea." *Trans. Roy. Soc., Edinburgh*, vol. xxxviii., pt. iv. (No. 23), pp. 787-802, pls. i.-ii. 1896.
3. **Chilton, C.**—"The subterranean Crustacea of New Zealand." *Trans. Linn. Soc., London*, vol. vi., pt. 2, p. 217. 1894.
4. **Dohrn, Anton.**—"Untersuchungen über Bau und Entwickelung der Arthropoden." Heft v., p. 96. Leipzig, 1870.
5. **Dollfus, A.**—"Les Idoteidae des côtes de France." *Feuille des Jeunes Naturalistes.* No. 292, p. 10. 1895.
6. ————.—"Les Isopodes terrestres du Nord de l'Afrique. Du Cap Blanc à Tripoli." *Mém. Soc. Zool., France*, vol. ix., p. 536. 1896.
7. **Meek and Worthen.**—*Geological Survey of Illinois*, vol. iii., p. 549. 1868.
8. **Rathbun, M. J.**—*Proc. U.S. Nat. Mus.*, vol. xix., No. 1104, p. 141. 1896.
9. ——————.—*Proc. Biol. Soc., Washington*, vol. xi., p. 99.
10. **Richardson, H.**—*Proc. U.S. Nat. Mus.*, vol. xx., No. 1128. 1897.
11. ——————.—*Proc. Biol. Soc., Washington*, vol. xi., pp. 105-107.
12. **Sars, G. O.**—"An Account of the Crustacea of Norway." Vol. ii. Isopoda, pts. iii., iv., v., vi. Bergen, 1897.
13. —————.—"Pelagic Entomostraca of the Caspian Sea." Pp. 1-74, 8 pls. *Ann. Mus. Zool. Acad. Petersbourg*, 1897.
14. **Thomson, G. M.**—"On a Fresh Water Schizopod from Tasmania." *Trans. Linn. Soc., London*, vol. vi. pt. 3, pp. 285-303, pls. 24-26. 1894.
15. **Walker, A. O.**—*Rep. Brit. Assoc.* 1896. P. 419. London, 1896 (? 1897).

T. R. R. STEBBING.

EPHRAIM LODGE, THE COMMON, TUNBRIDGE WELLS.

VI

South America as the Source of the Tertiary Mammalia [1]

Of the Argentine Territory during the Archaean era there only existed the frame of the massive mountains of the north-west and a few points and islets, which to-day form part of the various isolated mountain chains which rise from the plain of the Pampa, from Salta to Patagonia.

The oldest fossiliferous deposits of the first Palaeozoic epochs rest on these Archaean rocks: all the organisms are marine. In the latest times of the Palaeozoic era, during the Carboniferous and Permian periods, these small islands served as a nucleus for a greater extension of the land, and then great numbers of terrestrial organisms appeared, of a uniform aspect, precisely as the temperature in all parts of the globe was uniform.

The deposits of the greater part of the Mesozoic era, with rare exceptions, are found in the Cordillera, where they appear on either side in the form of narrow bands running north and south, proving that then as now the Cordillera of the Andes already existed as a long and narrow land which separated the Atlantic from the Pacific. Both oceans reached the foot of the Cordillera, but in the Atlantic the mountain chains of Tandíl, Ventana, Córdoba, San Luis, and various others formed large islands. At this time the geographical differences of temperature began to be felt, causing climatic zones, the most active of the factors which operate in the differentiation of organisms—a differentiation which allows us to determine the relations of the floras and faunas of different regions, and to restore the routes which they followed in their migrations across the lands of other times, which are not the same as those of to-day, furnishing us with the data to reconstruct the ancient connections of the lost continents.

We have now reached the latest time of the Cretaceous period, the most recent of those which constitute the Mesozoic era. Water predominated in the northern hemisphere, and land in the southern—the reverse of what happens at the present day. The European

[1] Translated by Mrs Smith Woodward from "La Argentina al través de las última épocas geológicas," an address delivered at the inauguration of the University of La Plata, April 18, 1897. (8vo., pp. 35. Buenos Aires: P. E. Coni & Sons, 1897.)

continent had not appeared, except as a few small islands. North America, completely separated from South America, formed a great island, with large lakes of brackish water; and this part of South America had lost its insular and peninsular form. The Argentine Territory had completely emerged, and extended to the east towards South Africa, while to the south and west it was prolonged to form a large continent, which placed it in connection with Australia and New Zealand.

It was during this epoch in that great southern continent, and especially in its central portion now constituting the Argentine Territory, that the highest organisms developed, the great class of the mammals which immediately spread over the southern lands, and subsequently penetrated by different routes into the northern hemisphere.

The great barrier of the Andes was then low, and did not hinder the atmospheric currents. The climate was hot and humid, and a luxuriant vegetation covered all the Argentine Territory. As far as the present Patagonian plains, to-day dry and sterile, there flourished large forests of palms and conifers, whose petrified remains fill whole deposits, in which one continually finds huge tree trunks transformed into flint still occupying their natural position and constituting dead forests, forests of stone, columns of flint such as that which one can see opposite the Museum of La Plata crowned with the bust of the unfortunate Crevaux, and which the imagination of the dwellers of the Patagonian deserts, on account of the undulation of the land, takes to be the masts of petrified ships.

Alternating with the branches and tree trunks transformed into stone, which fill the deposits of sandy rock appearing at various points of the Patagonian Territory, large bones are met with similarly petrified, belonging to terrestrial vertebrates of the extinct group Dinosauria. They were reptiles with an enormously thick tail, and the hind limbs much longer and thicker than the fore limbs, so that, supporting the body on the hind limbs and tail, they could assume a semi-vertical or oblique position resembling that of a kangaroo.[1] When one says that as a matter of fact they could have looked over the roofs of most of the buildings at La Plata, one can judge of the truly colossal size which some representatives of this group attained.

The birds of that time were no less noteworthy than the reptiles. They were such as *Physornis* and *Phororhacos*, true monsters, bipeds with short and thick wings, the claws of an eagle, and the beak of

[1] Of the three sub-orders into which the Dinosauria are divided, namely, Sauropoda, Theropoda, and Orthopoda, the characters mentioned above are peculiar to the two last. The sub-order of the Sauropoda, to which the gigantic genera of Patagonia, *Argyrosaurus* and *Titanosaurus* Lyd., belong, have the four limbs more or less equal, or the front pair scarcely any shorter than the hind pair.

a condor, of whose size we may form an idea from the head, which is much larger than that of a horse.[1] Being great runners, they gave chase to the mammals of that epoch, even to the most gigantic of them, and were doubtless not afraid to measure their strength with the Dinosaurs themselves.

But the animals of that period which in our formations offer special interest are the mammals. While in Europe and North America only some small representatives of that class lived, insignificant and little specialised, in Argentina they had attained an extraordinary development; they were large and small, of the most varied forms, showing that the Cretaceous deposits of our country contain the ancestors of almost all the groups of mammals which have succeeded each other one by one in different regions of the earth.

It would be a lengthy task to give you an account of the mammalian fauna of that time; it is only possible for me to outline the subject and to limit myself to noticing some forms related to others with which you are familiar.

That which first attracts the attention of the naturalist in this fauna is the presence of remains of the Primates or inferior quadrumana (*Notopithecidae*) of a greatly reduced size, which appear to be the ancestors of the extinct lemurs of Europe and North America, and of those existing in the South of Asia and Africa, while another branch leads to the *Homunculidae* (*Homunculus*, *Anthropops*, *Pitheculus*, etc.) of the Tertiary of our own country, which are the ancestors of the monkeys of both worlds, and consequently of man.

The carnivorous mammals were represented solely by a group to which I have given the name Sparassodonta, whose size varied from that of a 'laucha' (*Pharsophorus*) to that of the largest bear (*Proborhyaena*); they exhibit a mixture of the characters of placentals and marsupials, and represent the stock whence were derived the carnivorous marsupials of the Australian continent, the placental carnivores of both hemispheres, and a large number of the extinct forms of the northern hemisphere designated under the name of Creodonts.

Another most interesting group is that of the Plagiaulacoidea (*Polydolopidae*, *Abderitidae*, *Epanorthidae*, etc.), small marsupial mammals with a dentition of the type of the Australian kangaroos, but with the limbs more nearly equal, with five digits on each foot, and with traces of syndactylism. They were extremely numerous, and gave origin to the greater portion of the marsupials of Australia, designated under the name of Diprotodonts, a group of which the

[1] These fossils may now be seen in the Department of Geology in the British Museum (Natural History).—TRANS.

kangaroos form part. A few years ago no one would have suspected that these latter could have taken their origin in any continent other than that of Australia, and still less in Argentina, separated to-day from the Australian lands by the immense abyss of the Pacific.

These primitive Plagiaulacoidea or Diprotodonts were accompanied by the Pyrotheria (*Pyrotherium*), mammals of very variable size, with pentadactyl feet, the limbs in the form of perpendicular columns of support, a short neck, large head, square grinding teeth with two transverse ridges like those of *Dinotherium*, large upper and lower tusks as in the oldest Mastodonts, and a large trunk like that of the elephant. They are the stock whence have sprung the proboscidians which appear completely developed on the Euro-asiatic continent in the Tertiary period, their origin until now having been an indecipherable enigma.

Together with the Pyrotheria, there lived the Archaeohyracoidea (*Archaeohyrax*, *Argyrohyrax*, etc.), small plantigrade mammals half-hoofed and half-clawed, whose external aspect was that of a cavy (*Cavia*), and which have given origin to the Hyracoidea (*Hyrax*) existing in Asia and Africa, whose ancestors have not been known until now in these continents. The Notohippidea (*Morphippus*, *Rhynchippus*, etc.), small pentadactyl ungulates, but with the middle digit much larger than the side ones, constituted the stock from whence the horses have sprung. The Notostylopidea (*Notostylops*, *Trigonostylops*, etc.), whose dentition has a rodent-like appearance, and give rise to the Tillodonts of the northern hemisphere. The Isotemnidea (*Isotemnus*, *Trimerostephanos*) which probably represent the source of all the ungulates. The Homalodontotheria (*Asmodeus*, etc.), the oldest ancestors of the extinct Ancylopoda of Europe, Asia, and North America, curious and anomalous herbivores which possessed all the characters of perfect ungulates, except in the digits, which were bent in the form of hooks and armed with compressed claws like the unguiculates.

I have only mentioned a small portion of the ungulates of this period, which were very numerous. They were gigantic and with large tusks, like the Parastrapotheria, of medium size and generalised characters, like the Nesodonts and the Leontinidea; small, sturdy, and annectent forms between the ungulates and unguiculates, like the Hegetotheridea (*Prohegetotherium*), the Trachytheridea, and the Protypotheridea (*Archaeophylus*); tall and slender, like the deer, and with a single hoof on each foot imitating the horses in miniature, like the Proterotheridea (*Deuterotherium*), or with ambiguous affinities between the even and odd toed animals like *Didolodus*.

Of these different groups some few have completely disappeared, and the rest have dispersed over the Argentine Territory, passed on to other regions, where by means of successive transformations they have given rise to the different orders of mammals which live, or have lived, in all parts of the earth. But besides these primitive mammals, which have left no successors here to reach to our epoch, one also meets with the ancestors of those which to-day are characteristic of our country, such as the hystricomorphous rodents and the opossums (*Didelphys*), which were represented by types more or less resembling the living forms, but exceedingly reduced in size. Together with the Peltateloidea (*Peltephilus*), singular armadillos with variable, pointed, bony horn-cores above the snout, there were already armadillos almost similar to those now living, by the side of others very different called *Palaeopeltis,* which gave rise to the Glyptodons of more modern periods, and sloths, generally small, but similar to those which later were destined to reach the gigantic size of the Mylodonts and Megatheria.

In a sentence, at the end of the Secondary period there lived in the Argentine Territory not only the ancestors of the mammals which inhabit it now, but also of those which live in all parts and all climates of the world.

The Secondary era closed and the Tertiary opened with a disturbance and a general change in the orography of the continents, and in the distribution of land and water. Great volcanic eruptions accompanied the elevation of the large mountain ridges previously only indicated, and the oceanic waters were shifted from north to south. The northern hemisphere was transformed into a continental one, and the southern hemisphere into an insular and peninsular one. The antarctic continent has remained split up, and the faunas of its different parts have thenceforward evolved separately. South America became reduced to an island of varying outline, and the ocean in this tremendous encroachment covered the territory of the Republic, rolling over the isolated sierras of the Pampa, reached as far to the west as the base of the first spurs of the Andes and the great mountain mass of the North-West. This land served as the refuge for the terrestrial mammals which were saved from the catastrophe. It was in the bottom of this ocean that the beds of the marine formation called Patagonian were deposited, which can be traced along the greater part of the Atlantic coast to the south of the Rio Negro, with a thickness at times of 300 metres, and corresponding to the middle or lower part of the Eocene period.[1]

During the Upper Eocene period another great upheaval of the land or a retreat of the ocean took place, the territory of the Republic rising again with its eastern shores more to the east than at

[1] The Patagonian formation has no species in common with the territory of Chile (excepting the system of Lebú), as I have said, but there are some in the formation immediately above, which is known as the Santacruzian.

the present time. Freshwater and atmospheric agencies accumulated on this newly-raised land the great Santacruzian formation, which, with a thickness of more than 200 metres, appears exposed in different parts of Patagonia, and especially in the region of the Rio Santa Cruz.[1]

The mammals which had taken refuge in the heights turned to descend to the plain, but already many had become extinct. The Hyracoidea, the Condylarthra, the Pyrotheria, and the Tillodontia had disappeared. Of the Notohippidea, previously so numerous, there scarcely remained any trace. The Ancylopoda had diminished remarkably in size and number. The Notopithecidea of the Cretaceous (*Notopithecus*, *Eupithecops*, etc.) had been transformed into the Homunculidea, which are the direct ancestors of the monkeys of both continents. The Typotheria and Astrapotheria had also begun to decline. On the other hand, the rodents, the Plagiaulacoidea, the Sparassodonta, the Nesodonta, and the Litopterna (*Theosodon*, *Proterotherium*, etc.) had increased in an extraordinary manner, the same as the armoured and unarmoured edentates. The groups of the Glyptodons and the Megatheria were already perfectly developed, but with representatives of a comparatively small size.

The data concerning the period in question are still much confused, but we know that at the beginning of the Oligocene epoch the Argentine Territory suffered a fresh submergence, accompanied by new volcanic and tectonic disturbances. The sea flowed back to cover the greater part of the plain, while the lava streams thrown out by the submarine volcanoes formed the sheets of basalt which cover like a black shroud the older formations of the Patagonian slates. Later, during the beginning of the Miocene, impetuous torrents brought down from the rugged, rocky heights granite and porphyritic blocks, rocks of all kinds, which, beaten by the waves of the sea, formed that great deposit of boulders which covers the surface of Patagonia without break from the Rio Negro to the Straits of Magellan.[2] The inhabitants of the plains migrated again to the heights, many of them perishing, others adapting themselves to the new conditions.

At the end of the Oligocene period the ocean made a retrograde movement, and took up the position it occupies more or less to-day, and the mammals returned to live on the plains, but again fewer than they had been. The Nesodonts, the greater part of the

[1] The Santacruzian formation exhibits a considerable number of species of fossil mollusca which are also met with in the Tertiary system of Navidad in Chile, which proves that both formations belong more or less to the same geological period.

[2] It is this formation which has been designated under the name "Tehuelche Formation." There have been recently found in it beds of fossil shells, which show that it is a marine formation, probably of the same epoch as the Tertiary system of Coquimbo in Chile.

Typotheria, the Ancylopoda, the Astrapotheria, the Peltateloidea, the Plagiaulacoidea, and the monkeys had disappeared. Of the Sparassodonta and Litopterna few traces remained. On the other hand, the Glyptodons and Megatheria, though in smaller numbers, were represented by forms which frequently attained a gigantic size. The hystricomorphous rodents had increased extraordinarily in numbers and size: the fossiliferous deposits of the Parana contain remains which indicate the former existence of mice of the size of oxen and horses.

Let us see what was happening meanwhile in the other continents. Since the submergence and disintegration of the Antarctic continent, Australia has remained isolated until our days; the primitive fauna of the Sparassodonts and Plagiaulacoidea, which were derived from the ancient Argentine continent, continued their evolution independently until they formed the Thylacines, the Dasyures, and the Kangaroos, living and extinct, of the same region.

South Africa, on the loss of its connection with South America, united itself with Asia, which already formed a continuous land with Europe; but the Atlantic, which extended over the Sahara as far as the Red Sea, opposed a barrier to the direct passage of the faunas of South Africa to Europe, and *vice versa*. On the other hand, with the continental transformation of the northern hemisphere, lands emerged, which put the Euro-asiatic continent in more or less direct communication with North America.

The ancient mammals of the Argentine Territory, which by reason of the submergence of the Antarctic continent had remained in South Africa, passed on at once to the Asiatic Continent, where they found conditions favourable to their development and evolution. The Pyrotheria developed into the Proboscidia, the Archaeohyracoidea into the living Hyracoidea, the Notohippidea into horses, the Condylarthra into Artiodactyles and Perissodactyles, the Sparassodonta into Creodonts and Carnivora, etc. The remaining South American mammals, such as the Monkeys (Homunculidae), the Hystricomorphous Rodents and the Opossums, invaded the Euro-asiatic continent by the same route. From Asia they passed on to Europe, and from Europe to North America, where they became specialised under different forms, each more bizarre and fantastic.

We return to South America. We find ourselves in the last third of the Cainozoic era at the end of the Miocene period. The mammalian fauna has continued to diminish in number. The Proterotheridea and the large rodents of the previous epoch have disappeared. Of the numerous order of the Toxodonts, there only remains the genus *Toxodon*, whose representatives attained the size of large rhinoceroses. The Megatheria and Glyptodons reached the summit of their development, to end in those gigantic beings whose skeletons fill the galleries of the Museums of Buenos Aires and La Plata. The two Americas had been separated until now by the ocean, and the territories of Panama and Central America had been submerged in a deep sea which put the Atlantic and Pacific in communication.

Great tectonic movements produced a general raising of the mountain chains which traverse the New World from south to north, followed by a great retreat of the waters of the ocean. The continental mass acquired a larger extension, and both Americas were put into communication by the raising of a vast land-surface, in which to-day are the Gulf of Panama and the Caribbean Sea. The Galapagos Islands on one side and the Antilles on the other remained surrounded in this newly-risen land, and America in the form of a great rectangular continental mass extended from pole to pole.

The terrestrial faunas, confined hitherto by the inter-American sea, on the disappearance of this barrier began to move in opposite directions, that of the north towards the south and that of the south towards the north, producing a zoological interchange which had, as a result, the formation of a mixed fauna, whose origin has hitherto been a little inexplicable. Passing from the upper part of this recently-upheaved land, and describing a complete circle through time and space, there returned to Argentina many of the forms which had lived there during the Cretaceous period, but all of them modified and disguised. There emigrated at this epoch from North to South America the Mastodons, which had become extinct on the plains of the Pampa when, long geological periods previously, their forefathers the Pyrotheria disappeared from our land. With the Mastodons came the dogs, the felines, and the other carnivores descended from the ancient Sparassodonts, the llamas and the deer, the horses and the tapirs, which lived and multiplied on the Argentine plains by the side of the Toxodons, the Glyptodons and the Megatheria. But passing across these same lands the Argentine fauna advanced to the north and invaded North America. The clumsy *Toxodon* of our land was exterminated in Nicaragua. The heavy Glyptodons of the Pampa wandered away as far as Anahuac, where their carapaces are found on the slopes of the valley of Mexico in the neighbourhood of the city of the same name, and still further to the north in the surface deposits of the plains of Texas. The carpincho (*Hydrochoerus*) of the River Parana wandered as far as Florida accompanied by the *Chlamydotherium*, the most robust of the true armadillos which lived in our land. The gigantic extinct sloths of the Buenos Aires plains, the Mylodons and the Megatheria, advanced to a still greater distance, their remains being met with in the States of Virginia, Georgia, Carolina, and in the whole of the valley of the Mississippi, mingled with the most characteristic representatives of the North American fauna.

We arrive at the beginning of the Anthropozoic era, and with it ceased the communication between the two Americas, the land which for a long time had united them being again submerged. We see then during the Quaternary times North America invaded by new forms; the Mastodons were replaced by gigantic elephants, accompanied by various other genera and species from the Old World. We see the *Elephas columbi*, the bisons, the *Equus tau*, and *E. conversidens* descending by the valleys of Mexico and advancing towards the south as far as the isthmus of Panama, but they found it interrupted and were not able to tread the South American soil.

A last retreat of the ocean which made itself felt over the greater part of the American coasts of the Atlantic left dry great shore banks of marine shells, like those of the neighbourhood of La Plata, which provide material for the building of this beautiful city, made by the genius and energy of our sympathetic rector [of the university]; this fresh continental encroachment upon the ocean again united both Americas, when the *Elephas columbi* and the other great mammals which had accompanied it in its migration to the south had already disappeared from the north. The bridge reappeared in the form of a narrow and tortuously long piece of land, which served from that time as a highway to the pre-Columbian peoples of our hemisphere who migrated successively, and backwards and forwards, from north to south, and from south to north, strewing the road with ruins, in which the mixture of a hundred peoples to-day misleads the cleverest investigators of the prehistoric past of the world of Columbus.

FLORENTINO AMEGHINO.

SOME NEW BOOKS

"Terra Australis Incognita"

The Naturalist in Australia. By W. Saville-Kent. 4to., pp. xv. 302. Illustrated by 50 full page collotypes, 9 coloured plates by Keulemans and other artists, and over 100 illustrations in the text. London: Chapman & Hall. 1897. Price, £3, 3s.

Naturalists of all classes, and a good many other people besides, including the inhabitants of the nursery, should be grateful to Mr Saville-Kent for producing such a magnificent picture book of the natural history of the most interesting and least known region of the earth, and for pouring out such a wealth of observation and entertaining anecdote as are to be found in his latest volume. We can attempt no summary of so discursive a work, but may perhaps give some idea of it by extracting a few of the new bits of information that it contains. The book is in some sense supplementary to Mr Saville-Kent's former fine volume on the Great Barrier Reef of Australia, reviewed in *Natural Science* for June 1893 (vol. ii., pp. 453-460), and deals chiefly, though by no means exclusively, with Western Australia, about which little has heretofore been written from the naturalist's point of view.

In chapter i. we are introduced to various aborigines of Western Australia, where they have been less exposed to the undermining influences of civilisation than in the more settled colonies. An advantage of civilisation, however, from the native's point of view, is the introduction of glass, whether in the form of bottles or telegraph insulators, from which wonderfully fine spear heads are manufactured, not by blows nor by breaking off with a bone, but by pressure with a hard stone or, preferably, a piece of iron. The frictional methods of kindling fire are described, but the author adds that they are seldom used—not because of the introduction of lucifer matches, but because it is the duty of the women to maintain the fire unquenched, and during migrations to carry lighted firesticks with them. This casts a light on the origin of the Vestal Virgins of antiquity.

A good deal has been written about the spurs on the hind feet of the duck-billed platypus. Mr Saville-Kent suggests that they are claspers used by the male (to whom they are confined) for the retention of the slippery female. Similar spurs are found in the male echidna, and in each case they are connected with a gland on the back part of the thigh. The echidna, also known as the spiny ant-eater, does not, it appears, eat ants at all—that is to say, not adult ants, but it breaks open the ant-hills and devours the nymphs, larvae, and pupae.

Another error common to the text books, is the representation of phalangers flying from tree to tree in a horizontal position or with the head lower than the rest of the body. The truth, according to our author, is that the head and shoulders are always kept at the highest level, with the forearms outstretched ready to grasp the first object reached. It is a pity that the photograph given is no proof, owing to the absence of accessories.

Passing from mammals to birds, we regret to learn that the lyre-bird, *Menura superba*, is in danger of extinction. At the same time we can hardly wonder at it, since it is the supposed duty of every globe-trotter to bring home for his female relatives, present or future, a pair of the splendid tail-feathers to which the bird owes its name. In the excellent chapter on birds the greatest space is devoted to the fern-owls, *Podargus strigoides*; and the humorous series of photographs, illustrating the remarkable changes of form and expression in these quick-change artistes, should render them familiar in our mouths as household words. The familiar name, however, "more-pork," is based on a misapprehension, since the bird which utters this melancholy cry is really *Ninox boobook*. Other birds on which valuable notes are given are the Queensland shrike (*Cracticus torquatus*), the N. Queensland laughing jackass, various finches (*Poephila*), and the firetail (*Estrelda bella*.)

Zoologists will not be surprised to find a large space devoted to the frilled lizard, *Chlamydosaurus kingi*, since they will all be familiar with the interesting observations that Mr Saville-Kent has published on this reptile. Reptile one must call it, though its favourite mode of progression is rather that of the Anglo-Saxon messenger in "Alice through the Looking-glass," as shown in the figures. Another text-book error is to represent this animal with its frill extended, but with its mouth closed, a physiological impossibility, for the frill is supported by processes of the hyoid or tongue-bone, which are pressed out by the fall of the lower jaw. The bearded lizard (*Amphibolurus barbatus*), the mountain devil (*Moloch horridus*), the stump-tailed lizard (*Trachysaurus rugosus*), and many others are vividly brought before us by the author's pen and camera.

Chapter iv. introduces those marvellous structures, the homes of the termites or white ants, and gives some striking photographs of them. Among other things not generally known, we are told that both termites and termitaria may be used as food. The animals themselves, though eaten in Africa and India, do not yet grace the menu of Australian colonists or black-fellows, but the latter satisfy their hunger with the earthy substance of the mounds, which contains a large amount of proteaceous matter in the form both of termite-secretions and of microscopic fungi. Here we may also note that the green ants, described in another chapter, make, when mashed up in water, an acid drink pleasant to the European as well as to the native palate. Perhaps Mr Saville-Kent knows that Swedish children acidulate lump-sugar by leaving it in an ant-hill for half-an-hour. As for the food of the termites themselves, it is only too well known by those who have spent any time in our southern colonies, that many species have such a craving for wood that they will eat one out of house and home if constant care be not exercised. Their efforts produce a result like the sleeping palace of the fairy-tale, in so far as furniture and walls are outwardly sound but crumble to dust as soon as touched. There is therefore some consolation in learning from this book that the mound-builders do not eat wood but grass, sallying forth from their fortresses by night along hastily constructed covered ways, reaping the harvest, and garnering it for future use. It is probably the silica contained in the grass-stalks that imparts such firmness to the walls that these Neuroptera build. Before leaving them, we notice a figure, here published for the first time, of an infusorial parasite of the Tasmanian termite, described by Mr Saville-Kent under the name *Trichonympha leidyi*.

Though this volume does not deal with marine life to the same extent as did its author's last monograph, yet room has been found for a fascinating account of the island group known as Houtman's Abrolhos, off the coast of Western Australia. Here, in consequence, it is conjectured, of a southward flowing current from the Indian Ocean, there is a tropical marine fauna, including coral islands in all stages, situated in a temperate climate, and only a few hours' sail from the port of Geraldton. Mr Saville-Kent urges the advantages offered by Houtman's Abrolhos for the foundation of a biological station, and his account inclines one to cut the painter of bread-winning necessity and set sail for these Treasure Islands without delay. The guano, for which these islands are worked, need not deter us, for it is "absolutely devoid of smell." This, however, does not suit the farmer, who values his manure by its stink; and appropriately malodorous chemicals must be added before the guano can be placed on the market. Upon these reefs we shall find specimens of the corals which Mr Saville-Kent here describes and figures, apparently for the first time, as *Madrepora protaciformis* [*sic*] and *Montipora circinata*. Another new species, that may be found here, is the magnificent nudibranch mollusc *Doris imperialis*, which forms the subject of a coloured double-plate.

Brilliancy of colour also characterises many of the fish found in Australian waters; and that the chromo-plates of Syngnathidae (sea-horses) and Plectognathi are far from exaggerated in this respect will be admitted by anyone who has visited the little aquarium started at Hobart by Mr Saville-Kent himself. That these colours are more brilliant in the mating season, and therefore due to sexual selection, is not proved for all species, but is known to be the case with *Monacanthus rudis*, even as it is with our familiar stickleback. A very important observation recorded by our author is that on the latent colour-markings of certain fish. In the daytime longitudinal colour-bands are conspicuous, but at night there appear further dark transverse markings. These markings, controlled by the nerve-centres in the adult (as proved by a blinded fish, which behaved as though it were always night) are, in some cases at least, constant in the young, a fact suggesting that the species are derived from transversely-banded ancestors.

Chapter vii. does for the pearl-fisheries of Western Australia what the author's former work did for those of Queensland. We are not surprised to read that Mr Saville-Kent has unpleasant memories of wading ashore through the mud-flats of the port of Broome, which is the headquarters of the pearl-fishing fleet, and we are happy to be able to assure him that the substantial jetty for which he longs has already been built, and that from it there embarked as many as fifty passengers only a month or two ago.

Marine miscellanea are dealt with in the following chapter, which contains the account of a remarkable anemone, *Acrozoanthus australiae*, that builds itself a home on the outside of the tubes of a nereid worm. This the worm does not like, and stretches out its habitation in another direction. The anemone, equal to the emergency, follows the new branch, whereupon the worm strikes out again like a doubling hare. The process continues till the anemone secures its inevitable victory, and results in the formation of a singularly regular zig-zag polyp-stock. Many of these polyparies grow side by side on submerged rocks, sticking upwards when covered by water but hanging down when exposed by the ebb of the tide like the corkscrew ringlets of an old maid. We feel it our duty to note that in this chapter another new species of coral is proposed, "provisionally associated with the title of *Turbinaria revoluta*." Some day naturalists will recognise the futility of excusing their new names on the ground of their "provisional" nature. At present the phrase is generally diagnostic of the amateur, and should be shunned by so accomplished a naturalist as Mr Saville-Kent.

Insect oddities and vegetable vagaries are the titles of the last two chapters, to which space does not permit further allusion. It is, however, in these that some of the most beautiful illustrations of the volume are contained, notably of the shy-flowering cacti. Of the other illustrations, those of most interest to the naturalist are of the animals taken under water: and in this new branch of photography the author has made good progress since we first had the pleasure of calling attention to his efforts. The group of holothurians (*Colochirus anceps*), is a notable and instructive example of this *genre*. A word of praise is due to Messrs Waterlow, whose reproductions and printing do the fullest justice to the art of the author. The chromoplates are ambitious, but, with the exception of plate 4, representing a madrepore-reef, and Mr Frohawk's drawing of *Chlamydosaurus*, they do not appeal to us. The attempt to reproduce the vivid colours of the animals results in glaring masses devoid of life and natural chiaroscuro. It is with the camera pure and simple that the author is most successful, and he has learned the art of applying the scissors to his photographs with the happiest results. We wish, for his own sake, that he would apply those useful instruments to his prose. His golden rule is: never use one syllable when a word of four syllables is to hand, never use one word when six will do, and don't bother too much about the meaning of your phrases. When he wants to tell us that a certain lizard will eat any food, he says "the gastronomic proclivities of Trachysaurus are essentially omnivorous," and it amuses him to speak of a hansom cab as "that indispensable anticlimax of British Citizenship." To photograph an animal is "to immortalise it with the camera," an expression which shows that the author properly appreciates his own work. Neither can we fail to be struck by the number of slips in the names of people, and even in some of the long words so dear to him. Thus we find H. F. Blandford for W. T. Blanford, J. D. for G. D. Haviland, R. C. for A. C. Haddon: Gunther for Günther, Rontgen for Röntgen: R. M. Johnston of Hobart is called Johnson, though he must be well known to Mr Saville-Kent: even four of the officers at the very museum where the author was formerly an assistant are incorrectly referred to: Ipswich is confused with

Norwich. The bushman who says "triantelope," or, as we have heard, "triantulope" instead of tarantula, is said to be "less illiterate": what particular malapropism ought we to apply to a scientific writer who uses such unusual spellings as "chaelae" "fulchra," "mede," "Myrmicobius," "vestigeal," "synonomy," "Ostraea," "Geomiter," "Rhoea," and "spinnaret"? If, as our author might say, this sumptuously embellished volume be dedicated to a public with a predilection for the literary pabulum furnished by the now senescent lions of the *Daily Telegraph*, then our critical shafts are supererogatory. But the book deserves a higher circle of readers and a longer life, and therefore deserved a trifle more trouble in the preparation. Let Mr Saville-Kent learn, before it is too late, that one cannot take a snap-shot at immortality.

The Fossil-spotter's Manual

Die Leitfossilien. Von Ernst Koken. 8vo, pp. 848, with about 900 illustrations in the text. Leipzig: C. H. Tauchnitz, 1896. Price, 14 marks.

The object of this book is not to teach palaeontology, but to present the geologist with a means of discovering for himself the genera to which his collected fossils belong: the book may be described, in brief, as a guide to fossil-spotting. The aim is not one with which we have great sympathy; but within limits such a work is of value. Dr Koken will certainly have done good service if his book leads any geologists or others to pay more attention to the essential diagnostic characters of genera and species, as detailed by their authors in the text of their monographs, and to rely less on the superficial features shown in the illustrations, which, as every worker knows, are often incorrect.

The book is professedly incomplete, dealing as it does only with Invertebrata, and omitting even from them such forms as are not of much use to the stratigrapher. All the Tertiary species, too, find no place in the second half of the book, although the more important genera are discussed in the systematic section. The illustrations also though many are good, are very unevenly distributed. A book of this kind needs more diagrams, such as those of *Cardinia* (p. 200), *Megalodon* (p. 205), goniatite suture-lines (pp. 60, 61), and trilobites (p. 18), and can well spare elaborate pictures, such as that of the rare Silurian *Pollicipes* (p. 6, or the uninstructive *Polyjerea* (p. 332). Illustrations that suited Dr Koken's excellent semi-popular work "Die Vorwelt" (see *Natural Science*, vi., pp. 127-129, Feb. 1895) are not adapted to the present student's manual, however much the publisher may wish to utilise old clichés.

The first part of the book consists of a series of analytical keys, arranged in the form of short paragraphs, each connected by reference numbers with those that follow. It is an attempt to reduce dichotomous tables to the requirements of the printed page, and is at first somewhat perplexing. Let us try it in practice. Here is a small brachiopod from the Upper Chalk. Section I. is "without hinge": this has a hinge: turn to section II. II. A. are forms without free arm-skeleton; our fossil, however, has an arm-skeleton consisting of a narrow loop, which projects forward into the shell cavity, and this places it in II. B. c. 2. The first sub-division of this is according to the length and curve of the loop, and our brachiopod agrees with the second paragraph, "Loop recurrent, long. 11." Turning to 11, we read "The loop free," which does not agree with our specimen; and then, "The loop again fixed to the median septum of the small valve. 16." Reference to 16 again gives us two sub-divisions, the first of which includes shells that are "Smooth, . . . have large foramen, and rudimentary deltidium. 17." Number 17 includes 3 genera, *Kingena*, *Magas*, and *Rhynchora*. *Kingena* has "median septum in the large valve," and a "cross-band connecting the recurrent loop-bands"; these structures are not found in our fossil. *Rhynchora* has "hinge-line straight, long, large valve with area"; this also does not fit. We are therefore restricted to *Magas*, and find in fact that the specimen agrees with the characters here ascribed to that genus. Now this is admirable, and as scientific as it is possible for such keys to be. But how often will the student or the field-geologist have a specimen of *Magas pumilus* showing all, or even a few, of the necessary characters? Not one specimen in a hundred shows them. In fact Professor Koken himself says of the Brachiopoda: "Since the delicate calcareous bands are usually destroyed or only discoverable by laborious preparation, other characters have to be used in practice." In short, give the student a decent work of reference, such as Davidson's Monograph or the "Paléontologie Française," and he will have determined genus and species long before you have made up your mind whether the specimen has a brachial skeleton at all. The truth is that the principles of classification are one thing and the methods of fossil-spotting are another. The first essential for the latter is an extensive acquaintance with specimens. Any collector of Chalk fossils can tell *Magas pumilus* if he has once seen it. When he has this acquaintance, then he can proceed to the true knowledge required for the best systematic work. We must learn these concrete sciences like we learn a language: get a good vocabulary first, and proceed to the structure and syntax afterwards.

The second section of the book gives short diagnoses of the chief species characteristic of the various formations, and is to be used after one has determined the genus. It is inevitably incomplete, and chiefly intended for German students. Even for the fossils of Germany it is not to be relied on without confirmation by the more complete original monographs; and this being so, it is a pity that there are no references to literature. The fortunate collector of a *Taxocrinus rhenanus* certainly should not be able to identify it as a *Cyathocrinus*, the genus in which Dr Koken leaves it. Some of the genera and species to which reference is made, especially among the Gastropoda, we have been unable to discover in literature at all, and have a strong suspicion that they are here introduced for the first time (*e.g.*, *Ectomaria*, p. 395). This is undoubtedly the case with the name *Amorphocystites*, introduced in a footnote (p. 411) as proposed by Jaekel for *Caryocystis testudinarius* Von Buch and *C. pumilus* Eichwald. It is very doubtful if any such change of nomenclature be needed; and in any case this hole-and-corner method of bringing out new names has never yet been productive of aught but confusion, and we are astonished to find it adopted by so careful a worker as Professor Koken.

We realise the enormous labour expended on this work, which may be of use to many under the guidance of a good teacher, and as a supplement to scientific palaeontology on the one hand and to field-work on the other. But we ourselves prefer Professor Koken when playing his other parts of original investigator or high-class populariser.

Miniatures by Hansen

The Choniostomatidae. A Family of Copepoda, Parasites on Crustacea Malacostraca. By Dr H. J. Hansen. 4to, pp. 206, with thirteen copper plates. At the expense of the Carlsberg Fund. [Author's Motto:—"We want facts, not inferences, observations, not theories, for a long time to come."—*Natural Science*, 1896.] Copenhagen: Andr. Fred. Host & Son, 1897.

Within the memory of men still living an artist could obtain a respectable reputation and a good income by painting miniatures. The features of the original might reach any assignable degree of the plain and the commonplace. It mattered not; the portrait on ivory was always like and always lovely. All this delightful flattery has been destroyed or banished by photography, cheap and (sometimes) cruel. But Dr Hansen's volume proves that there are mysteries of portraiture with which the camera is still incapable of dealing. Though the likenesses are not those of decorated officers or fashionable beauties, but of forms more fitted to excite wonder than admiration, the picture of each is drawn by him with exquisite delicacy of touch and the most minute attention to detail. Each is confined within the compass of an inch or two. But really this is a gigantic enlargement. The true miniature is the natural object, often only one-hundredth of an inch in length, and sometimes much less. Under a powerful microscope animals of this size may become decently conspicuous. The same can scarcely be said of the mouth, which in the Choniostomatidae is not only absolutely but relatively small. It may be left to professed arithmeticians to calculate the dimensions of their two pairs of antennae and three pairs of jaws and the joints thereof, all which need observing for purposes of full and accurate scientific description. When it is added that the animals are not transparent, and that they will not submit to pressure, the microphotographer will probably leave them for the present, without attempting to challenge the deftness of Dr Hansen's pencil.

For the neglect which this curious family has till lately experienced there is more excuse than usual. The poet might bewail that in labouring to be short he became obscure. These Copepoda were probably short without labour and obscure by preference. How else can we account for their choosing to belong to the neglected class of crustacea, choosing a life of self-effacement within that class, choosing their hosts chiefly among its unpopular and little known sessile-eyed groups, and burying themselves for the most part in brood-pouches and branchial cavities? To be plain, they are crustaceans parasitic on crustaceans principally on Amphipods, Isopods, and Cumacea, having been found in only a few instances on stalk-eyed shrimps. A solitary species courts the public gaze on the outside of its host's body.

Some prejudice attaches to the habit of existence in which these creatures indulge. On the other hand, the Parasite in Lucian maintains that his profession and personality are the true charm and glory of social life. The parasite in zoology may urge in its own favour that it is an eminent preacher and teacher of cleanliness, and an unanswered advocate for the theory of evolution. The family Choniostomatidae is at present divided into six genera. Forty-five species are known, chiefly through Dr Hansen's researches. The first published, however, was *Sphaeronella leuckarti*, described by Salensky in 1868. Thus, so far as their history is known, it is open to suppose that the whole batch has been specially created within the limits of the present century. But the reverential motive which prompts hypotheses of that kind is surely undermined when they require us to contemplate one set of crustaceans as specially contrived to live and multiply, and another set of crustaceans as specially contrived to be vampyres on the first set, and to stop them from breeding. The latter strange effect produced by the presence of some crustacean parasites on their crustacean hosts was first expounded by Prof. Giard. Dr Hansen finds reason to believe that, as a rule, with the exceptions to which all rules are liable, the Choniostomatidae prevent their entertainers from rearing a family. With the opinion advanced by Giard and Bonnier in regard to the Epicaridea, that each parasite has its particular host, and is found on no other species, he does not fully agree, and he also adduces evidence to show that such a rule is not applicable to the whole of the present group. Certain members of it have been discussed by the French authors just mentioned, and some of their results are subjected to rather severe criticism. This, amid the intricacies of a new subject, will be highly acceptable to the general reader. Apples, for choice, need a subacid flavour. They must not be so sharp as to set one's teeth on edge. As the eminent authors reciprocally compliment one another in the names of the new species, there is evidently here no very desperate quarrel. By the extraordinary patience with which during several years Dr Hansen has been accumulating his observations he is entitled to be a little intolerant of more rapid methods, which cannot fail to be hazardous in a material so difficult. The remark which he quotes on his title-page, "We want facts, not inferences, observations, not theories, for a long time to come," is from *Natural Science* itself, so it must be true, and a paragraph of his own, beginning, "Now-a-days many authors have a remarkable weakness for publishing innumerable immature notes," deserves cosmopolitan circulation. In another passage Dr Hansen says, "I confess that, though I honour everybody who is capable of suggesting a theory which proves to be well founded and fertile in results, I have always felt, and, as time goes on, feel more and more distaste for superficial conjectures." But this is almost like saying, "There are too many anglers; what we want is fish." People will go on angling to please themselves, without regard to what we want. Allowance must be made for differences of temperament and taste. Some misguided persons hear of the discovery of new families, genera, and species with a stolid want of enthusiasm. They perhaps for their part think nothing important but the course of the nerves or the action of the hepatopancreas. Mr Henslow dismisses the origin of species by means of natural selection as a superficial conjecture, and hopes for a speedy recognition that Darwin's deduction, as he calls it, was a most unfortunate one. Supposing it to have been so, the world could well do with one or two more misfortunes of a similar kind.

Dr Hansen's book is in English. This is evidently part of a conspiracy to discourage the English-speaking peoples from studying foreign languages, a plot in which Russia has, unfortunately, not yet joined. The translation from the Danish manuscript has been well executed by Miss Louise von Cossel. It is unlucky that one frequently recurring word has been too literally rendered 'list,' not in any of the accepted English senses of the word, but to signify a ridge or linear prominence, or possibly a seam or an unraised line of hardening of the integument. In naming the mouth organs Dr Hansen himself adopts the terms maxillulae and maxillae, respectively for the first and second maxillae, on the analogy of antennulae and antennae for the first and second antennae. The great objection to these terms is that sometimes the first maxillae and the first antennae are larger, even very much larger, than the second, and then the diminutives are misleading. As a matter of fact, in a paper published only last year by F. Vejdovsky, the second antennae are called the antennules. The confusion is not unnatural in describing Amphipods, which often have the second antennae shorter than the first, occasionally less than one-fifth as long. But these are not matters of vital concern. For the pith and marrow of the research the reader must have recourse to Dr Hansen's volume. It is a masterly piece of work, which will confirm and increase his high repute as a naturalist of distinction.

T. R. R. S.

Prehistoric Problems, being a Selection of Essays on the Evolution of Man and other Controverted Problems in Anthropology and Archaeology. By Robert Munro, M.A., M.D. 8vo, pp. xix. + 371. London: Blackwood, 1897. Price, 10s.

In these days of scattered scientific literature, the bringing together into a single volume of a number of essays by one author is a very desirable thing, particularly when, as in the present instance, the author is a scientist of distinction. Although the volume contains comparatively little that has not already appeared in print, Dr Munro's newly-published selected essays on "Prehistoric Problems" will be welcomed by many as a valuable addition to Archaeological literature. The book consists of a number of chapters, each of which is a separate and distinct essay. This collection of essays is of a decidedly heterogeneous nature, comprising as it does so varied a selection of subjects as: The Rise and Progress of Anthropology; Man's Antiquity and Place in Nature: Prehistoric Trepanning; Otter Traps; Bone Skates; and Prehistoric Saws and Sickles. The very varied nature of the subjects discussed imparts a character of inequality to the volume, and imposes a certain lack of proportion, which is evident to the reader who, taking the book as a whole, would read it straight through from beginning to end. Taken individually, the essays are decidedly both instructive and interesting, and the first four, which form Part I. of the volume, may well be taken together, as they form a very fairly connected and consecutive series.

They chiefly deal with the Antiquity of Man and his Place in Nature, together with a brief history of the scientific study of Anthropology, a field wide enough to have filled the whole volume, and one which we would gladly have seen treated in a more complete and less condensed manner by Dr Munro. So far as the allotted space admits the subject is skilfully handled, and the points are clearly brought out; the style, too, is simple, so that it does not require a trained scientific mind to grasp either the general conclusions or the details. The first part of the book is, in fact, well suited to the general reader, as well as of value to the scientist. Dr Munro is hopeful in regard to the possibility of bridging over the gap between the Palaeolithic and Neolithic civilisations in Western Europe, and advances the important finds of M. Piette in the Mas-d'Azil cavern, and the curiously similar finds in a cave at Oban described by Dr J. Anderson, as helping possibly to link the two periods. The evidence of a continuity between the two periods is not as yet sufficiently complete, but a step has been made in the right direction, and Mr A. Evans' researches in the Balsi Rossi caves are much to the point in this connection.

One of the most interesting essays in the book is that dealing with the importance of the assumption of the Erect Posture as a factor in the physical and intellectual development of Man. Dr Munro is a strong advocate of the enormous advantage which Man derived from the attainment of the erect posture, and the consequent differentiation of the limbs into hands and feet; in other words, the releasing of the fore-limbs from locomotive duties, so that they might become the servants of the brain in other directions, and thus assist the development of mental qualities. The position of *Pithecanthropus erectus* in the human phylogeny is reviewed in a judicial manner, and it is pointed out how the calvaria and femur of this seemingly intermediate type bear out the theory of the erect posture having preceded the higher development of the brain in Man.

A slight rearrangement of the material in Chapters II. and IV. would have obviated a certain amount of repetition in connection with this point.

The second part of the volume, headed "Archaeological," comprises four essays on quite distinct and unconnected subjects. These will probably appeal less to the general public than those contained in Part I., as dealing with more special points of archaeological interest. The chapter on "Prehistoric Trepanning" is well worth reading, and the subject is rather to the fore just at present, it having been discovered that, in addition to the interest attaching to the primitive surgical methods adopted in conducting so important an operation, and the fact of the patient having so frequently recovered, there is also a good deal of folk-lore connected with the practice, well worthy of study. Dr Munro has brought together, in Chapter VI., all the available data regarding the curious wooden objects which he on fairly good grounds calls "otter and beaver traps." He handles the subject with skill, and, in the absence of direct evidence, the probable use of these objects can only be arrived at by comparative study of the examples. Space does not allow more than the mere mention of the essay on "Bone Skates," whose claims to be in some instances considered as prehistoric are called in question, and that on "Prehistoric

Saws and Sickles," in which is given an excellent general account of these implements, together with views on such controversial matters as, for instance, the use of the wooden-handled flint "saws" from Polada, which Mr Spurrell regards as sickles allied to those found by Mr Petrie at Kahun.

The illustrations are numerous, and for the most part good; the text is not always free from blemish, in the shape of curious printer's errors, which have survived the proof-reading ordeal; there are also sundry awkwardly turned sentences. These, however, do not in any way affect the value of the work, nor do the unimportant, if inartistic slips, impair our indebtedness to the author. H. B.

Some Elementary Text-Books.

First Stage Physiography (The Organised Science Series). By A. M. Davies. 8vo, pp. viii. 238, with 110 illustrations. London: W. B. Clive & Co., 1897. Price, 2s.

Elements of Physical Geography. By S. B. J. Skertchly. 28th Edition; revised by J. H. Howell. 8vo, pp. viii. 224. London: F. Murby, 1896. Price, 2s.

A Text-Book of Geology. By W. J. Harrison. 8vo, pp. viii. 343, with 140 illustrations. London: Blackie & Son, 1897. Price, 3s. 6d.

First Stage Mechanics of Fluids (The Organised Science Series). By G. H. Bryan and F. Rosenberg. 8vo, pp. viii. 208, with 77 illustrations. London: W. B. Clive & Co., 1897. Price, 2s.

First Principles of Natural Philosophy. By A. E. Dolbear. 8vo, pp. x. 318, illustrated. Boston, U.S.A., and London: Ginn & Co., 1897. Price, 4s. 6d.

The constant alterations in the syllabus for Physiography in the Science and Art Department's examination render necessary a continual series of new or greatly revised text-books. Mr A. M. Davies' "First Stage Physiography" will, therefore, no doubt be extremely useful. It has all the merits of a good elementary text-book; it is concisely and clearly expressed, it is thoroughly reliable and up to date; it is illustrated by a series of well-selected diagrammatic figures of which many are new; and the definitions are explained by homely illustrations which are so chosen as to be very suggestive to an intelligent student. The only point in the book we regret is the use of the metric system for all dimensions, an innovation in an elementary book on this subject which we think hardly likely to lead to accurate perception among students. It was perhaps a pity to refer to a lustre in the explanation of the form of a prism; for as the point in which students most often go wrong is by regarding a prism as a triangular pyramid, an error for which comparison with a lustre, which has a pointed end, is apparently generally responsible. It is not quite correct to say that the snow-line reaches the sea-level in Greenland. But except for one or two trivial points like these, there is nothing in the book with which we can find fault. We can only wish the book the circulation it deserves.

Opportunity has been taken of the issue of a 28th edition of Skertchly's small "Physical Geography" to subject it to extensive revision, which might, however, have been made even more thorough. The book, as it now stands, has many good points, the chapter on "Astronomical Relations" being probably the best; subjects such as the precession of the equinoxes, and the method of finding latitudes are generally stumbling blocks to the beginner, but they are here clearly explained. The main points to which the editor might attend in the preparation of a future edition is greater uniformity of standard, and the reduction in number of needless technical terms. In regard to the latter, the editor even proposes new terms in the course of the book, describing some springs as "transtatic." Even if the term were useful, its first publication in a shilling text-book could hardly be commended. The restriction of "isothermal" to mean annual temperature is neither usual nor convenient. There are still many points in which revision is necessary; Africa is not now regarded as exempt from earthquakes (as stated on p. 154); it is too late to say that the cause of the rising of the Nile is covered by "much obscurity," or to affirm that glacier ice is "not plastic." The geological classification of lakes into two divisions only (p. 111) is quite inadequate, while it is only burdening a student with useless definitions to separate rivers into oceanic and continental, according to whether they flow into the ocean or not. The appendix on the geographical distribution of animals could do with thorough revision: *Colubus* is not a "tail-less ape" (p. 209); and to say that the long-tailed manis and the ground-pig are "almost exclusively African" is an error from excess of caution; the python is not only found in the Indian region; *Lepidosiren* is not a reptile, and it is not excusable now to include the crocodile among the lizards.

Mr W. Jerome Harrison is a very experienced science teacher, a practical geologist, and has always shown himself a painstaking and accurate worker; hence it is not surprising that his "Text Book of Geology" has reached a fourth edition. It now appears so much enlarged and revised that it is practically a new book. The syllabus for geology issued by the Science and Art Department is reprinted at the end, accompanied by the questions set at the May examinations for the past eight years. This fact suggests the class of students the author wished to help; and for the elementary stage of that examination we know of no better class-book. The book is, as a rule, reliable and well up-to-date: but we notice a few old figures that might have been omitted, and a few points that might be revised. The author might have added the supposed land plant *Berwynia* to the list of his pseudo-fossils, instead of accepting it as unhesitatingly as he has done on p. 177. The pre-glacial age of man is not proved by either the Cae Gwyn Caves or the Brandon implements. The explanation of the Moel Tryfaen shells as "*pushed up* to their present heights in front of" a glacier [the italics are Mr Harrison's] is one of the type of explanations which prejudices the anti-marine theory. The statement (p. 186) that crinoids "are dying out, a few specimens only lingering at the bottom of the deep seas," is a survival from twenty years ago, which still lingers in many elementary works. Another common mistake is regarding the Neocomian as the equivalent of the whole of the Lower Cretaceous. The illustrations are numerous and good, and we hope the book will soon reach a fifth edition.

The fourth and fifth books are not quite within our range; but geographers and geologists occasionally have to deal with questions to which some knowledge of the mechanics of fluids is essential. We therefore need make no apology for calling attention to works in which the elementary principles of the subject are clearly and simply taught.

The Nectaries of Flowers

Beitrage zur Kenntnis der Septalnectarien. By J. Schniewind-Thies. 8vo, pp. 87, with 12 plates. Jena: Gustav Fischer, 1897. Price, 15 marks.

This volume, with its large well-spaced text and its beautiful supply of nicely lithographed plates, including 266 figures, once more brings home the fact of the extreme specialisation of present-day science. It is surprising to know that Mr Gustav Fischer can find it worth while to publish at fifteen shillings an independent work dealing with a special kind of simple honey-secreting tissue, and containing about as much matter (if we except the plates) as half of a single part of our Linnean Society's Journal.

Septal nectaries are the honey-secreting layers found, sometimes on the outer surface of the ovary, but generally in the walls separating the ovary chambers, in many genera of Liliaceae and other petaloid monocotyledons. They have attracted the attention of various botanists during recent years, and we could add to the references to papers cited in footnotes by Mr Schniewind-Thies. The author gives an account of the structure and position of the nectaries in genera of Liliaceae, Amaryllideae, Scitamineae and Bromeliaceae, and distinguishes seven groups. In the simplest the secretion is effected by the epidermal cells of the whole exterior surface of the ovary, from its base to the origin of the three style-arms. The only examples given of this are in two species of *Tofieldia*, one of the simplest genera of Liliaceae. In the second group a "double nectary" is found, secretion occurring on the surface of the ovary in three furrows lying along the septa, and in three slits which permeate the separating walls of the carpels. Examples are found in *Yucca* and *Agapanthus*. In the third group there are no superficial glands, secretion occurring only in true septal slits as in *Funkia* and species of *Allium*. Where the ovary is only partly superior a double nectary may occur in the upper part and internal ones only in the lower, as in *Haworthia* and *Urginea*, or only in the inferior part, as in *Phormium* and other Liliaceae, where a further complication ensues in lateral branching of the slits and strong development of vascular tissue in their vicinity. Where the ovary is wholly inferior, as in Amaryllideae, Irideae and Scitamineae, and some Bromeliaceae, secretion is confined to three septal slits, or occurs also in three outer furrows at the thickened style-base. In Bromeliaceae, with a superior or half-inferior ovary, the most complicated arrangement is found, since, besides the double nectary as described for the second group, there are also three internal glandular surfaces penetrating the dorsal suture of each carpel, and opening upwards into the ovary-chamber. Thus, it is suggested, increased complication in the form of the nectary accompanies a similar change in final complexity. In the second part of the paper the histology of the secreting cell and the part played by the various constituents of protoplasm and nucleus are discussed. In conclusion, we must again refer to the great number of excellent drawings, which add greatly to the interest of a communication consisting largely of somewhat detailed structural and histological descriptions of individual cases.

The Origin of the Diamond.

Papers and Notes on the Genesis and Matrix of the Diamond. By the late Henry Carvill Lewis: Edited from his unpublished MSS. by Professor T. G. Bonney, D.Sc., etc. London: Longmans, Green & Co., 1897. Pp. 72, with 2 plates and 35 figs. Price, 7s. 6d.

The late Professor Carvill Lewis was much interested in the remarkable occurrence of the diamond at Kimberley, and shortly before his death devoted considerable care to a study of the rock in which the diamonds are found. He communicated two papers on the subject to the British Association in 1886 and 1887 with the intention, apparently, of continuing his researches and of writing a book on the general question of the origin and occurrence of the diamond. This work was cut short by his premature death, and the present volume contains merely the full text of the two British Association papers, with a few notes and an appendix by Professor Bonney.

The most diverse opinions have been held regarding the nature and origin of the peculiar rock, known as "Blue Ground," in which the diamonds of Kimberley are embedded. A vertical column of serpentinous material, unlike anything else upon the surface of the earth, extending to an unknown depth, and of enormous dimensions, it was supposed by some to be the neck of a volcano, by others to be a volcanic breccia due to a sort of mud eruption. The object of Professor Lewis's papers is to show, by an elaborate and minute microscopic study of the rock itself, that it was a true igneous lava, or, to use technical language, the 'Blue Ground' was, according to him, a porphyritic volcanic peridotite or basaltic structure, an olivine-bronzite-picrite-porphyrite, rich in biotite (now very much decomposed), and for this remarkable rock he proposed the name "Kimberlite."

The chief argument upon which his conclusions were based is that in two American localities, namely at Syracuse, New York, and in Elliott County, Kentucky, a precisely similar rock occurs, though without diamonds, and is there obviously an eruptive rock. Professor Bonney's appendix consists of a detailed description of these two rocks, which he also regards as practically identical with "Kimberlite," although he does not quite agree with Professor Lewis's views concerning the origin of the latter.

It has generally been supposed that the diamonds in the blue ground were either caught up from some underlying rock or are due to the fusion of the carbonaceous shales through which the blue ground passes, or are decomposition products. Professor Lewis emphatically states his opinion that the diamond is an essential constituent of the rock like any of the other minerals which it contains; in this view he probably stood alone at the time of his death, and it is not one which has been generally accepted since.

It cannot be said, therefore, that these papers contribute much to our knowledge of the origin of the diamond; they constitute a careful description of the rock in which the precious mineral occurs and establish the existence of a similar rock elsewhere, but no reason is suggested why it only contains diamonds at Kimberley.

Professor Bonney has done well in giving these posthumous papers to the world, and has considerably enhanced their value by appending his own description of the American Kimberlites. As he states in the preface that he has purposely avoided all reference to more recent literature, the reader must be content to miss any allusion to the occurrence of diamond in meteorites, although the resemblance between Kimberlite and certain meteorites is frequently mentioned, neither will any account be found of recent experiments upon the solvent action of the blue ground upon diamond, or of the artificial production of the mineral.

For these reasons the book can only be regarded as a publication of papers that should have appeared ten years ago, which, though interesting and important as a petrographical study, do not throw much light upon the vexed problem of the genesis of the diamond.

H. A. Miers.

Landslips.

Report on the Geological Structure and Stability of the Hill-Slopes around Naini Tal. By T. H. Holland. Officiating Superintendent, Geological Survey of India. Pp. viii., 85, with a map and 11 plates. Calcutta: Office of the Superintendent of Government Printing, India. 1897.

This report shows the practical value of a thorough knowledge of the geological structure of a district as affecting its suitability for habitation. It is entirely a practical work, written for the guidance of engineers and others familiar with the locality, and treats the subject from a purely utilitarian standpoint.

Naini Tal is a lake in the north-west provinces of India on the flanks of the Himalaya. There is a hill station located here, but the district suffers somewhat from the frequency of landslips. It is, indeed, probable that the lake owes its origin to the damming up of a stream by a great landslip, as was suggested by Dr Ball in 1878, though his views have not been universally accepted.

The object of the investigation, of which this report is the outcome, was to discover the cause of the instability of the hill-slopes in the district, to determine the extent of the insecure sites, and to suggest means for increasing their stability.

The methods adopted by the author were the following:—

(1) On a large scale (20″ to 1 mile) contoured map were inserted details of the distribution and petrological characters of the rocks.

(2) The angle of repose of the rocks under different conditions was determined.

(3) Cross-sections were constructed from the map showing the slope along the selected lines, and the portions of the rock lying outside the lines of safety were determined from the angles of repose.

The direction of the movements is shown in the report to be governed by the direction of the stratification planes, which in many areas have a dip in the same direction as the slope of the hill-sides but smaller in magnitude.

The rocks most affected are shales and dolomitic sandstones, and the lubricant is provided by the decomposition of the rock, which is brought about by water percolating along the stratification planes, and forming in the first instance a slippery clay, and in the second a layer of loose sand.

The great difference between the angle of repose of dry broken slate and that of the same material when disintegrated and wet, is shown by the fact that the former will stand at an angle of 37°, while the angle of rest of the latter is not more than 16°, and the author points out that "the maximum angle of safety of interbedded rocks is determined by that of the weakest constituent."

The author devotes a few pages to the classification of the landslips according to Heim's scheme, and proceeds to describe the warnings which foreshadow a landslip, such as the opening of cracks parallel to the strike of the beds and changes in the courses of streams. Besides landslips of the ordinary type there occur subsidences caused by the removal in solution of the dolomitic cement of the sandstones and the consequent settling down of the loose material.

The most efficient methods of preventing the landslips appear to be the provision of means for the removal of the rain-water before it can percolate into the ground, and also for the discharge of subterranean water by the construction of adits.

The Report concludes with a detailed description of three particular sites, whose characters illustrate the results of the various forces described by the author. The value of the work is increased by the well-drawn diagrams and sections, and by the excellent map for which a special survey was made; but it may be worth while to ask whether such terms as 'demi-official' and the contraction 'para' for paragraph are improvements on the conventional modes of expression.

Investigations into Applied Nature. By William Wilson, Junior. 8vo, pp. viii., 143. London: Simpkin, Marshall & Co., 1896.

This little book hardly calls for serious review. The author may be a pleasant enough companion in a country walk, if one were willing to be a little bored, and may also have certain powers of observation, but he is unable to arrange his matter in book-form or to express himself in English. The book is a strange conglomeration. The first few chapters relate to "Our Indigenous Flora as Food-Plants"; "On the Habits and Instinct of the Rook"; "Our Birds and their Functions"; "The Potato Disease"; and so on. The first paper has already been inflicted upon the British Association, another on the Inverness Scientific Society, and another on the Keith Literary Institute. Of pasture plants Mr Wilson says, "We have not as much in general use, taking the knowledge of variety as known to the average agriculturist into account, as we can scarcely say there is any variety in them." And speaking of the winter food of animals, "We find that human ingenuity has invented a large number of so-called spices or condiments to assist in feeding and keeping them (besides the fields' produce, turnips and straw), and are generally used." The funniest chapter is that on the crow, which, "like most objects of natural history, is very imperfectly understood." It seems to be a selfish and quarrelsome bird, but wily withal; "unusual operations on the part of man on the top of a stack is watched by the rook with suspicion, and in nine cases out of ten that stack will be avoided by them." "Atmospheric changes produce a very marked effect on them. There is no doubt but this causes the peculiar reeling in the atmosphere [!]. Before rainfall a dulness passes over them, early brightening up after the rain is over. Late out of doors, so to speak, before snowstorm, and the same again before a thaw in many cases." Dealing "with the horses of Britain, it is supposed that they were first introduced by Julius Caesar into history." The hackney "is a class of beast well adapted for many requirements to which horseflesh has been attached to"; and the sheep, "our most arduous animal inhabitant of pastures." In his preface the author congratulates the public generally on the spread of technical education and the increasing association of agriculture and pure science. We regret that we cannot congratulate either the author or the public on the appearance of these "Investigations," which tend to the advancement neither of pure nor applied Science.

A Malagasy Geology

Geology. Nataon-d Rev. R. Baron, F.G.S., F.L.S. Vol. I. Nohazavain' ny Sary 51. Pp. vii. + 91, with 51 figs. Antananarivo: London Missionary Society's College, 1896. Price, 6s.

This very interesting production of the London Missionary Society's press is the first and possibly the last geological work in Malagasy that we shall see. The author divides his work into three sections and twenty-five chapters, and deals with mineralogy, and the dynamics of volcanic, metamorphic, and sedimentary rocks, with notes on the several districts from personal observation and otherwise. Vol. II. is promised, and will deal with the fossils. As it is difficult to give a fair criticism on a book written in Malagasy we can only offer a specimen of the author's easy style:—"Koa ny horohorontany dia fipararetan' ny hoditry ny tany, fa mievotrevotra ka manalonalona hoatra ny rano izy, ary ny toetran' izany fievotrevony izany dia tahaka ny fitopatopan' ny alon-drano hiany. . . ."

Scraps from Serials

In the last number of *La Feuille des Jeunes Naturalistes* (No. 323, Sept. 1897), M. L. Vignal concludes his notes on the fossil shells of the family Cerithiidae from the Eocene of the Paris Basin, this final instalment being illustrated by two photograped plates.

In the *Scottish Medical and Surgical Journal* for September Prof. Cossar Ewart prints his address delivered at the Graduation Ceremonial in the Edinburgh University last July. He announces that he has "practically proved that, notwithstanding the statements of Weismann and the experience of scientific German breeders, there is apparently such a thing as Telegony." He promises to contribute a note on the subject to the next number of the same journal.

New Serials

Messrs Schleicher Frères of Paris announce a forthcoming international journal for zoology, botany, physiology, and psychology, entitled *Intermediare des Biologistes.* It is to appear on the 5th and 20th of each month, under the editorship of M. Alfred Binet. The price and the date of the first issue are not yet decided.

According to *Science*, a small scientific monthly of a popular character has been established at De Land, Fla., entitled *Studies from Nature.*

A syndicate in Boston has purchased the *American Naturalist* from the executors of the late Professor Cope, and the September number is the first issue under the new management. Dr Robert P. Bigelow is now chief editor.

Further Literature Received

Elements of the Comparative Anatomy of Vertebrates, adapted from the German of R. Wiedersheim by W. N. Parker, ed. 2: Macmillan. Volcanoes of North America, I. C. Russell: Macmillan. Guide to Zermatt and the Matterhorn, E. Whymper: Murray. The New Psychology, E. W. Scripture: W. Scott. A Critical Period in the Development of the Horse, J. C. Ewart: A. & C. Black. Beiträge zur Dioptrik, A. Kerber: Fock.

Variations in the Spinal Nerves of *Hyla aurea*, G. Sweet: *Proc. R. Soc. Victoria.* Affinities of *Tarsius*, C. Earle: *Amer. Nat.* Princeton Contributions to Psychology, vol ii., Nos. 1, 2. Baeveren i Norge, R. Collett: *Bergens Mus.* Field Columbian Museum, Chicago, Geol. Series, No. 18; Zool. Series, Nos. 19, 20; Anthrop. Series, No. 16. U.S. Dept. Agriculture, Rep. No. 9. Australian Mus., Sydney, mem. No. ii. (Atoll of Funafuti). Journ. Marine Biol. Assoc., vol. v., No. 1. U.S. Bureau of Ethnology, 14th and 15th Ann. Reports. Roemer Mus. Hildesheim, mitth. No. 9. The Glacio-marine Drift of the Vale of Clwyd, T. M. Reade: *Quart. Journ. Geol. Soc.* Anatomy of *Apera burnupi*, W. E. Collinge: *Ann. Mag. Nat. Hist.* Australian Termitidae, pt. ii., and other papers, W. W. Froggatt: *Proc. Linn. Soc. N.S. Wales* and *Agric Gazette.* Glacial Observations in the Umanak District, Greenland, G. H. Barton: *Technol. Quarterly.* The Diplochorda, A. T. Masterman: *Quart. Journ. Micro. Sci.* Ann. Rep. Trustees S. African Mus., 1896. Ratzel's History of Mankind, English Trans., pt. 20: Macmillan. Journ. Coll. Sci. Imp. Univ. Japan, vol. x., pt. 2. Bull. Nat. Hist. Soc. New Brunswick, No. xv.

Shooting Times, Aug. 21; Newcastle Courant, Aug. 21; Pharmaceutical Journ., Sept. 4; Amer. Geol., Sept.; Amer. Journ. Sci., Sept.; Amer. Nat., Sept.; L'Anthropologie, July-Aug.; Feuille des Jeunes Nat., Sept.; Irish Nat., Sept.; Journ. Essex Techn. Lab., May-July; Journ. Malacol., July; Knowledge, Sept.; Literary Digest, Aug. 14, 21, 28, Sept. 4; Naturae Novit., July-Aug.; Naturalist, Sept.; Nature, Aug. 19, 26, Sept. 2, 9, 16; Nature Notes, Sept.; Naturen, Aug.; New Age, Aug.; Photogram, Sept.; Psychol. Rev., Sept.; Review of Reviews, Aug.; Rev. Scient., Aug. 21, 28, Sept. 4, 11; Rev. Sci. Nat. Quest., vol. vii., No. 1; Science, Aug. 13, 20, 27, Sept. 3; Sci. Gossip, June-Sept.; Sci. Amer., Aug. 14, 21, 28, Sept. 4; Scot. Geogr. Mag., Sept.; Scot. Med. and Surg. Journ., Sept.; Victorian Nat., July; Westminster Review, Sept.

OBITUARIES

SAMUEL ALLPORT

BORN JANUARY 23, 1816. DIED JULY 7, 1897

WE learn from the *Geological Magazine* of the death of the veteran petrologist, Mr Samuel Allport, who was one of the pioneers in the microscopical study of thin sections of rocks, and one of the most generous helpers of the younger generation studying his favourite subject. He was born in Birmingham, where he resided for the greater part of his life. For eight years only he was absent as manager of a business at Bahia, in Brazil, and there he made his first original observations on geology, collecting the cretaceous fossils from the coast near Bahia and contributing a paper on the subject to the Geological Society in London in 1860. On returning again to Birmingham his interest was excited by the work of Dr Sorby on the microscopical study of rocks, and thenceforward he became an accomplished petrologist. He made his own sections with great skill, and amassed a large collection of slides. His papers, chiefly published by the Geological Society, were not numerous, but very valuable, and related almost exclusively to the structure of igneous rocks. In 1887 he received the Lyell Medal from this society in token of appreciation of his researches. In 1880 he quitted business occupations and became librarian of the Mason College, Birmingham, an office which he held for seven years, until failing health necessitated his retirement.

FRANCIS AURELIAN PULSKY

BORN 17TH SEPTEMBER 1814. DIED 9TH SEPTEMBER 1897

FRANCIS PULSKY, the great Hungarian patriot, and the friend of Kossuth, is dead. His political life needs no mention here. On his return to Austria after the Imperial pardon, he became Director in 1869, and in 1872 General-Director, of Hungarian Museums and Public Libraries. An archaeologist, Pulsky's chief claim to the remembrance of our readers is his "Copper Age in Hungary," which was published both in Magyar and German.

THOMAS BRUMBY JOHNSTON, the Queen's Geographer for Scotland, died at Edinburgh on September 9th, in his eighty-fourth year. He was the last of the firm, of which he became a partner in 1852.

SIR EVERETT MILLAIS died on September 7th. He was born in 1856, and paid especial attention to the breeding of dogs and stock, and for some time was editor of *The Stock Breeder*.

NEWS

The following appointments are announced :

Dr Rodet to be professor of bacteriology in the University of Lyons ; Dr W. Ernest Thomson to the chair of physiology in the Andersonian College, Glasgow ; Dr Alfred Osann to be teacher of mineralogy in the Chemical School of Mülhausen ; Prof. Raphael Blanchard to be ordinary professor of botany in the Medical Faculty of Paris ; Prof. Vladimir I. Belajeff, professor of botany in the University of Warsaw, to be director of the Botanical Garden in the same city ; Prof. Vladimir I. Palladin, of Kharkoff, to be director of the Pomological Garden at Warsaw ; Dr H. V. Neal, of Harvard, to be professor of biology at Knox College, Galesburg, Illinois ; Prof. George Ruge, of Amsterdam, to be professor of anatomy and director of the Anatomical Institute at Zurich ; Dr Joseph Baldwin and Wm. S. Sutton as professors, and W. W. Norman, as assistant-professor, of biology in the University of Texas ; Dr H. Fling, professor of biology at the Oshkosh Normal School ; Prof. H. de Vries and Prof. Ph. Stohr to be professors of botany and anatomy in the University of Würzburg ; Miss A. A. Smith as assistant in botany in Mount Holyoke College, Mass. ; Ernest Wm. MacBride as professor of zoology in McGill University ; and Dr Kihlman as assistant-professor of botany in the University of Helsingfors.

Women are now admitted to the College of Physicians in Chicago.

Mr Alex. White is the recipient of the silver medal of the Zoological Society as a reward for his researches in the fauna of Nyassaland.

Dr O. F. von Moellendorff, the well-known conchologist, formerly German Consul at Manilla, has removed to the German Consulate at Kovno, Russia.

Professor R. Koch has returned to South Africa to continue his experiments on the nature of the rinderpest. The conference of the South African States on this great scourge was held in Pretoria during the first week of August last.

The Cothenius Gold Medal of the Imperial Leopold-Caroline Academy has been awarded to Prof. Albert von Kölliker, the veteran anatomist of Würzburg. The Baly Medal of the Royal College of Physicians of London has been presented to Prof. E. A. Schäfer in recognition of his important researches in physiology.

The first Flückiger Medal—an honour to be awarded every five years by the German and Swiss Pharmaceutical Societies alternately—has been presented to Mr Edward Morell Holmes, Curator of the Museum of the Pharmaceutical Society of Great Britain. A short account of Mr Holmes' work in botany, illustrated by an excellent portrait, appears in the *Pharmaceutical Journal* for Sept. 4.

Mr William Shaus of Twickenham, late of New York, has presented his collection of tropical Lepidoptera, comprising over 10,000 specimens, to the American Museum of Natural History. Mr E. A. Hoffman has also presented to the same museum his collection of North American Lepidoptera.

According to the *Revue Scientifique*, the late M. J. Jackson left a legacy of 100,000 francs each to the Geological Society of France, the French Association for the Advancement of Science, and to other similar bodies.

The Brazilian Government has decided to offer two prizes of $110,000 each to the discoverer of a bacillus of yellow fever and its precise characters, and to the investigator who shall determine the most efficacious means of dealing with the disease. The Medical Institute of Rio de Janeiro, the Pasteur Institute of Paris, and the Hygienic Institute of Berlin are conjointly to make the award. Dr Sanarelli is likely to be recipient of the first prize.

Dr H. H. Field's Concilium Bibliographicum is again showing promising activity. We have received a parcel of slips relating to the contents of *Natural Science.* Dr Field is, we are glad to say, restored to health.

A bronze statue was unveiled at Crevalcore, near Bologna, on Sept. 8, to Marcello Malpighi, the famous anatomist and microscopist. Dr Vallardi promises a volume "Malpighi e l'opera sua" as a memorial of the event.

The University of California, having $4,000,000 promised or received, has advertised for plans, the competition for which is international.

Science states that Peoria, Illinois, is to have a University on the death of Mr Washington Corrington of that city, who is now eighty-five years of age, and will leave a sum of over $1,000,000 for the new foundation.

Various donors have subscribed $100,000 to Hope College, Holland, Mich. The Laman Missouri Educational Association has received a gift of $10,000 from Mr D. A. Beamer.

The Indiana Academy of Science is now receiving State aid in the printing and publication of its *Proceedings.* It now holds much the same relation to the State that the National Academy of Sciences bears with respect to the Congress of the United States.

A bill for providing for a geological survey of the State of West Virginia was passed by the legislature last session. The commissioners will be the governor, treasurer, and president of the West Virginia University, the president of the State Board of Agriculture, and the director of the West Virginia Agricultural Experiment Station, who will serve without compensation, except out-of-pocket expenses. They will appoint a geologist of repute and such assistants as may be necessary. The survey is to examine the geological formation of the State, with especial reference to economics; soils and adaptability to particular crops; forests; physical features with reference to occupations, industrial development, and prosperity of the people; and to make geological and economic maps, and special reports on the geology and resources.

The American Association for the Advancement of Science will meet next year—its fiftieth anniversary—at Boston, under the presidency of Prof. F. W. Putnam. The Vice-Presidents for the sections are as follows:—Geology, H. L. Fairchild of Rochester; Zoology, A. S. Packard of Providence; Botany, W. G. Farlow of Cambridge; Anthropology, J. McKeen Cattell of New York City. Mr L. O. Howard of Washington was elected Permanent Secretary. A considerable number of papers have already been entered, a full list of which will be found in the *American Journal of Science* for September.

Apart from the President's Address, the addresses of the Presidents of the Section, especially interesting to the readers of this Journal, are those of Dr G. M. Dawson, Prof. Miall, Dr Keltie, Sir Wm. Turner, Prof. Michael Foster, and Dr Marshall Ward. Dr Dawson dealt with the Ancient Rocks of North America, tracing the history of the discovery, differentiation, and classification of the Palaeozoic formations. Prof. Miall protested that we study animals too much as dead things, and are content, many of us, to name and arrange them, according to our own notions of their likeness or unlikeness, and to record their distribution. Dr Keltie gave a sketch of recent progress in geography, and pointed out directions for further work. Sir William Turner's paper dealt with distinctive characters of human structure, and was largely concerned with the erect attitude. Prof. Michael Foster reviewed the progress of Physiology since the Association last met in Canada, in 1884, and Dr Marshall Ward gave a long and interesting paper on the economics of Fungi.

"THE GLOBE" of Toronto provided a good account of the proceedings of the Association, and illustrated it with amusing portraits and interesting information; one of the series of pictures gave the coats of arms of past presidents. The Honorary LL.D. of Toronto University was conferred on Lords Kelvin, Rayleigh and Lister, and on Sir John Evans; the D.C.L. of Trinity on Lords Kelvin and Lister, Sir John Evans, Sir Wm. Turner, and Sir George Scott Robertson. The following grants were made to Committees of Biology and Geology:—Seismological Observations, £75; Erratic Blocks, £5; Investigation of Coral Reefs, £40; Geological Photographs, £10; Age of rocks near Moreseat, £10; Pleistocene fauna and flora of Canada, £20; Table at Naples Zoological Station, £100; Table at Plymouth, £20; Index generum et specierum Animalium, £100; Biology of Ontario Lakes, £75; Oysters, £30; Climatology of Tropical Africa, £30; North-Western Tribes of Canada, £75; Glastonbury Lake Village, £37, 10s.; Ethnography, £25; Silchester excavations, £7, 10s.; Ethnology of Canada, £75; Torres Straits Expedition, £125; Changes of nerve cells, £100; Fertilization in Phaeophyceae, £15. The total amount granted was £1350. The total attendance numbered 1362.

WE understand that, after various delays, the fitting and arrangement of the new Paris Museum of Natural History are now making good progress. It is hoped that the public galleries will be ready for opening early next year.

WE hear from *Science* that plans have been submitted to the Department of Buildings, New York, for two additions to the American Museum of Natural History—one, a lecture hall at the north end of the Museum; the other, a six-story building attached to the west wing.

THE Report of the Trustees of the South African Museum, Cape Town, for 1896, received this month, records the re-organisation of the staff and the completion of the new buildings, to which we have previously referred. The Museum now has the services of Mr W. L. Sclater as director; Mr L. Peringuey as assistant-director, with special charge of the insects; Dr W. F. Purcell as keeper of land invertebrates; Dr G. S. Corstorphine as keeper of geology and mineralogy; and Dr J. D. F. Gilchrist as honorary keeper of marine invertebrates. During the year 1896 a special grant was expended upon the purchase of a series of large mammals for the collection, while an exchange with the La Plata Museum furnished an important series of South American mammals and birds. Large acquisitions of European rocks and fossils were also purchased for comparison with the South African specimens.

WE have received from Dr J. W. B. Gunning, Director of the Museum of the South African Republic, Pretoria, a list of acquisitions for the month of July 1897. The Zoological Department is being especially enriched with examples of the South African fauna.

THE trustees of the Albany Museum, Grahamston, have decided to erect a new and more commodious building. The necessary funds are already in hand, and the work is to be proceeded with at once. The plans have been prepared by Mr Viesebosse, architect of the Cape Town Museum. The new museum will be a two-storied building, 150 feet long by about 60 feet deep.

WE have received the first annual report of the Geological Survey of Cape Colony, under the direction of Prof. G. S. Corstorphine. The new department seems to be much hampered by a clamour for immediate economic results. We hope the Colony will not be too impatient, but realise that the purely scientific part of the survey must first be accomplished in more or less detail before the economic problems can be satisfactorily attacked.

According to the *American Naturalist*, the Academy of Natural Sciences of Philadelphia is trying to raise $50,000 to purchase the palaeontological collections of Professor Cope. Since the fund received from the sale of the collection is to go to the Academy for the foundation of a professorship of palaeontology, it would seem appropriate that the collections themselves should become the property of this society.

We have received the last part of the *Bulletin* of the Natural History Society of New Brunswick (No. XV.), containing a long review of the scientific work of Abraham Gesner, pioneer in the geology and mineralogy of Nova Scotia, by Dr G. F. Matthew. Dr Matthew also describes supposed evidence of a thysanurous insect from the early Palaeozoic rocks (Little River Group) of New Brunswick. The thirty-fifth annual report of the Society makes the gratifying announcement that the membership has considerably increased during the year. The library also increases rapidly, and H.M. Treasury has generously presented to it a complete set of the 'Challenger' Reports.

We have received from Mr C. A. Snazelle, the energetic honorary secretary of the Jersey Natural Science Association, a report of the second meeting of this new society, and the first programme for the winter's meetings. In addition to the meetings for general papers and lectures, there will be small sectional committees for various departments of detailed scientific work. We regret to learn that Natural Science is so little cultivated in Jersey that the total membership of the Society is still less than 50.

Mr William Bidgood, the Curator of the Museum of the Somersetshire Archaeological and Natural History Society, in Taunton Castle, has just issued the sixth edition of his Guide. The Museum is chiefly remarkable for its archaeological collection relating to Somersetshire, while among the geological specimens are the cave remains from Banwell, Bleadon, Sandford Hill, and Hutton.

The total number of visits of students to the reading-room at the British Museum during the year was 191,363, being 3,600 less than that of 1895, which again was lower than that of 1894 by 8,000. This we regard in a very favourable light, as it shows that the wise regulations of Sir Maunde Thompson, regarding a certain class of readers, have resulted in greater comfort for the more serious students. It also shows indirectly the value of Free Libraries.

The last number of the *Journal* of the Marine Biological Association of the United Kingdom (vol. v., No. 1, issued August 1897) contains the annual report of the Director and of the Council for 1896-97. Under the direction of Mr Allen the Plymouth station continues to flourish and increase in utility. The Association is also fortunate in retaining the services of Mr Holt, for the time being, as Honorary Naturalist. The Lords Commissioners of H.M. Treasury in granting the usual £1000 for the year 1897-98, have made it a condition that the Association will give all the assistance in its power to the Inspectors of Irish Fisheries in investigations which they desire to be made on the habits and migrations of the mackerel visiting the Irish coast. This important work has thus been begun, and the principal contribution to the new number of the *Journal* is Mr Allen's report on the present state of knowledge with regard to the habits and migrations of the mackerel (*Scomber scomber*). Most of the other papers also have an important economic bearing. The large laboratory in the Plymouth station has been provided with a new flat tank, eight feet by five feet and eight inches deep, in which Mr Garstang has been making the observations on crustacea to which we refer elsewhere. The sea-water supplied to the laboratory is still kept distinct from the general circulation in the show tanks, and is never returned to the laboratory tanks after it has passed through them. Experience shows that the theory of 'circulation,' as applied to aquaria, is illusory and in practice disastrous.

Science announces that the Zoological Expedition sent by Columbia University this summer to Alaska have lost all the results of their season's work by the wreck of the *City of Mexico*, in which they were returning. Fortunately all the members of the party were landed in safety. The Duke of the Abruzzi and his companions successfully ascended Mount St Elias on July 30, 31. The height of the mountain was ascertained to be 18,100 ft. Dr Sella was of the party and we may hope for excellent photographs as illustration to the report of the expedition.

We are indebted to Mr Duerden for an interesting account of this summer's work of Prof. W. K. Brooks' party of Baltimore students in studying the tropical life of Jamaica. As we have previously announced, the party this year was under the direction of Prof. J. E. Humphrey, and established a temporary laboratory in an hotel at Port Antonio, a locality in many respects more advantageous than Port Henderson, the former headquarters. The director, one of the most distinguished of the younger American botanists, collected and preserved a large amount of botanical material, giving special attention to the shell-perforating algae and to the embryology of certain flowering plants. We deeply regret to add that towards the end of the course of study he became ill and died almost immediately. Another botanist, Mr A. Fredholm, dried a large collection of Jamaica plants for the herbarium of the U.S. National Museum. Dr F. S. Conant continued his researches on the medusae, begun last year. The chief object of his investigation this season was the function of the sensory organs of the medusa, and material was prepared with especial reference to a study of the changes, under the influence of light and darkness, in the pigment of the retina of the eyes. Dr H. L. Clark also continued his previous researches on the echinoderms. He devoted special attention to the life-history of the interesting holothurian *Chirodota*. Mr Sudler returned to Port Henderson to dredge for the small crustacean *Lucifer*, which he could not find at Port Antonio. Mr Grave studied various ophiurans, and doubled the number of species recorded from Jamaica. The eggs of one species were artificially fertilised in the laboratory, and a complete series of the embryos from the single cell to the fifteen-day Pluteus stage was satisfactorily preserved for future examination. Mr E. N. Berger devoted his time chiefly to insects and arachnids, obtaining many embryonic stages, especially of a pseudoscorpion, probably *Obisium*. There were also junior students. Mr Duerden, as Curator of the Jamaica Institute, was invited to join the party ; he profited by the occasion in enriching the Museum collection, and in continuing his researches on the corals.

ERRATA

Page 88,	line 3.	*For* 'invariable'	*read*	'variable.'
,, 148,	,, 6.	,, 'Marlott'	,,	'Marlatt.'
,, 158,	,, 12.	,, 'collection'	,,	'collector.'

NATURAL SCIENCE

A Monthly Review of Scientific Progress

No. 69—Vol. XI—NOVEMBER 1897

NOTES AND COMMENTS

The Confirmation of Darwin's Theory of Coral Islands

During the past few years there has been a lull in the long controversy regarding the origin of the coral atolls of the Pacific. The arguments for and against Darwin's theory had been so often repeated that people were tired of deductive reasoning on a problem which might at any time be solved by a practical test. It has been admitted for many years that the only method of ending the controversy is to make a deep boring in an oceanic atoll. The first attempt to apply this method was made during the expedition of the United States cruiser, the *Tuscarora*, but the boring tools broke at a slight depth. In 1891 a committee was appointed by the British Association to arrange a plan for a new attempt. The Admiralty was willing to assist by help of a surveying ship, and the Government of New South Wales was ready to lend the boring equipment. A report was prepared, and it was recommended that Funafuti, one of the Ellice Islands, should be the site of the experiment. Accordingly a joint expedition was sent out by the Royal Society and the Australian Museum at Sydney, and was led by Prof. Sollas. Valuable natural history collections were made at Funafuti, and many interesting anthropological observations recorded; but the main object of the expedition was not accomplished, as the boring failed. The observations collected were claimed by Admiral Sir W. J. L. Wharton as opposed to Darwin's theory; whereas Mr Chas. Hedley of the Australian Museum maintained that the general survey of the island strongly supported it. With splendid perseverance the Australian authorities resolved to renew the attempt. Accordingly a new expedition was sent out under the command of Prof. T. Edgeworth David of Sydney. A telegram from Sydney on October 4 announced that all the difficulties had been successfully overcome, and that the boring had been carried to a depth of 557 ft., and was still in coral rock. A further telegram on October 12 announces that the depth of 643 ft. has been reached and the boring is still being carried through coral rock.

Prof. Edgeworth David may therefore be congratulated on having finally proved conclusively the truth of Darwin's brilliant theory. It may be objected that it is too soon to shout, as the cores have not yet been subjected to microscopic examination, and that one boring is not sufficient. But neither objection is worth much. Coral-reef rock is of very varying composition; the coral grows in hummocks separated by more or less narrow spaces, which are filled up by coral sand, broken shells, foraminifera, &c. Microscopic examination of fragments of limestone broken from coral reefs sometimes shows no trace of coral. Coral, moreover, is more readily decomposed than shell sand or foraminiferal limestones. Hence it will not be surprising if most of the slices cut from the deep core shew no trace of coral structure. But that will not prove that they are not to be regarded as reef rock. Nevertheless it is to be hoped that some parts of the limestone from the bottom of the core will be sufficiently well preserved to show the nature of its formation.

To the objection that one boring is not sufficient to prove a theory proposed for so many islands, it is only necessary to point out that Darwin himself insisted that his theory was not universal. He never intended it to apply to the coral reefs of the West Indies, the Red Sea or the Persian Gulf. What the Funafuti boring has proved is that the subsidence method is possible; and if one island in the Ellice Archipelago is sinking, it is probable that the other islands in that group are also doing so. No doubt some cases will be found in the Pacific of coral islands formed on banks left by denuded volcanoes. But the arguments which Darwin used to show the improbability of many of the atolls having been formed in this way are still valid; and they are strengthened by the demonstration in the only atoll yet tested, that the basis is a block of subsided reef limestone.

There is some zoological evidence in support of a former migration of land animals across the area now occupied by the coral sea; and naturalists are now at liberty to explain their distribution by the former existence of land in that area.

The persistence with which the Australian naturalists have persevered in their attempt to settle this controversy, and the skill with which Prof. Edgeworth David has overcome the mechanical difficulties, are worthy of the highest praise.

The Temperatures of Reptiles, Monotremes, and Marsupials

Several other interesting contributions have reached us this month from the Antipodes, and among the most valuable may be noted an account of some new experiments on the body-temperature of vertebrates by Mr Alexander Sutherland (*Proc. Roy. Soc. Victoria*, n. s., vol. ix., pp. 57-67, pl. vi.). For many years past there has been a tendency to diminish or ignore the distinction between the cold-blooded and the warm-blooded types of animal life. The new results, however, seem to confirm the idea that the distinction is a real one, though they show more clearly than ever that several gradations between the two types still survive in the existing world. Cold-blooded animals sometimes develop a capacity for heat-production in the action of their viscera. Mere digestion, for example, may increase the temperature of a snake from 2° to 4° C., while amatory emotion is known to have the same effect on snakes, lizards, and frogs. But in general this excess of warmth is not great, and it leaves the gap between the warm-blooded and the cold-blooded type quite evident.

Mr Sutherland's first two experiments were arranged to re-determine to what extent the temperature of a reptile varies with that of its surroundings. He placed some lizards in a tank of water, leaving only their noses uncovered, and then warmed the water at various rates of speed by means of one or more lamps. In each case he found the rise in temperature of the animal and of the water to correspond almost precisely. Other observations also point to the same conclusion, namely, that cold-blooded animals at rest take their temperature almost absolutely from their environment.

Mr Sutherland next records his experiments with the Monotreme Mammalia. Their low temperature has often been remarked upon. Baron Miklouho-Maclay once determined that of the duck-billed platypus (*Ornithorhynchus*) to be only 24·8° C., while the average temperature of forty-five specimens of the higher orders of the mammalia (excluding monotremes and marsupials) observed so long ago as 1825 by John Davy, was proved to be nearly 39° C.—a result subsequently confirmed by Max Fürbringer. The platypus is, indeed, almost a cold-blooded animal, and the echidna rises very little higher in the scale. Mr Sutherland finds the average temperature of *Echidna hystrix* to be 29·4° C., but it curiously varies :—"An echidna one cold morning was so low as 22°; another, brought in from the forest in a sack exposed to a fierce midday heat, registered so high as 36·6°. . . . This is an immense range for a mammal, and suggests a reptilian want of capacity for temperature regulation."

As the result of 126 observations, Mr Sutherland determines the average temperature of sixteen different species of marsupials to be 36° C., or three degrees below the average of the higher mammals. The marsupial most nearly approaching the monotremes in temperature proves to be the wombat (34·1° C.). Next comes the flying squirrel (*Petaurus*), with average 35·7° C. Eighty-three observations on the koala (*Phascolarctos*) show its average temperature to be 36·4° C. The range in any one individual at different times is generally proved to be very small. Mr Sutherland, however, observes:—"I have often known healthy specimens (of *Phascolarctos*) that had been for a while in the sun stand as high as 37·9°, while on a cool day, or in a very shady place, the same individuals would be only 35·3°, a range greater than we would find under the same circumstances in any of the higher mammals. The highest register I ever obtained for a thoroughly healthy koala was 38·4°, which is a degree and a half above the normal temperature of man; the lowest was 34·9, or nearly two degrees below man's normal. The former temperature would in man imply some constitutional derangement, a distinct case of feverishness; in the koala it denotes only that it has been out in the sun. The lower temperature, though common in the koala, is never met with in man except in rare pathological conditions."

It is clear, therefore, that there are grades of temperature, and that the mammals which are classed lowest on anatomical grounds are not only of the lowest temperature, but also of the greatest range, and they are likewise, of all mammals, those which are under the strongest and most direct influence of the temperature of the environment. Similar, though more incomplete, connecting links may also be noted in the case of birds.

In a very general way (concludes Mr Sutherland), and not forgetting numerous limitations and contradictions, it may be said that bodily activity depends on body temperatures, that creatures, such as insects and reptiles, are active only when warmed up from without, but become torpid with decreasing temperature. The type in which activity is generally habitual, maintains its own body temperature. This is seen in the mammals, but more still in the birds. But this warm-blooded active condition was produced by no sudden emergence; the monotremes and marsupials form a gentle gradation between the reptile and the carnivore or ungulate; while, so far as indications point, there is reason to believe that the lower birds still are reminiscent of a once existent chain of links which equally joined the cold-blooded lizards to those warmest-blooded of all creatures, the passeriform and fringilliform birds.

Entomology in Australia

Mr W. W. Froggatt has published (*Proc. Linn. Soc., N.S. Wales*, 1896, pp. 510-552) the second part of his work on the termites of Australia. A short account of the life-history and social economy of the insects, unfortunately written without reference to Grassi's recent researches, is followed by a revision of the genera, which will be valuable to naturalists elsewhere than in Australia. The Australian species which have come under observation are carefully described and figured.

In addition to such systematic work, Mr Froggatt turns his attention to economic problems, and we have received several of his entomological notes from the *Agricultural Gazette of New South Wales* for the current year. Some of the insects dealt with are native species, which have become destructive in gardens and orchards, while others are familiar British forms introduced with European plants.

THE CUCKOO

THE nesting habits of cuckoos are always interesting, and Mr A. J. Campbell has recorded in the *Victorian Naturalist* for August a list of the foster-parents of the Pallid Cuckoo (*Cuculus pallidus*, Lath.) of Australia. The observations show that the cuckoo almost always selects open nests, and that the Honey-eaters are the most favoured foster-parents. Mr Campbell mentions that the supposition that the cuckoo throws out the egg or eggs of the foster-parent to make room for its own has not been proved with regard to the Pallid Cuckoo; but he has found a broken egg of the bird underneath the nest of the White-shouldered Caterpillar-catcher (*Lalage tricolor*). At present there is no record of any cuckoo's egg having actually been taken from any Caterpillar-catcher's nest, though one of these birds has been observed feeding a young Pallid Cuckoo.

THE MENTAL DEVELOPMENT OF A CHILD

THE latest of the series of larger monographs published by the editors of the *Psychological Review* is a careful record by Mrs Moore of the behaviour of her boy during his first two years. Such records by psychologically-instructed observers are likely to be of much use for child-psychology; and this may be said of the present one, though the want of arrangement in the earlier part detracts seriously from its value. Great masses of facts are bewildering unless arranged on some principles. The writer begins very suggestively by distinguishing four periods—first, of seeing till the end of the fourth month; second, of feeling or fingering things till the seventh month; third, of examination or more systematic exploration; fourth, of speaking from the close of the first year. Unfortunately, this division does not reappear in the body of the work. The classification of movements is also confused. But having said this, we may be grateful to Mrs Moore for what she has given us; and it will be best here to select a few of the interesting points which occur in the course of the work. The first is a notice of certain epochs or periods in which the child made very rapid progress, on the thirty-fifth day, in the eleventh month, etc. (pp. 6, 7) — an important observation on which more data are desirable. Under the head of movements (Part I.) there is a useful record of voluntary movements, pointing to the growth of volition out of "repetition of an act which had originally caused either a cessation of discomfort or a sense of gratification" (p. 27). Many of the conclusions drawn are naturally rather corroborative of current psychology than pointing in any fresh direction. Parts II. and IV. are inferior in quality to the rest. Under Part II., which deals with Sensations, we have, on pp. 45 and 56, the note that even on the second day the eyes followed the movement of a bright pair of callipers—a date important on its bearing on the theory of visual space-perception. On page 66 we have the less important denial of an unborn ability of localising sounds. On pp. 80, 81, there are some remarks on the localisation of pain, which is declared to follow that of "other" dismal sensations. On page 87 it is denied that attention, at any rate in its involuntary form, comes late : the child gazed at a patch of light continuously on the thirtieth day (p. 46). Part IV. deals with language, giving careful tables of the sounds used and the principal substitutions of sounds for one another, as well as full vocabularies of the child at the close of the second year. The order at which the different parts of speech begin to appear is noted, as well as their numerical importance. One point which is emphasised more than once (pp. 123 and 97) is that the child's first names do not refer to indefinite or vaguely conceived individuals, but that the child does not understand the necessity of a name for each separate thing, and his words stand for what is interesting to him in his experience. As against any idea that general concepts arise from the fusion of individual precepts—this is however what would generally be understood. Mrs Moore's work will be a useful repertory of facts, to which she has been careful to supply an index.

The International Geological Congress

The Seventh Session of the International Congress was held in St Petersburg last August with great success. The attractive programme offered by the Russian geologists, with the aid of their Government, brought together a large number—nearly a thousand—especially from Germany and Austria. Americans and French were well represented, largely by mining engineers anxious to study the rich ore deposits of the Ourals. Englishmen were no doubt diverted to the other side of the Atlantic by the meeting of the British Association, and were therefore proportionally few.

Of the excursions before the Congress, those to Finland and

Esthonia seem to have been more successful than that to the Ourals, owing to the difficulty of transporting the larger number in the latter case. Those who visited Finland had an opportunity of testing the classification of supposed Archaean rocks recently put forward by Mr J. J. Sederholm (see *Natural Science*, vol. x. 1897, p. 79). The general opinion seemed to be that the evidence was insufficient to warrant the ascription of an Archaean age to many of the rocks. Under the leadership of Baron de Geer the glacial phenomena of the district were studied, and the Åsar, with which many English geologists made their first acquaintance, gave rise to interesting discussions. The main object of the Esthonian excursion, under the direction of Akademiker Friedrich von Schmidt, was the examination of the Cambrian, Ordovician, and some of the Silurian rocks of the Province. The junction of Ordovician and Silurian was not over easy to follow under the conditions of the excursion, but the grand series of absolutely unaltered Cambrian with *Platysolenites*, *Olenellus*, *Obolus*, and *Dictyonema*, was a revelation to many. The party had also the advantage of the presence of A. von Mickwitz, whose palaeontological work in these Cambrian beds has been of the highest value.

At the Congress itself, if no practical results were arrived at, still many discussions on matters of general importance to geologists took place. The main problem put before the Congress was the classification and nomenclature of rocks, both stratified and igneous, but chiefly the former. Treatises by Messrs Frech, Bittner, Walther, and Loewinson-Lessing formed the basis of discussion. This resulted in the following resolutions: "The Congress is of opinion that the historical method of classification must be adhered to, though it should continually be made more natural. The council is to nominate a committee to study the principles of classification in this spirit." "The introduction of a new stratigraphic term into international nomenclature should be based on a clearly defined scientific necessity supported by peremptory reasons. The appellations applied to a terrane in a definite sense cannot be applied in any other sense. The date of publication is to decide the priority of stratigraphic names given to the same series of beds." "For the minor stratigraphical divisions, sufficiently characterised palaeontologically, in the case of the creation of new names, it is preferable to take as their basis the most important palaeontological characteristics. Geographical or other names should only be used for divisions of a certain importance containing many palaeontological horizons, or when the terrane cannot be characterised palaeontologically." "Names badly formed from an etymological point of view are to be corrected without excluding them from the domain of science." Certain proposals of minor importance were referred to the above-mentioned committee.

There is no doubt that with the advance of geological knowledge we suffer from a superabundance of names. But it is to be hoped that the promulgation of these rules will not call into existence an army of nomenclaturists, whose delight it will be to search antique literature for forgotten appellations, or to puzzle their brains with lexicons in the attempt to make all names pass the same etymological standard. We have enough of this sort of thing in systematic biology, and we don't want any more of it.

After the Congress about half the members took part in the various excursions that culminated in the Caucasus and the Crimea. The Carboniferous rocks of the Moscow neighbourhood, the Permian of Nijni-Novgorod, the Mesozoic and Cainozoic strata of the Volga and Donetz basins were introduced to the foreign geologists by such able students of them as S. Nikitin, A. Pavlov, W. Amalitzky, T. Tschernyschev, and N. Sokolov. The difficulties of transporting and personally conducting such large numbers were successfully overcome, but necessitated the compression of the passage of the Caucasus into far less time than was desirable or was originally intended. Side excursions to Elbruz, Grozny, Astrakhan, the Tsei and Devdorak glaciers, and other points of interest compensated for this in some measure, and usually proved to be the most enjoyable parts of the excursion. After ridding themselves of superfluous roubles in the bazaars of Tiflis, the visitors explored Baku, the city of petroleum, and then crossed the Black Sea to study the Jurassic, Cretaceous and igneous rocks of the Crimea under the leadership of A. de Lagorio, N. Andrussov, C. de Vogt, and others. F. Loewinson-Lessing, after his arduous Caucasus campaign, engaged in the no less difficult enterprise of transporting some forty persons, of both sexes, to Ararat. With the rough way made plain for them and the crooked straight, those who took part in this excursion declared that in beauty and interest it was worth all the rest put together. Its close was unfortunately saddened by the loss of Mr Stoeber, a lecturer on pharmacy at Vladikavkaz, who was helping Professor Lessing. Joining in an unsuccessful attempt to ascend Great Ararat, he was more rash than his companions, who subsequently found him frozen to death.

The pleasure of many of these excursions was seriously marred by the great numbers that availed themselves of the exceptional opportunity. Worse still, it appeared that many of the members were hardly geologists. An attempt will probably be made in future to restrict the membership of the International Geological Congress to recognised workers. There is no reason why such a body as this should have its dignity and usefulness marred by all the tag-rag-and-bobtail that choose to rush for railway passes and free champagne, and we shall warmly support any movement for the more sparing distribution of its privileges.

A Floating Scientific Station

An important proposal was laid before the International Geological Congress by Professor Andrussow. It was that a ship, fitted with scientific laboratories and apparatus, should constantly be maintained at sea by international contributions, and that geologists and biologists of all contributory nations should be allowed a place on board for carrying out observations. The importance of the study of the ocean-floor, and of all marine deposits now forming, will be denied by no geologist, but the opportunity as a rule is lacking. Hence geologists no less than biologists are interested in the maintenauce of such a floating scientific station. The difficulties in the way of the proposal are too obvious to need comment, but if there is a real desire to see it put into effect nothing need prove insuperable. Since the idea received the warm support of Dr John Murray, Professors von Zittel, Haeckel, Walther, Prinz, and other influential scientific men, there is no doubt but that we shall hear more of it, and we wish it all success.

The Black Sea

An excellent illustration of the geological value of thalassography was afforded by the Black Sea. On the steamer that conveyed the main body of geologists from Batoum to Odessa, dredging apparatus was provided and a small laboratory fitted up, enabling those who wished to verify for themselves the interesting account of this sea given by Professor Andrussov in the Livret Guide. The most striking peculiarity of the Black Sea is the absence of all life except bacterial at depths exceeding 100 fathoms. The cause of this may be put briefly thus. Into the deep and steep-sided Euxine basin there is poured, especially on the northern side, a vast amount of cold fresh water from the rivers. Thus there is started in the direction of the Bosphorus a surface stream,

> "Whose icy current and compulsive course
> Ne'er feels retiring ebb, but keeps due on
> To the Propontic and the Hellespont."

From the Bosphorus into the Black Sea a very slow under-current brings the warmer but salter and therefore heavier waters of the Aegean. These scarcely mingle at all with the surface waters, but sink to the bottom. The exchange takes place so slowly that, according to the calculations of Admiral Makarov, it requires at least 1700 years to renew the water of the lower strata, whereas the water of the upper 100 fathoms is renewed annually. Below 100 fathoms, therefore, the quantity of oxygen contained in the water diminishes rapidly, and while the cold brackish water of the surface and of the narrow shelving shore is unfavourable to the development of ordinary marine life, the deoxygenated water of the depth, despite its saltness and warmth, is absolutely fatal to all organisms other than bacteria.

The action of these deep-sea bacteria is truly remarkable. In other seas the rain of dead organisms from the surface plankton forms the food of other organisms living in the depths. But here the rain, after it has fallen through the upper 100 fathoms, finds no organisms to eat it. The microbes have it all to themselves. The albumen of the descending dead organisms putrefies under the influence of the bacteria; oxygen is taken from it to form carbon dioxide, and hydrogen sulphide is evolved. The carbon dioxide appears to help in the formation of the fine precipitate of carbonate of lime that is found in the depths. The hydrogen sulphide partly acts on the salts of iron in the water, forming iron sulphide, partly decomposes as it reaches the oxygen of the surface.

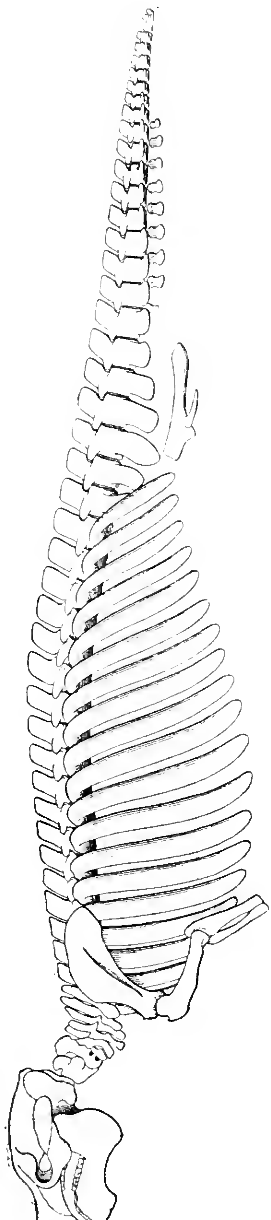
The gradual establishment of this peculiar state of things can be traced. Geological evidence shows that in Oligocene and Miocene times the Euxine and Caspian basins were connected, only being separated by the final upheavals of the Caucasus. Connection with the Aegean was due probably to the cutting down and lowering of a river-channel, of which the Bosphorus and Dardanelles are the remains. When this took place is uncertain. It cannot have been long ago, geologically speaking, because the shells of *Dreissensia* and other brackish-water molluscs are found lying on the bottom of the Black Sea at depths where neither they nor any animals can now exist. On the other hand the northern character of the Black Sea fauna, notably the presence of the common porpoise, suggests that the connection existed already during the Glacial Period.

Further interesting details, together with a description of the deposits now forming in the Black Sea, will be found in Professor Andrussov's guide. We have merely quoted enough to show the intimate relations between geology and oceanography, relations which we are glad to see officially recognised by the International Congress.

An Extinct Sea-Cow

Last month (p. 223) we briefly referred to our unfortunate lack of knowledge of the ancestors of the Sirenian mammals commonly known as sea-cows. In reference to this subject, we have now been favoured by Mr A. S. Woodward, of the British Museum, with the accompanying restored drawing of the skeleton of the best-known extinct Tertiary Sirenian, *Halitherium*, which he has recently had prepared for a forthcoming work on Vertebrate Palaeontology. This figure (p. 299), which is of about one-fifteenth the natural size, is mainly based upon a skeleton in the Museum of Mayence and upon the researches of Dr G. R. Lepsius, of Darmstadt.

Halitherium schinzi is found in the Lower Miocene sands of Hesse Darmstadt, and so dates back to the early part of the Tertiary period. It will be seen, nevertheless, that it only differs from



Restored Skeleton of Halitherium Schinzi,
An Extinct Sirenian from the Lower Miocene of Hesse-Darmstadt
(One-fifteenth natural size)

such a Sirenian as the existing manatee in but the smallest particulars. Some of its teeth seem to have been replaced by vertical successors. The vertebrae exhibit distinct traces of terminal epiphyses in young animals. The hind limb is represented not only by the rudimentary pelvis, but also by a trace of the femur. Otherwise, its skeletal parts are almost identical with those of its surviving relative. Since the Sirenians came into existence, indeed, very little change has taken place among them.

The Beaver in Norway

The beaver, which was once so common throughout northern Europe, still survives in Norway, and several notices of its occurrence in that country have been published in recent years. These notices, however, are more or less limited in their scope, and Prof. R. Collett, the eminent zoologist of Christiania, has lately done good service in investigating the whole subject with thoroughness. His results are published, with twelve beautiful photographs of the modern beaver-haunts, in the first article of the *Bergens Museums Aarbog* for 1897.

Trade in beaver skins was carried on in Norway in the Middle Ages, and the former wide distribution of the animal in the country is indicated by reference to it in many place-names. Now, however, its range is much restricted, and unless the laws for its preservation are rigidly enforced it will soon become quite extinct. It is chiefly confined to the Stifts of Christiania and Christiansand, and the largest colony is at present located in the middle and southern parts of the river Nisser (or Nid) in Nedenaes Amt. The banks of this river are for the most part covered with forests of *Pinus sylvestris*, and wherever these are interrupted by trees with deciduous leaves the beaver is to be found. Its chief food is the fresh bark of the last-mentioned trees, especially of *Populus tremula*; and for winter use small branches with the bark on are submerged in the water in front of the habitations. Bark that has been gnawed off is not collected for winter provender. Most trees are felled quite close to the water, and they are rarely brought from a distance of more than 300 metres.

Immediately after the break-up of the ice in spring, the beaver commences to search for food, and traces of it are sometimes seen in the snow. Work on the lodge or habitation is mainly done in the autumn, and almost exclusively at night. When the animal is seen in the daytime, it is as a rule only swimming in the water without any set task on hand.

The trees cut down by the beaver do not fall in any one definite direction, but lie pointing in every way. The current of the river is used for transport purposes whenever possible; but most of the lodges are situated in still water, and there the animal must itself convey the logs, holding them between its fore paws, while swimming solely by the hind legs. The construction of a lodge occupies at least two years, and it is repaired annually. It is usually elongated in shape, rarely round or conical, and it is always far distant from its nearest neighbour. Numerous burrows are made in the bank of the river near the lodge, but rarely in connection with it. They seem to be inhabited chiefly by young individuals, and they are the first refuges formed by the beaver at any spot where it has decided to settle down and build.

As to the inhabitants of a lodge, Prof. Collett thinks that only one pair with their latest young occupy each. The older litters either migrate or occupy the neighbouring burrows.

The Fishes of the North Atlantic

We have also been favoured by Prof. Collett with a copy of his handsome memoir on the fishes collected by the Prince of Monaco on his yacht the *Hirondelle* during the years 1885-1888, which is one of the most important contributions to Ichthyology of the last decade.* It is published in the sumptuous style with which the generosity of the Prince of Monaco has now made us familiar, and the illustrations are among the most exquisite figures of fishes we have yet seen.

The fishes obtained belong to ninety-five species, and are of great interest not only in their elucidation of the ordinary pelagic fauna of the North Atlantic, but also in the light they shed upon the geographical and bathymetrical distribution of a certain number of remarkable forms obtained from depths of no less than 2000 metres. Only six new species are determined, and only one new generic name is proposed, namely, *Halosauropsis* for *Halosaurus macrochir* of Günther—an emendation also made some time ago by Goode and Bean, who, however, proposed to term the genus *Aldrovandia.*

The classification of Günther is mainly adopted in this memoir, and the new specimens of the more important species are carefully described in detail. The value of the work is also greatly enhanced by the copious references to the literature of the subject, and the comparison of the results with those of previous authors. It would tend much towards the progress of systematic zoology if this laborious method were more generally followed by the authors of such reports. When recording new facts, it adds much to the toil of the work to incorporate them and correlate them precisely with existing knowledge; but the additional labour is well spent, and it converts dry catalogues, comprehensible only to a few narrow

* Poissons provenant des Campagnes du Yacht *l'Hirondelle* (1885-1888). By Robert Collett. 4to, pp. viii., 198, pl. vi. (Résultats des Campagnes Scientifiques accomplies sur son Yacht, par Albert 1er., Prince Souverain de Monaco, fasc. x., 1896.)

specialists, into readable narratives which any zoologist can appreciate.

The Study of Botany

At the meeting of the London County Council on October 12, the following resolution was adopted :—" That it be referred to the Parks Committee and to the Technical Education Board, to consider and report as to the practicability of laying out plots of ground in certain parks in such manner as will afford assistance to scholars of elementary and secondary schools in the study of practical botany." We hope the Committee and Board concerned will report favourably and that some of the resources of the London parks will be turned to account in the interest of the humble student of botany. If we consider only those who sit each year for the examination of the Science and Art Department, there must be a large number of students scattered through the metropolis, to whom the suggested arrangement would be very welcome. Examiners tell us that answers to the questions show knowledge derived mainly or entirely from books, and insist on the necessity of more thorough practical work. But the London student has not much opportunity for such. The Botanical Department of the British Museum in Cromwell Road, by means of carefully dried specimens, models and illustrations, supplies an excellent systematic review of the plant-world, and by skilfully prepared fruits and seeds, and wax models remarkable for their life-like accuracy and beauty, associated with clearly written labels and explanatory sketches, demonstrates to all who come to see such matters as the structure and mode of operation of insect-eating plants, or the means of distribution of fruits and seeds. But there is still much which can only be learnt from the living plant. We believe that Kew and the Royal Botanic Society's Gardens are the only ones to which the student can get access; the former by right (after 12 o'clock), the latter by courtesy only of the Council at certain times on certain days. The latter are useful for those living in a certain part of North London, while a journey to Kew means the underground railway or a happy day on the South - Western. And after all, life is short, and there are often other subjects which must be studied in addition to Botany. With even very little alteration or additional expense a park, such, for instance, as Battersea Park, might be made very helpful to an elementary student. There is a sheltered path by the lake where, in the summer, tree-ferns and cycads flourish, and in the same sub-tropical garden grow palm trees, most of them quite large enough to show a characteristic habit. But if we remember aright, many have no labels at all, and labels when present are very inadequate. A bare binomial name conveys little information; the addition of the group or order to which the plant belongs, and the country of which it is a native will help to crystallise certain ideas about that group or order which have been vaguely floating in the student's mind. A considerable sum of money must be spent every year in providing the chrysanthemum shows which are to be found in many of the parks each autumn. Some of the chrysanthemums are beautiful, others very ugly; but we are of opinion that the money would be better spent in keeping a variety of plants which need the shelter of a house all the year round. Such as, for instance, a few temperate orchids, examples of insectivorous plants and the like, which the ordinary student knows only from pictures. The resolution now before the County Council has reference probably to laying out beds to illustrate some of the more important families. This might be done at very little expense, and under proper management would be a great boon to the would-be botanist.

Botany of the Azores

The Eighth Annual Report of the Missouri Botanical Garden, issued by Prof. Trelease, the director, has just reached us. It is prepared in the same clear and elaborate style as its predecessors, and gives an exhaustive account of the work, educational as well as horticultural, achieved during the year. Excellent reproductions of photographs taken in the gardens are again an interesting feature. One gives us an idea of the destruction caused by the memorable tornado of May 27, 1896, which worked such havoc in St Louis. Though the grounds were not actually traversed by the cyclonic funnel, the violence of the wind was such that a number of the structures were either unroofed or totally wrecked, while some 450 trees, often of large size, where wholly or practically destroyed, and many of those left standing were seriously broken. Six days before the tornado "the most destructive hailstorm that has ever been experienced at the garden," also caused great damage. The scientific papers which occupy the greater part of the volume are extra-American in interest, and embody the results of some work by the director in the Azores. Mr J. Cardot supplies an account of the mosses found on the nine islands. These (excluding bog-mosses, of which there are eight) number eighty, fifty of which occur also in Madeira and the Canaries, sixty-one in Europe, especially the Mediterranean region, and in Algeria, and about forty in North America. One found in Flores has been known hitherto only from Madagascar and equally distant relationships are shown in the distribution of the genus *Sciaromium*, which has three species in the Azores, while nearly all its other representatives are to be found in New Zealand and temperate South America. Seven species are described for the first time. The fact that several of the most important islands do not as yet muster together more than fourteen species indicates that there is still much to be done before a satisfactory knowledge of the bryology of the islands is attained, and Mr Cardot believes that the number (eighty) could easily be doubled. The fungi are practically unstudied, and many species should be found in the moist wooded regions. The list of marine algae, "though unquestionably small, may doubtless be increased considerably by collections prosecuted through the entire year, while there is reason to expect a very large number of diatoms and desmids, as well as many representatives of other groups of fresh-water algae, whenever careful collections shall have been made." As regards flowering plants and ferns, though the list is probably nearly complete, there is still scope for much interesting work, such as a detailed local flora for the islands, with an analysis of the influences which favour the extended distribution of one species while restricting another to a very limited area. For such an enterprise the catalogue which Prof. Trelease appends to his botanical observations would form a useful basis. Most of the species, it is suggested, "may have been introduced by ordinary means, largely through human agency, since the discovery of the islands, for they are so precisely comparable with similarly named species from other parts of the world as to suggest the lapse of a very short time since their separation from the parent stock." Only a few are peculiar. Some of the latter are limited to one or other or several of the islands; but the native flora has clearly suffered so much through the inroads of man and domesticated animals, that it is impossible to say whether or not these local limitations have always existed. The greater number of the flowering plants are either wind-fertilised or adapted for pollination by but little-specialised insects, having as a rule open flowers, with readily accessible nectar or pollen. As regards relation between plants and animals, Prof. Trelease remarks that, as there are only seven species of wild mammals found in the islands, and nine endemic or commonly concerned with plant-dissemination elsewhere, and few birds capable of aiding in this work, except for aquatics or marsh plants, "it is scarcely to be expected that special dissemination adaptations would be found on the part of aboriginal plants, which presumably have been associated with these animals for a relatively short time, nor of recently introduced plants, unless the relations have been established and the modifications worked out before either plant or animal reached the Azores." Well developed burrs, for instance, are found only on recent introductions, and the great majority of species "either have no special modification adapting them to certain dissemination, but depend upon gravitation, the wind, or hygroscopic movements of their seed vessels, or else their adaptations are out of harmony with their surroundings."

I

The Fundamental Principles of Heredity

(*Concluded from p. 239*)

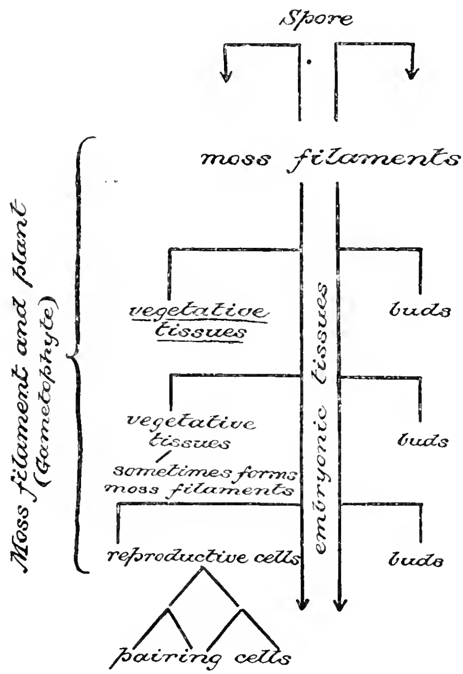
THE power of propagation of animals by small fragments is possessed very largely by Sponges, some Coelenterates, Starfishes and certain Flat worms; it is practically lost in the higher groups for several reasons, considerations of nutrition being most important. An Animal fragment can only obtain the nutritive matter for forming new cells by eating up, as it were, part of itself, until it has formed new organs for the prehension and digestion of food. To do this, the fragment must be always big enough to render this sacrifice possible; and, moreover, the tissue-cells must not be too specialised to adapt themselves to the altered conditions. Thus, the complex tissues of a human arm, accustomed to be served by a constant supply of blood current bearing in an abundance of food and oxygen and carrying off all waste materials, and to the guidance of a highly developed nervous system, can never adapt themselves to a life of isolation. In this respect Animals contrast markedly with Plants.

To study in the way we have applied to Animals the laws of reproduction and propagation in Plants, we must revert to those Protists whose life is essentially vegetal. These possess a coloured portion of protoplasm (green, yellow, or red), in which, under the stimulus of light, inorganic materials are combined to form the organic food on which (like animals) they feed. As these inorganic materials exist in solution, they can soak into the cell, which needs neither mouth nor stomach; and the cell can exist, grow, and multiply by division at the limit of growth, even while invested with a thin coating of the papery material, cellulose. If the cell start as a cylinder or ovoid, and the divisions are always in the same direction, at right angles to its length, the product (a colony of our first type) is an elongated filament, like those which form the green, slimy scum on our way-side ditches; if the divisions take place in two planes, the colony will form a plate or disk; if in three, a solid mass which is much more rare. When a period of increased vital activity ensues, brood-formation sets in; the brood-cells are at first naked, lacking the cellulose wall, and usually provided with swimming lashes. The brood-cells may in one and the same species[1]

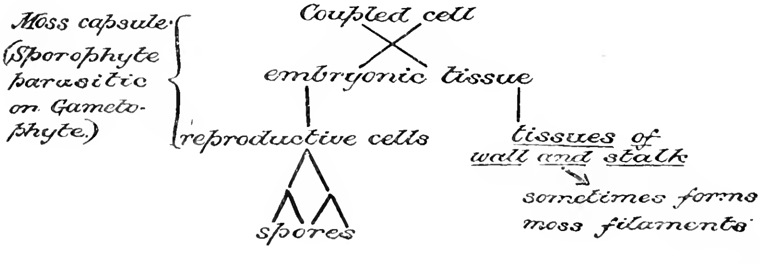
[1] The filamentous Alga *Ulothrix zonata*.

have very different fates. They may (1) settle down within the wall of the parent cell, and grow out into filaments, which finally rupture the parent cell-wall by their elongation; or (2) they may escape, and only after swimming about for a short time settle down to grow into filaments; or (3) they may pair first of all, and then

A



B



the coupled-cell, after a rest, makes a fresh start of life and growth and multiplication within the cell-wall. The life-cycle may be very complex. We may even find states in which the cell-walls of the filament gelatinise, and the cells themselves round off, the colony forming a very irregular mass.

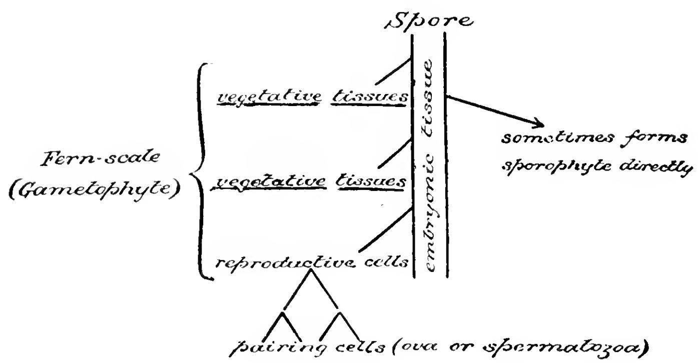
In some forms that are in other respects very primitive, we find a true differentiation that has advanced further than *Protospongia*, the lowest animal type we have selected as an illustration. *Volvox globator* is a beautiful green sphere the size of a small pin's head, found actively rolling over and over, as its name implies, in still waters fully exposed to the light. On microscopic examination it is seen to consist of many hundreds or even thousands of green cells imbedded in the surface of a spherical mass of gelatinous cellulose, and sending their active lashes into the water. Scattered among these are a few larger cells, which may be seen in all stages of segmentation; and as these grow and segment, they protrude into the cavity of the sphere, and finally rupture it and become free as new individuals. The ruptured sphere sinks to the bottom, and the colonial cells at its surface soon die, whether from the unfavourable conditions or no it is impossible to say. At the time for pairing it is only the few large cells that become or give birth to pairing-cells; the resulting coupled-cell segments to form a new colony. Here again we have a well-marked sterilisation of tissue-cells, and their characters are transmitted only through the reproductive cells, their collaterals. From our standpoint *Volvox* must rank as a lowly Metaphyte.

The majority of Metaphytes show a much higher differentiation and a power of colonial propagation far greater and more continuously exercised than in any Animals.

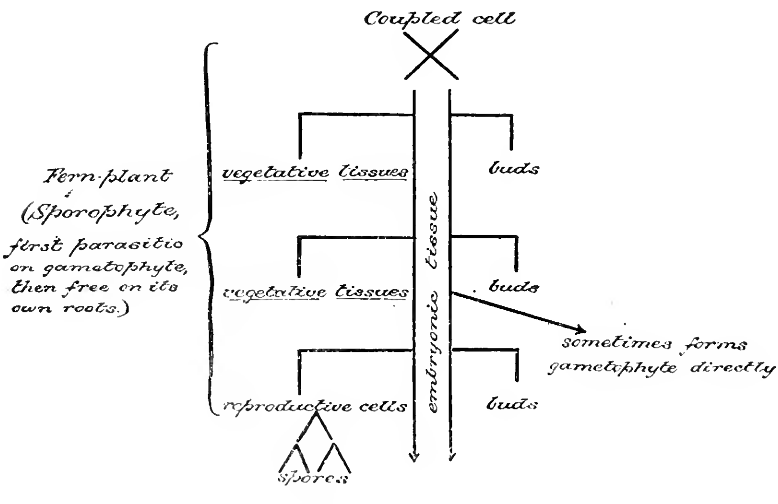
The first that we shall consider are the Scale and Leaf-mosses. As is well known, the little capsule or urn is full of a fine dust consisting of reproductive brood cells or 'spores.' These germinate and grow, as in Protophytes, into filaments consisting of elongated cells, some of which are green and run on the surface of the ground, while others penetrate it and serve as roots. But so little specialised are they that the reversal of a minute sod containing them will determine a change of their relative character and functions. On branches of these other cells are formed, which are short and thick. These divide, and by their colonial growth the proper leafy moss-plant is formed, but only the lower part for the time being assumes the condition of the moss tissues, the uppermost cells being colourless, nourished by the green cells of the stem and leaves, and assuming and retaining the functions of an embryonic tissue. This constitutes the 'growing point' characteristic of all the higher plants.

Ultimately, in the deeper parts of certain outgrowths, near the growing point, are formed reproductive-cells which give rise to pairing-cells, male or female, as the case may be. Fertilisation is internal, the male cell swimming up to the immovable female, and fusing with it in situ. The coupled-cell remains imbedded in the Moss-plant, and is nourished thereby as a parasite, and, undergoing segmentation, is converted into a colonial mass. The outer layer of this colony in the most primitive Scale-mosses is converted into a capsular wall, while the inner cells are reproductive cells, each of which forms a brood of four spores. In the Leaf-mosses the colonial body formed by the segmentation of the coupled-cell is much more modified, with increase of specialisation

A



B



and corresponding sterilisation; for its lower part is converted into a bristle-like stalk, and the wall and centre of the urn-shaped capsule are both composed of green tissue adapted for the formation of organic food materials.

Before we group these facts into a table we must notice the extraordinary powers of propagation of the Moss-plant: if cut up into fragments, almost any green cell, whether of the Moss-plant or the young urn, is capable of growing out into a green filament that will produce new leafy plants; and this is in addition to the propagative power by ordinary branching or budding of the embryonic tissues at the growing point. We will, according to custom, begin our table with the coupled-cell.

It will be seen here that there is no necessary colonial death (as in *Volvox*) of the leafy Moss-plant, though the older tissues of the stem and the leaves usually die down after the maturation of the parasitic capsule, and that the power of propagation possessed under certain circumstances by the green cells of the Moss-plant and urn make them possible direct ancestors of reproductive cells.

Still, in what we may regard as the normal cycle, the reproductive cells produce among their offspring collaterals as well as direct ancestral forms. The character of the cycle is noteworthy; two systems of colonial growth each beginning with a single cell are determined or closed by the production of brood mother-cells; and these systems contrast both in the characters of the colony and in the nature of the brood-cells. The colonial outcome of the spores is the filamentous growth and the leafy Moss-plant, and the brood-cells formed therefrom are the sexual pairing-cells; the colonial outcome of the coupled-cell is the capsule, and its brood-cells are the asexual spores. This is then an 'alternation of generations' in the sense of colonial or habitual terminology. Botanists have termed the contrasting colonial plants 'Sexual' and 'Asexual,' Gametophyte and Sporophyte, respectively from the character of the brood-cells which each produces in turn.

In the ascending scale of the Vegetable Kingdom we first meet in the Moss-plant with those tissue-cells which we term 'embryonic'; these must be defined as colonial cells nourished by the adult part of the colony, and having for their sole function growth or continued division at the limit of growth to form new cells and organs. Such cells are obviously not at all 'primitive,' as they are frequently called, but on the contrary are the essential outcome of high colonial differentiation. That the whole colony may exist in this condition in the early stages of development is only rendered possible in the case of the Moss-urn by its receiving nourishment as a parasite from the leafy plant.

The Fern is only comparable with the Moss by a complete detachment from preconceived ideas. Most readers know that the Fern sheds from the brown ridges or spots on the under side of its leaves a fine dust, whose particles are the spores. Each spore in germinating produces a cellular filament, which soon expands into a green plate, the equivalent of the leafy Moss-plant, or, better, the 'plant' of the Scale-moss or Liverwort, to which it bears a close resemblance. On this are borne sexual organs which produce sexual cells. The coupled-cell, as in the Moss, is at first parasitic on the scale, and develops into a Fern 'plant,' such as we know it with stem, roots, and leaves, and finally spores. The essential difference here is that in the Mosses the spore-forming plant is entirely parasitic and of limited growth, while in the Fern it becomes independent, and is of unlimited growth, being provided with organs of support and conduction as well as of nutrition. We may well say that the sterilisation (to use Bower's term) of part of the colony has led to so extended a power of colonial growth and branching, that the power of forming reproductive cells is in the end enormously increased. The propagative capacities of Ferns by buds from embryonic tissue are very great; those of fragments of the spore-bearing plant are slight; but the sex-bearing scale may be artificially propagated by being cut into small pieces, although its life is usually limited by the formation of the parasitic Fern-plant from the coupled-cell.

Ferns then show the same alternation between spore-bearing and sex-bearing generations as Mosses, but the order of relative conspicuousness and abundance of colonial growth is inverted. We have seen that in Mosses a vegetative transition by cell growth might take place from the spore-bearing generation to the other. In Ferns similar transitions are possible both ways, so as to cut out the stage of brood cell formation, which we regard as the critical reproductive stage.[1] Thus in many Film-ferns, instead of producing spores, the leaves grow out into scale-plates bearing sexual organs, while in the common Cretan fern, the scale produced from the spore grows out directly into the spore-bearing leafy Fern-plant instead of giving rise to sexual cells. In flowering plants the relations of the sex-bearing plant are much obscured, and it would lead us too far to explain them here. Suffice it to say that the 'plant' as we know it corresponds to the Fern-plant or moss-capsule: it is the Sporophyte, not the Gametophyte. The parasitism of the embryo formed from the coupled-cell is usually intense and prolonged.

A very remarkable character of Dicotyledons or Exogens is the continuation downwards from the growing point of a zone of embryonic tissue, the 'cambium,' which habitually by its growth and multiplication forms zones of wood on the inner side, and inner-bark (or bast) on the outer. This layer has, in cuttings, an especial tendency to form buds. But all the living cells retain a power of forming a similar tissue at or near an exposed surface; for instance, such a layer is formed a little within the surface of trees to produce the

[1] These transitions have been aptly termed "short circuitings" by Sir Edward Fry.

cork,—this is known as the cork-cambium. We are all of us familiar with the little brown scars on plums, &c., that have been slightly injured when green: these are due to the local development of a layer of embryonic tissue below the injured surface, and the formation of a thin protective layer of cork therefrom.

Colonial propagation in Flowering Plants may take place by the separation of buds (which form normally at the growing point), or by development of so-called adventitious buds from the embryonic cambium zone of the stem or roots. Such propagation by minute fragments as occurs in Mosses is unknown here; but larger fragments of leaves can frequently produce buds and ultimately plants. The cells within the cut surfaces produce an embryonic tissue, which gives rise both to a protective skin of cork and to adventitious buds.

The readiness to form cork and adventitious buds in this way varies extremely, and with this the power of leaf propagation. For the formation of cork is an indispensable protection against the opposite dangers of drying up on the one hand, and of the attacks of microbes and moulds on the other. Again most Begonias are readily propagated by pieces of leaf; but the bulbous varieties form a mass of embryonic tissue, well protected by cork, which remains for months or years before active buds are developed, so that they were long thought incapable of this mode of reproduction. Not only Begonias, but Gloxinias and other members of the showy order Gesneriaceae, the Peperomias with their massive speckled or veined foliage, and Chrysanthemums, are habitually multiplied in this way; and the list of possibilities in this direction is daily increasing.

On reviewing these facts we see that the law of collateral transmission applies to Plants as well as to Animals, but that they have much greater powers of colonial propagation, by the formation of embryonic tissue from already specialised colonial cells, and by the persistence of a portion of the colony (the growing point, and in Exogens the cambium layers) in the embryonic state. The fact that green cells can manufacture plant food in the light explains the greater vitality and propagative power of small Vegetable fragments as compared with those of Animals; and it is needless to assume any more recondite intrinsic differences. Even in this mode of propagation, the law of collateral transmission holds; for many of the cell-forms of plants, such as hairs, wood-cells, &c., are absolutely sterile, and consequently can never take part in the formation of an embryonic tissue capable of giving rise to a new plant.

Thus, throughout the Higher Kingdoms we find the problem of heredity rests on different data to those supplied by the Protista. In these lowly forms, where the law of direct transmission prevails, it is easy to admit that when a cell resolves itself into two new ones which exactly reproduce its original state, they should each possess its original qualities; even where the transmission is alternate, we may admit that the different conditions at the different stages of a genetic cycle modify the organisms produced. In the simplest case of collateral transmission, as presented by *Volvox globator*, the sterilised colonial cells so closely resemble more primitive independent forms in their behaviour and character, that we may well believe that they have inherited these from such forms, directly and unaltered, from some Protist ancestor, while the reproductive cells have become modified. But it is impossible to suggest such an explanation for the higher Animals and Plants, since a nerve-cell with its outgrowths many feet long, or a woody fibre which has expended all its living protoplasm in the building up of a firm wall, can only have been evolved as portions of a highly-specialised colonial organism.

The difficulty of explaining the mechanism of collateral transmission in Metazoa and Metaphytes by the direct transmission in Protista has been the origin of the recent lively discussions on heredity. To biologists saturated with the implicit conviction that only direct cellular transmission was alone possible, some mysterious agency, that should be contained in the reproductive cells, and be handed down by them in their direct cellular descent, was an essential assumption; and this agency is supplied by Weismann in his Germ-Plasm Theory. The reader will do well to bear in mind that it has been presented to the world in successive editions; each has been greeted as final by the disciples, who have made light of the objections raised thereto, though on every occasion such objections induced the Master to recast the theory in his next work. Our presentment of the theory upheld in the "Germ Plasm; A Theory of Heredity," published in London in 1893, may therefore, for aught anyone can tell, become obsolete very shortly, owing to the author's "having (to use his own phrase) in the meantime gained a deeper insight."

Weismann conceives that in the nucleus of what we have termed 'reproductive' (and also, in part, 'embryonic') cells is a mixed plasm, the 'germ-plasm,' composed of certain entities, the 'determinants' for the several organs of the colony; that when the cell divides at the limit of growth into two similar cells, the germ-plasm and the several determinants divide in the same way, so that the determinants are the same in each of the daughter-cells as they were in the parent. But in those divisions which give rise to specialised cells the germ-plasm divides as a whole, in such a way that the determinants are only distributed between the daughter-cells, some to one, some to another: we may say that there is distribution or repartition, not the true division of the several determinants. Similarly, the determinants each contain a group of minor entities the biophors, and in the ultimate divisions of the cells of an organ these biophors are distributed between the cells; and the proper biophors in each cell constrain it to play its specific part in the organism.

Those cells which constitute the direct line of descent between the reproductive cells of one generation and those of another are formed by true divisions of the germ-plasm, with all its determinants. But we are met by the facts of propagation by fragments composed only of tissue-cells in Animal, and still more in Plants, where specialised tissue-cells revert to an embryonic condition, or rather beget embryonic cells with a complete germ-plasm. To explain this difficulty, we must suppose that in these cases a portion of complete germ-plasm has passed at their formation into such tissue-cells, and that it has remained dormant until the stimulus of separation from the colonial organism has revived its vitality. Again in the four-celled stage of the segmented embryo of various widely distinct Animals (even in the sixteen-celled stage of some Medusae) it is possible to isolate a single cell, which then develops into a complete embryo, though had it remained associated with its fellows it would have formed only a definite part of the embryo. Here again we find the assumption of the existence of dormant determinants, which become active only in the separated cell, adduced by Weismann to save the theory. This assumption is also used to explain alternation of generations, where the Moss-plant and Moss-urn, or the Fern-scale and Fern-plant alternate; their germ-plasm must contain two sets of determinants, one for the first, the other for the second generation, alternating in sleep and waking like the printer and the hatter in Box and Cox. We are reminded of the complex epicycles required to render the universe workable on Ptolemy's geocentric hypothesis, and the Spanish king's comment thereon. "Had I been consulted at the creation, I could have simplified matters."

So far indeed, this might be held as a formal or fictive hypothesis to explain the mechanism of heredity on the basis of Special Creation—each organism being created fully equipped with its own proper germ-plasm, determinants, biophors, and all. But no; Weismann is a firm believer in the theory of common descent, and, as we have seen, he and his school profess to be the only true Darwinians; and we come to his Theory of Variations.[1]

The germ-plasm with its contained determinants, as it lies in the reproductive cells of the body, is subject to nutritive changes, and consequently to constant slight variations which apparently are not correlated with anything else whatever. The haphazard variations, of the determinants induce corresponding, and

[1] This essay was written nearly two years ago. Since then Weismann has enlarged his theory by the hypothesis of germinal selection. Without going into this we may note that it makes no difference to the present argument.

therefore haphazard, variations of the organism; and the Almighty Natural Selection now steps in, weeds out the unfittest, and so induces the endless variety of form and function in the Organic Realm. This has been irreverently termed the 'toss-up' or 'dice-box' theory of variation. It is hard to see how variations in feeding or starving hypothetical determinants can have ever ended in the development of a vertebrate eye, or in the exquisitely co-operating organs that render possible the parasitism of the offspring on the viviparous mother: it would be difficult if we had limitless aeons of biological time at our disposal, instead of the paltry million of centuries conceded as an outside limit by Lord Kelvin, even when multiplied by 4000, as Perry and Poulton suggest. We have all heard of the German astronomer who was reading Lucretius, and said to himself as noontide approached, "So if the atoms had been flying about for all time, cold potato, oil, vinegar, garlic, and salt might have combined to form a salad." "Yes, dear," said his wife, who had come in unperceived to call him to dinner, "but not as good as you shall have with your beef."

It must be admitted that marvellous ingenuity is shown in giving explanations on this theory to cases where they are not needed; we may cite the limitations of propagation by small fragments of Animals or Plants, and the variations in the power of leaf propagation in the latter, which are so readily explicable without the germ-plasm hypothesis. On this hypothesis, however, we are asked to overlook the plain and obvious questions of nutrition, cork-formation, and bud-formation, and to concentrate our ideas on the presence of more or less dormant germ-plasm in the tissue-cells. We may well note here that among "Inductive Fallacies" Bain cites the error of assigning more causes than a phenomenon needs. "It is involved in the very idea of cause that the effect is in exact accordance with the cause; hence the proof that more causes were operative than the effect needs defeats itself."[1]

But the cardinal defect in the theory is its objective baselessness. It professes to be founded on the microscopic study of the changes in the nucleus in cell-division; but there we find nothing to justify the assumption of two modes of nuclear division in the embryo, the one dividing the determinants, and the other only distributing them between the daughter-cells. To justify such a theory there should at least be some such basis in fact, as indeed there is for the author's 'id' theory of the relations of 'amphigonic' inheritance (from two parents),[2] which does not

[1] "Logic," by Alexander Bain. Part II., Induction, ed. 2, 1873, p. 395.

[2] To avoid complication and the undue lengthening of this essay we have been obliged to omit the consideration of the effect of double parentage in the higher organisms that reproduce sexually. But it is obvious that of itself it must tend to efface and not to accentuate the variations from the average standard of the race.

come within the purview of the present article. As it is, the theory falls under the ever-trenchant blade of Occam's razor, "Entia non sunt multiplicanda praeter necessitatem."

The antagonistic school, of Herbert Spencer, regard Living Beings as characterised by their continuous adjustment of internal relations to external conditions, and cannot see à priori grounds for regarding the reproductive cells as especially lacking in this power of adaptation. They regard instinct as only explicable as habit transmitted and relatively fixed by constant transmission from one generation to the next, and are disinclined to admit (even as a formal hypothesis) any scheme that leaves all such considerations on one side. They therefore are compelled to refer variations in the offspring to the adaptive reaction of the parent to the environment, and hold that there must be some mechanism of transmission other than that of direct cellular inheritance, by which the reproductive cells hand down to their differentiated cell-offspring the characters of the corresponding cells in the parent organism as a whole.

Charles Darwin felt this need so keenly (in a way largely ignored by those who style themselves his only true disciples) that he formulated his elaborate provisional hypothesis of Pangenesis to supply the mechanism that he postulated. He supposed that every cell in the body gave forth minute buds or 'gemmules' which circulated in the blood, and were carried by its current to the reproductive cells where they were stored up, and that in the development of the embryo they induced the formation of cells like those from which they were given off. Galton tried the crucial experiment of transfusing blood from one breed of rabbits to another, and found that this had no effect on the purity of the offspring. This not only shattered the theory of Pangenesis, but settled in the negative every conceivable theory of hereditary transmission based on the conveyance of formed material particles or of chemical substances from the other parts of the colonial organism to the reproductive cells.[1]

The second theory is that of Herbert Spencer, of 'biological units,' of definite form and relation, which by their polarity tend to complete the organism. I shall describe that development of it recently put forward with great skill and ingenuity by Wilhelm Haacke under the title of the "Gemmaria theory."[2] He holds that all living plasma is composed of minute units, the 'gemmae,' grouped together in aggregates, the 'gemmaria,' both being of definite form and size, in virtue of which they tend to assume certain relations of

[1] "Life and Habit," Lond. 1878; and "Unconscious Memory" (Lond., 1890). The latter work contains a translation of Hering's paper. "A Theory of Development and Heredity," by Henry B. Orr (London and New York, 1893), is written essentially from this point of view.

[2] See "Gestaltung und Vererbung," Leipzig, 1893, and "Schöpfung der Tierwelt." Both these works are written in a German style of exceptional charm, ease, and vivacity.

equilibrium in the cells and in the whole organism. Owing to this being a labile equilibrium, any disturbance due to an altered condition of the environment will alter the 'set' of the gemmaria and change the conditions of their equilibrium. It is as the result of their relation to the organism at large that the gemmaria of the reproductive cells R of an organism A are compelled to reproduce the likeness of A; consequently when the continuance of altered surroundings alters A to A′, the gemmaria of the reproductive cells will get a 'set' changing them to R′, which will reproduce the altered organism A′. Now, as a formal hypothesis, this serves to give a very pretty provisional explanation of many phenomena of organic life; but we have no sufficient microscopic evidence in its favour, and, to me at least, much that speaks against it. We know too little of the physical relations of cell-life to be able to accept, even provisionally, a theory based mainly on geometrical and mechanical conceptions.

The most satisfactory explanation, perhaps, is that put forward by Hering and Samuel Butler,[1] the latter of whom has written with singular freshness and an ingenuity which compensates for the author's avowed lack of biological knowledge. This theory has indeed a tentative character, and lacks symmetrical completeness, but is the more welcome as not aiming at the impossible. A whole series of phenomena in organic beings are correlated under the term of memory, conscious and unconscious, patent and latent. Our memory is conscious, when we say a lesson or remember a birthday; unconscious, when we let our fingers play of themselves a piece of music of which we could not write down a note; patent, when we remember to call at a friend's house; latent, during the interval while the servant is waiting at the open door, until the sight of the familiar stick in the hall recalls the owner's name which would not recur to our consciousness. Of the order of unconscious memory, latent till the arrival of the appropriate stimulus, is all the co-operative growth and work of the organism, including its development from the reproductive cells. Concerning the modus operandi we know nothing: the phenomenon may be due, as Hering suggests, to molecular vibrations, which must be at least as distinct from ordinary physical disturbances as Röntgen's rays are from ordinary light, or it may be correlated, as we ourselves are inclined to think, with complex chemical changes in an intricate but orderly succession. For the present at least the problem of heredity can only be elucidated by the light of mental and not material processes.

Queen's College, Cork. MARCUS HARTOG.

[1] "On Memory as a Universal Function of Organised Matter" (Vienna, 1870, ex. S. Butler in "Unconscious Memory," p. 97).

II

Reproductive Divergence: A Factor in Evolution?

IN the September number of *Natural Science* (p. 181) Mr H. M. Vernon propounds a new theory, called Reproductive Divergence, which, he says, is essentially different from Romanes' Physiological Selection. Inasmuch as both Reproductive Divergence and Physiological Selection are a process, not a cause, are based on the occurrence of the same kind of variation among the individuals of a species, and have the same end in view, the differences between the two principles do not appear to me to be of great importance; the premise of Reproductive Divergence is, however, more general, and the way in which the principle is demonstrated is certainly independent. Reproductive Divergence (like Physiological Selection) is brought forward to show that under certain propositions given differences between the individuals of a species inhabiting the same locality and presumed to stand under the same external influences will develop into specific differences solely by means of Reproductive Divergence. What we have to understand by specific differences is quite clear in this case: it is that kind of difference which we find to exist between two morphologically very closely allied forms which, though existing together in the same locality, are entirely independent of one another, the two forms (1) breeding true, the one never producing an individual that belongs to the other, and (2) never fusing into one form, in spite of their not being mutually absolutely sterile. In another place[1] I have referred at some length to Physiological Selection, and endeavoured to show that this principle does not hold good in so far as the outcome of Physiological selection, as propounded by Romanes, is, at best, dimorphism, not specific distinctness; and I came to the conclusion that forms deviating from each other in the way just mentioned cannot be evolved without the aid of some kind of local separation.[2] It does not seem to me that Mr Vernon's arguments in support of Reproductive Divergence as a factor in the evolution of specific distinctness are any more valid than those which were adduced by Romanes in favour of Physiological Selection. The occurrence of such a

[1] "Novitates Zoologicae," 1896, p. 426 ff.

[2] Local races are now generally termed subspecies; their high significance, especially in questions of general Biology, will doubtless be recognised in time also by those systematists who still persist in ignoring subspecies.

correlation between morphological characters and fertility of the specimens of a species as the theory requires cannot be denied. It is quite conceivable that, for instance, in insects the copulatory organs of one or the other species vary correlatively with the size of the individuals in a similar way as the horns on the head and thorax of Dynastid beetles, or the mandibles of stag-beetles do; such a variation in the copulatory organs of the males and a corresponding variation in the females would necessarily have the result, that copulation between specimens of different size could be less easily effected, and would be less effective than copulation between individuals of the same size. Hence the premise of the theory, or 'the first part,' which 'can only be verified by experiment,' must readily be accepted, and would be admissible even if there were as yet no facts observed which proved that the required variation actually occurs. Mr Vernon promises (p. 185) to make further experiments in this direction, for which biologists will surely be very thankful.

The second part of the theory, or the statement, that, if the above premise is given, a species will necessarily develop into two or more varieties or even fresh species, is 'demonstrated mathematically.' Let us examine this mathematical demonstration. Mr Vernon divides the 1800 specimens of a hypothetical species, which is assumed to vary in size from 64 to 73 inches, into three sets of males and females, of 300 specimens each, the sets being designated as S, M, and L, and s, m, and l respectively; then, if it is further assumed, that on an average of the 300 S, 100 S will copulate with s, another 100 with m, and the third 100 with l, and so also in the case of M and L, the total number of offspring will be—the total number of individuals is accepted to be constant in each generation—

I. 100 Ss, 200 Sm, 300 Mm, 200 Ml, 100 Ll.

If now "the comparative fertility of the various sized individuals is slightly changed," so that for instance 100 specimens copulating with individuals of the same size will give birth to 120 offspring, 100 specimens copulating with individuals of slightly different size will produce 95 offspring, and 100 individuals copulating with specimens of considerably different size will give birth to 80 offspring of either sex, the total number of offspring will be distributed as follows:—

II. 120 Ss, 190 Sm, 280 Mm, 190 Ml, 120 Ll.

By a comparison of II. with I. Mr Vernon comes to the two conclusions (*a*) that the limits of variation, which originally were 64 and 73, will be altered to 62·5 and 74·5; and (*b*) that the individuals of intermediate size will, in succeeding generations, decrease in number, while the individuals of small and large size will increase.

To show that inference (*a*) is correct, Mr Vernon argues as follows:—(1) Variety S varied originally from 64 to 67 inches, the mean being 65·5, and L from 70 to 73, the mean being 71·5; (2) let us then suppose that by the principle of Reproductive Divergence the average of S were reduced to 64, [the specimens varying now from 62·5 to 65·5], and that of L increased to 73, [the individuals varying from 71·5 to 74·5]; (3) then "it follows that these groups S and L would (approximately) contain individuals varying between 62·5 to 65·5 inches, and 71·5 to 74·5 inches respectively." But surely this inference (3) is merely a re-statement of assumption (2)!

And as to the conclusion (*b*) that the intermediate individuals will disappear, it has apparently escaped Mr Vernon that the figures given under II. are nothing else but a re-statement of the proposition that 100 pairs of equal size give birth to 120 offspring (etc.); the result of the chance-breeding is quite different. We must divide the original 900 individuals into five sets, and then compare these five sets with the five sets of II., thus:—

I. 180, 180, 180, 180, 180 = 900.
II. 120, 190, 280, 190, 120 = 900.

It is not for me to point out under which new conditions the range of variation would be widened and the species be split up into varieties. Under those propositions upon which Mr Vernon bases his mathematical demonstration, the mean of S will not decrease, and that of L will not increase, but the smallest and largest specimens will very soon disappear altogether, and the species become monomorphic, as a mathematical consideration of the chance-breeding in succeeding generations will show. If we start with 300 S, 300 M, and 300 L, the number of small, medium-sized, and large individuals in the first generation of offspring will depend on the size of the offspring of each pair; the offspring of a pair may be the same in size as the parents, or may be smaller or larger. It is sufficient to consider two of the infinite possibilities. (1) The 300 S produce on an average equal numbers of small, medium-sized, and large offspring, and so do the 300 M and 300 L. The result will be that the numbers of different-sized individuals will not be altered in succeeding generations, and the variation of the species will also remain the same. This is the usual result of chance-breeding, if no special factors come into play. (2) The 100 S which copulate with 100 s will produce 100 small specimens Ss, no medium-sized and large ones; the same applying as to M and L. This is what Mr Vernon assumes to take place. Though this assumption cannot be allowed to stand, as what is here assumed to be true is one of the characteristics of specific distinctness which the principle of Reproductive Divergence is propounded to explain, we will accept, for the sake of argument, that parents of the same size breed true. Then of the 120 Ss of the first generation 24 will copulate with small females and 24 each with the other four sets; hence there will be produced only 29 Ss. These 29 will have to copulate with nine sets, and so on. The same applies to Ll. Or to put it generally, if A is the number of individuals of each original set, a the number of original sets, x the surplus fertility, n the number of generations, then under the propositions adduced by Mr Vernon,

$$S_s^n = \left(\frac{100+x}{100}\right)^n \cdot \frac{A}{a\left((a-1)^2+1\right).\left((a-1)^3+1\right)\dots\left((a-1)^n+1\right)}.$$

In our case the numbers of $\dot{S}$ in the succeeding generations will, therefore, be—I. = 120; II. = 29; III. = 4; IV. = 0, 3.

That is to say, after the fourth generation, the largest and smallest specimens will be weeded out, and this result will not materially be altered, even if we assume that the largest and smallest individuals are mutually absolutely sterile. (Compare also Galton's regression towards the mean.)

Although Reproductive Divergence does not achieve what Mr Vernon claims for it, it is not altogether to be rejected under other premises than those accepted by Mr Vernon. There are certain species, for instance among Lepidoptera, which vary in the same locality in such a way, that there are two well-marked varieties which breed freely with one another, but produce comparatively few intergraduate specimens, the offspring belonging mostly to the one or to the other variety.[1] Here Reproductive Divergence may eventually have free play, and then necessarily will evolve incipient dimorphism into complete dimorphism, and in so far Reproductive Divergence might be called a factor in evolution.

KARL JORDAN.

ZOOLOGICAL MUSEUM, TRING.

[1] Standfuss, "Handbuch f. Schmetterlingssammler," 1895.—See also Giard, *Natural Science* I. p. 388 (1892); Romanes, *ibid.* p. 398.

III

A New Scheme of Geological Arrangement and Nomenclature

Part I

THE only scientific men whom Charon will carry across the Styx without a fee will probably be those who conform to orthodox shibboleths. For the rest, including most of the editors of and contributors to *Natural Science*, I mean the purveyors of audacious heresy, a heavy charge will no doubt be made. Meanwhile we ought to have our turn in this world and if we shock those who sit on velvet and dislike to have the picturesque dust of their cherished prejudices disturbed, they will remember that they will have their comfort when the Conservative old boatman leaves the flagrantly and impudently wicked in the mud.

In venturing to ventilate a fresh heresy I thought it needed such a preface.

The systematic arrangement of the various beds which compose the Earth's crust began, as is well known, with the Italian writers of the seventeenth century. It was Lehmann, however, who first really proposed a rational arrangement by separating the crystalline unstratified rocks, which he called Primitive, from the beds arranged in successive strata, which he called Secondary.

This classification with modifications including notably the introduction of a third class of beds called Transition, and answering largely to our present Cambrian and Siberian strata, continued in vogue until the beginning of the present century, and it was, in fact, the only possible arrangement so long as petrographical considerations were alone considered of importance in discriminating between different rocks, for it was early and easily seen that beds of very different chemical composition might graduate horizontally into each other, being therefore probably on the same horizon, while in other cases beds of the same chemical composition were clearly situated at different horizons.

The key to the problem which finally unlocked the geological riddle was the discovery, not made at one bound, but first applied systematically by William Smith, namely, that different geological horizons are marked by different species of fossil remains. This prime discovery has, of course, enabled us to map out the long series of stratified rocks in a continuous chain and to unerringly distinguish any particular horizon in discussion by its particular ear mark, namely, the special fauna and flora whose debris are found in it.

This I need not say is the corner stone of modern stratigraphical geology, and this, so far as we can see it, will remain. I have nothing to say about the key which is an absolutely indispensable one. What I propose to criticise, however, is the arrangement founded upon the facts thus ascertained, and I propose to attack it in two ways and on two grounds. If our object is to ascertain the past sequence of events in any particular spot on the earth's surface, we cannot do better than make a boring at the particular spot and describe in detail the successive beds lying upon one another or which we can fairly conclude once lay upon one another in that particular spot. This will undoubtedly, so far as that spot is concerned, give us the sequence of events. If the record be complete it will, of course, be a complete story. If some pages be torn out of the book it will, of course, be incomplete. This we may or we may not be able to infer. What is clear is that the column of different strata thus pierced represents not a general universal geological pedigree, but the geological pedigree of one particular spot only. This, of course, is universally admitted. No fact is more elementary than that two wells dug in the same parish may present us with very different columns of strata. Some thin out, some grow thicker, some disappear, and some make their appearance. The one cardinal fact, however, remains, that so long as we remain in the same "Zoological Province" so long will these beds when found together be found arranged in the same order.

This being so it is perfectly justifiable and perfectly logical so long as we remain in the same zoological province to collect all the beds occurring within that province and to arrange them in sequence, and having done so to make that sequence a test and touchstone by which the relative position of any particular bed in any particular section may be ascertained; always remembering, however, that we do not mean by this arrangement that the sequence of events in every spot within this area was precisely the same. In some cases certain stages were possibly absent as the record seems to infer, or a particular stage in one area may have become a complicated series in another. These are, however, matters of detail in which we have no necessity to guard ourselves since they are obvious and simple. What it is important to remember, and what has been made the subject of adverse comment by more than one distinguished palaeontologist, is the fact that the arrangement we have been considering only holds good and is alone useful when applied within one zoological province, and becomes utterly misleading when applied to any other. Let me explain what I mean. Zoologists have divided the land surfaces of the earth into several provinces marked by special faunas. Of these divisions that proposed by Mr Sclater long ago, and which was founded mainly on the distribution of birds, is the most popular. Each one of these provinces is marked by a special animal and vegetable facies. Similar provinces with a similar variation in their inhabitants occur also beneath the sea. Now it is clear that each of these life provinces has a special pedigree of its own. It may be that they all converge eventually upon some common and universal original, but the various lines of descent must have been separate from early geological times. How then is it possible or is it profitable to attempt to measure and test in any way whatever the geological record of one zoological province by that of another? We may eventually be able to say what was the character of the different zoological provinces contemporary with different geological horizons in our own country, but this kind of knowledge will profit us little. What we want to know is the pedigree of each zoological province by itself, and to keep that pedigree intact and separate and unsophisticated by any false correlations with the pedigrees of other provinces. When I am asked if a particular bed in India is Miocene or Pliocene, or a particular bed in New Zealand is Tertiary or Quaternary, etc., etc., I cannot attach any useful meaning to the question. If it mean that the bed is actually contemporary with, or that it is homotaxial with the beds so-called in Europe, the question is desperately hard to answer and of very little use when answered. If it means that a particular bed is the penultimate or the ante-penultimate geological stage in each area irrespective of actual equating of periods and dates, it may convey some meaning but it is a meaning crossed and sophisticated with danger and with doubt. What we want to do if we are to do justice to the great fact of the continuity of life, is to keep the two stories entirely apart, and to do so if possible by using a nomenclature which shall not be misleading.

If then we are to retain the present geological nomenclature and arrangement for the beds of the pan-Arctic or hol-Arctic region where a common fauna now prevails, we ought to apply an entirely different nomenclature to the arrangement of the beds in the Neotropical, the South American, the Indo-African, the Australian, the Indonesian, and the New Zealand provinces. We may then indulge in theories and systems of *homotaxis* without any danger, and we shall always be sure that we are measuring the horizon we are dealing with by a fixed and not by an unstable barometer. This is the first parable I wish to preach.

Secondly. In the last sentence I commenced with a very large "If" because I am afraid my heretical perversity carries me a good deal further than I have yet dared to admit.

Before Herbert Spencer held up his great lamp and bade us see in animal and vegetable life not a discrete collection of disintegrated units but a continuous unbroken chain, it was prudent and wise perhaps to be content with an arrangement of the stratified beds marked by no other law or rule than their mere order of superposition in any particular place. This will no longer content us. We want to know a great deal more than this. We want to know what was the route and road by which a particular fauna and flora came to occupy a particular zoological or botanical province, and what were the stages of its growth and development. For this supreme purpose we must go further afield than merely examine the column of strata existing in any particular place. A very cursory examination at once shows us that in every such column marine beds are intercalated with sub-aerial beds, and no ingenuity can possibly derive the fauna of the land from that of the sea and *vice versâ* by successive jumps and starts. They have absolutely nothing to do with each other, and if our purpose is not merely to calendar the revolutions of land and sea which have occurred in a particular place, but to trace out the history of the particular fauna occupying a particular province, either of the land or of the sea, and thus to track the continuous history of each of these divergent portions of the earth's surface along lines of continuity and growth, we must absolutely discard our present method of geological arrangement and nomenclature for a very different one. In the first place (and the change is so obvious that it has always seemed to me a paradox that it was not made long ago) we must absolutely separate the marine beds from the sub-aerial ones, put them into two entirely different columns and perhaps give them entirely different names. To apply the term Pliocene to the marine beds marked by the Norwich or the Weybourn Crag and to apply it also to the sub-aerial beds known as the Forest bed, is not an illuminating but a darkening process. These two sets of beds may have been contemporary but they have no other element in common, and it is utterly misleading to give them a common name because the marine and land debris are sometimes mixed as in the Norwich Crag just as Ammonites and Mammoths both may be mixed with striated boulders in the soft beds on the coast of Holderness. If we are to retain our present geological nomenclature for the pan-arctic region we must qualify each name by a distinctive epithet showing whether the bed we mean is in the marine or the sub-aerial series.

Again, we continually read in geological books of unconformability, an excellent term expressing a very patent fact when we are dealing with what I would call parochial geology, but a term which ought to have no place in a general scheme in which the progressive history of a life province is to be illustrated. If there is a hiatus and a gap here, it must be in the evidence and not in the actual story. It is the imperfection of the geological record and not the occurrence of a real unconformability in two successive stages of the history of a life-province which is the infirmity of our inquiry, and we disguise and distort the picture utterly by painting it as we do.

The ideal arrangement of our beds ought to correspond with their history in time as marked, not by their accidental sequence in any particular spot, but by the successive phases of their life contents. The land surface of the Palaearctic zoological province of to-day must have been in geographical contact and continuity with a corresponding land surface yesterday, and so on to the beginning of time. A succession of land bridges must have connected the present land surfaces with those of the primitive world by a perfectly unbroken chain, unless we postulate the complete periodic destruction of land faunas and their re-creation; and similarly with the marine faunas. This being so, unconformability and break ought to entirely disappear from our series. If we find signs that a marine submergence intervened between two stages of sub-aerial history of some locality and caused a breach between them, we must, nevertheless, conclude that these two stages were connected geographically at some point or points by a third one affording us the intermediate chapter, and it is along these bridges that we ought distinctly to travel. This seems to me to open up an entirely different mode of arranging and studying our beds to that usually current in text-books, one more consonant with modern zoological and palaeontological notions.

Our first step, as I have said, is to entirely separate the submarine and the sub-aerial beds from one another, and to range them in two series. Secondly, to arrange the beds, not according to their vertical distribution in one or more spots, but according to their continuity in regard to conditions of deposition. This will lead us along some unexpected and some not infertile lines of inquiry. Once we grasp this notion we shall cease to attach much, if any, value to the accepted generic terms of stratigraphical geology—the primary, secondary, tertiary beds, etc. In our own country, no doubt, the beds are separated by great gaps, represented fairly by their names, and if we are studying English geology only, the nomenclature and classification are justified; but these gaps cannot have existed everywhere, unless we are to reverse all our modern teaching. The story of biological development must have been quite continuous, and the book in which it was recorded must have contained a continuous series of pages, unseparated by gaps, dislocations, and unconformabilities, and this in the marine series just as much as in the sub-aerial series.

There are two initial lessons, then, I wish to press home before I venture on to more concrete lessons, namely:—1. That our present system of geological arrangement and nomenclature is only adapted to the purely local sequence of facts and history of the beds of Western Europe, and not to the whole world. 2. That we ought to arrange our beds on a universal and ideal system in two perfectly continuous series, one marine and one sub-aerial, marked not by their superposition in any one spot, but by the successive changes in the biological history of the world, and by no other test.

HENRY H. HOWORTH.

IV

The Authenticity of Plateau Man

THE antiquity of man and the locality of his birthplace are problems of perennial interest, to which Sir John Evans' address to the British Association has again directed general attention. Sir John Evan's emphatic dismissal of all the evidence yet advanced in favour of the existence of man earlier than the date of the Palaeolithic gravels will no doubt arouse controversy, especially as in the time at his disposal he could give only a passing reference to any single case, and not state the grounds of his distrust. As I have during the past year given careful attention to the asserted 'Eolithic' implements found in the high plateau gravels near Sevenoaks, with the result that I have had to abandon my first belief in their human origin, it may be of interest if I state the reasons for my change of opinion.

The implements come from the deposits described by my late friend Sir Joseph Prestwich in an important paper published by the Geological Society in 1891. The chipped flints themselves were described by Prestwich in a paper read to the Anthropological Institute a year later. Further descriptions have been given by their original discoverer, Mr Benjamin Harrison of Ightham, by Professor T. Rupert Jones, Mr Lewis Abbott, and other writers. Some geologists objected to the idea that the plateau flints had been worked by man at the time of their first description; but the drift of expressed opinion has been lately rather in their favour.

Early in 1896 I visited Ightham, and Mr Harrison kindly showed me the great collection of chipped plateau flints which he has formed during the past thirty or forty years with an indefatigable perseverance that has excited the admiration of every student of archaeology. As Mr Harrison's specimens lay side by side there appeared a remarkable recurrence of the same external form. This fact led me to accept the conclusion that the specimens had been shaped by man, but a more searching examination necessitated the abandonment of this opinion, as the flints themselves tell me quite a different story.

Let us first examine the flints and see what traces they show of the natural agencies that have acted upon them.

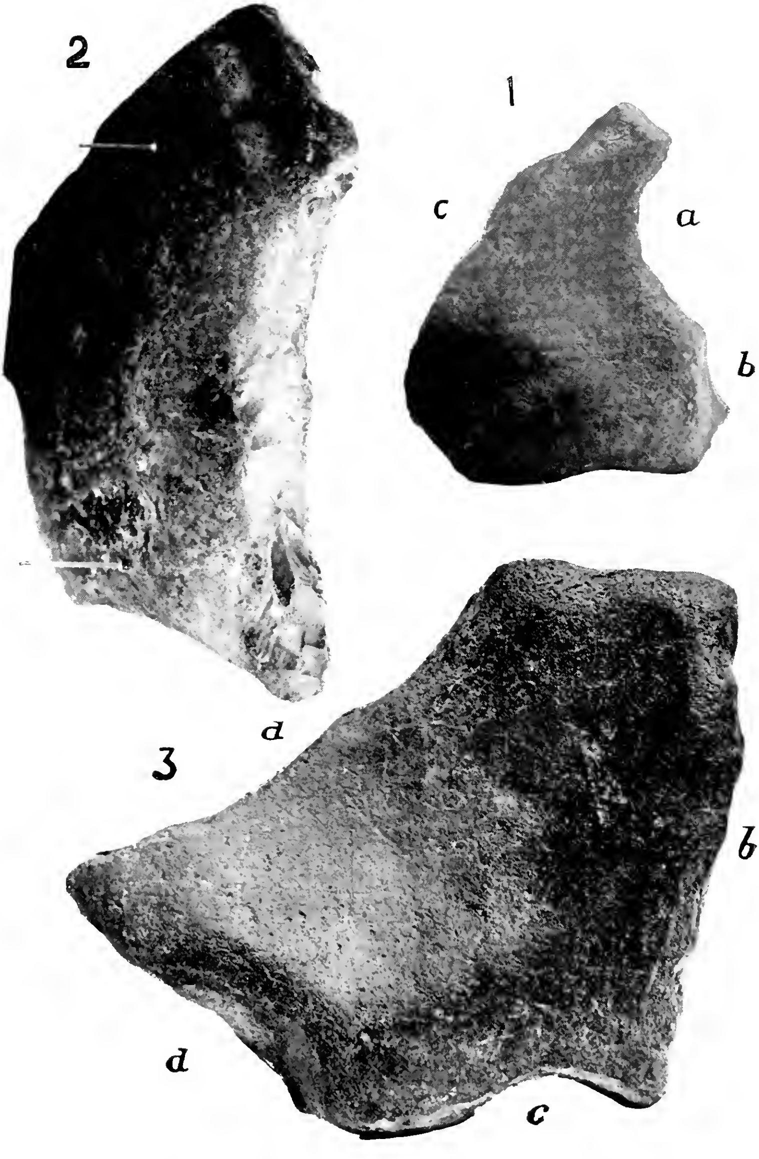
The flints occur as a gravel on the surface of the chalk plateau, or on the upper part of the escarpment face. That the flints have been derived from the chalk is unquestioned, and the process by which they were removed from it need not be considered. During the first stage of the life history of the independent flints they were split into slabs or tablets, most of which have one flat side, and one showing the original external surface. Some of the flint slabs were flat on both sides, and they are often six or eight inches in length. The formation of these flint slabs was probably due to extreme cold, as many of the fractured surfaces resemble those of frost-flakes. Then a siliceous encrustation was deposited over the flint. The next process—leaving the chipping of the edges out of consideration for the present—was the staining of the flints to a red or reddish brown colour. The staining was no doubt due to the action of ferruginous solutions. The iron may have been derived from beds of iron sand in which the flints were once embedded, as grains of dark ferruginous sand are found still adhering to the flints in the hollows of chipped surfaces. Subsequently to the staining, the flints were scratched by some glacial agent. The striae are very abundant, and closely resemble those produced by ordinary glacial action. They were no doubt caused by the movement of pointed flints across the flat surfaces of other flints under considerable pressure. The movement of frozen masses of gravel might produce these scratches as well as the flow of dirt-laden ice, so that it is perhaps unnecessary to assume the existence of true glaciers in Kent on the evidence of these scratched flints alone.

The next change in the flints was the deposition over them of a thin layer of silex, which covers most of the chipped surfaces, and often fills up the scratches. The silica occurs in two varieties, one brown and often very fibrous, and the other white. They may have been deposited at different dates. The material is a variety of chalcedony, sometimes having the characteristic botryoidal form of that mineral.

The nature of this siliceous encrustation is not yet completely understood, but it must have been deposited by some siliceous solution similar to that which has often re-cemented shattered flints. There is, for example, in the Devizes Museum a flint that has been broken into countless fragments, many of which are as fine as grains of sand; but they are all united by a chalcedonic infiltration into a mass sufficiently solid to bear a high polish. The occurrence *in situ* of flints which have been similarly crushed and re-cemented has been recorded by Englefield and Mantell.

One feature that renders this siliceous encrustation the more interesting is that it was sometimes deposited later than one set of glacial scratches and earlier than another set.

The last process which the plateau flints have undergone is a



SO-CALLED FLINT IMPLEMENTS FROM THE PLATEAU GRAVEL OF KENT.

certain amount of polishing by the action of blown sands. Sometimes the whole surface of the flint has been thus polished, but specimens often occur in which the action has been limited to one side. Many of the flints of the Thames gravels, as, for example, at Clapham, show the same feature. In the cases of these low level gravels, the polishing was probably effected at a time of drought, when the pebbles were exposed and the river sand was dry and loose.

We therefore see that these plateau flints have been subjected to six different processes, all of which are undoubtedly natural, and each of which has left clearly recognisable traces. Taken in order of date, the processes are:—1st, splitting of the original flints into slabs, probably by frost; 2nd, a deposition of a siliceous encrustation; 3rd, iron staining; 4th, scratching by some glacial agent; 5th, deposition of a second siliceous encrustation; 6th, polishing by blown sand.

This series forms a chronological table by which we can determine the relative dates of the various chippings of the flints which are affirmed to be the work of man. I think it is quite clear from the evidence of the flints that natural agencies are sufficient to account for every splinter and scratch that they exhibit.

The first fact that tells against the artificial chipping of the flints is that the chipping is of very different dates. The process must have been continued for a considerable length of time. It is not difficult to distinguish between the chippings of different periods. The earliest fractures are the largest, as might be expected, since the conditions were the most rigorous. As the climate became milder the forces that acted on the flints became feebler, and the chips removed were therefore smaller. On some specimens it is possible to detect chippings of three different periods. The first set were struck off before the flints were coloured red; the second set broke across the margins of the first, and sometimes exposed part of the uncoloured flint below. Then came an interval during which the glacial scratches were made. These scratches cut across the surfaces formed by the first two sets of chippings. The fractures of the third set, on the other hand, are never scratched, but have themselves cut across the striae. These latest chips were small, and usually more distinctly conchoidal than the others, and in many instances they present the appearance of small frost-flakes.

As definite illustrations of the different dates of the chippings, let us examine in detail four flints which were given to me by Mr Harrison as fairly good implements (Plate IX.).

The first is shewn in Fig. 1 of the Plate.[1] It was found by Mr Harrison at South Ash, and is numbered 4997. Its size is 6 cm.

[1] For the photographs I must express my indebtedness to Mr T. H. Powell of Denmark Hill.

long by 5 cm. wide. One surface is flat and smooth, while the other is convex and rough, as about one-fourth of it is part of the original surface of the flint. From the convex side two large flakes have been forced off, probably by frost, and no doubt at about the same time as the formation of the flat face. Both sides of the flint are scratched. After the scratching a thin coating of white silica was deposited over the two large-flaked surfaces, filling up some of the scratches. And after this a second set of scratches has cut across the siliceous encrustation.

The history of the chipping of this specimen is as follows :—The curve at *a* is the result of natural forces acting on the thin edges of a natural hollow of the flint : the hollow retains the original surface, except at the edge, where three or four small chips have been forced off at a period later than the flaking. The single chip at *b* has been forced off from the other side of the flint, and probably dates from about the same period as the chips of the *a* series.

The almost straight side of the specimen (*c*) was formed by chipping at a much earlier period, before the stone was stained red, but later than the large frost flakes.

This specimen therefore exhibits surfaces of four distinct dates—

1. The original surface of the flint.
2. The flat side and the two large frost flakes.
3. The chippings on the straight edge before the iron-staining.
4. The chippings at *a* and *b* subsequent to the iron-staining.

No. 4390. A flint from Branshatch (Fig. 2) 14 cm. long by 8 cm. wide. This specimen has the original rough surface of the flint on one face and a flat frost-flaked surface ; both have been encrusted by silex. The specimen is much chipped. The chips have been forced off by pressure mostly from the flat side, but at the larger end the chipping was done from both sides, forming a slight irregular ridge. The significant point about this specimen is that most of the chipping occurred subsequent to the dark staining of the flint.

No. 2711. A tablet of very dark-red flint from Rogersfield, near Ightham, measuring 15 cm. by 9·5 cm. This slab of flint was also chipped before the date of the staining, and also shews chipping by pressure from opposite sides. The chips are of different dates ; the first and second sets are scratched, and the third set are insignificant and irrelevant to the main chipping. The whole surface is encrusted by silica except where it chipped.

No. Pit VI. A triangular slab of flint (Fig. 3) probably flaked from a large block by frost. The original surface occurs on the convex side and extends over a third of the edge marked *a*. Ancient chippings from opposite sides occupy the edges *d*, and some of about the same date occur along *b*. These chips were all earlier than the iron-staining. The whole of the original surface is scratched, and the scratches sometimes extend over the edges and also cut across the surfaces left by the chips previously referred to. A dark brown, in parts fibrous, incrustation of silex thickly covers the original surface of this specimen. This was succeeded, after the glacial scratching, by a deposit of white silex filling up the hollows and scratches; the whole has been subsequently smoothed down and polished by blown sand. This specimen was dug from one of the pits in the Plateau gravel in 1896.

These four specimens illustrate the main process which the flints have undergone. They show that the chippings were not all formed at one period, a fact which it seems to me is quite inconsistent with the theory that they were artificially shaped by man. The objection seems especially convincing as, according to the advocates of that theory, all the shaping must have been done before the flints were imbedded in the gravel in which they now occur. If the flints were worked, used, and then thrown down again, we should expect to find them widely scattered over the surface as is the case with palaeolithic and neolithic implements. What possible agency could have picked them all off the surface and collected them together into this gravel bed? Further, we are told that the shaping and working of the flints by man "had taken place before the flint entered into the remarkable deposit which so altered the surface of the stone, and changed its colour into that characteristic dark-brown."[1] So according to the theory, the flints were first chipped into shape, and then carried into the plateau gravel. They were coloured subsequently, and the deposition of the siliceous encrustations, the glacial scratching, and the sand polishing all took place while the flints were in the gravel in which they now lie.

Another objection to the human working of these flints is the uselessness of the shapes into which they have been made. Flints often break naturally into a triangular form, and as the chipping has mainly acted on thin edges, abundant examples of pointed forms are found. Some of these resemble in outline the implements of later dates, but all the details of the flaking are different. Less importance is apparently placed on these triangular flints than on those with concave edges, which are supposed to have been used as flesh-scrapers. Some of the South Sea Islanders have, it is true, been observed scraping their limbs with stones; but we can hardly suppose that such vast numbers of these concave flints would have been required by the plateau folk for this purpose, especially as they would never wear out, and one would last for an indefinite time.

The chipping in some cases has not only been useless, but has even spoilt stones that might otherwise have been useful. Some of

[1] *Nat. Sci.*, April 1894, p. 259.

the specimens with sharp concave edges would have served a better purpose as scrapers if left as nature had shaped them. The irregular chippings on the edges of the natural curves has spoilt them as tools.

The vast number of the 'flint implements' from the plateau gravels is another difficulty in the supposition that they have been made by man. The 'implements' occur in an abundance described as 'marvellous' by their discoverers. We are told that two pits, dug in 1894 into a bed of gravel one foot in thickness, yielded thousands of artificial flakes and some hundreds of hollow-notched and horseshoe-shaped scrapers. The pits dug in 1896 in the same beds have yielded a similar profusion. Such results are indeed startling. Plateau man must have been a very prolific race, for his implements, almost all of one hollow-scraper type, far outnumber those of his palaeolithic successors.

In ordinary palaeolithic gravels the proportion of implements to pebbles is extremely small, and there is no difficulty in drawing a line between artificially and naturally shaped flints. But on the chalk plateau the stained flints are *all* more or less chipped. There are millions of flints on the plateaux, and it is therefore not surprising that a large number occur in which the shape resembles that of palaeolithic implements. But no distinctive line can be drawn between flints which are described as 'good implements' and others which are admittedly only naturally broken.

But if the chipping be not the work of man, what agency, it will be asked, could have produced it. Careful examination of the chipped flints soon suggests suspicious features. In the first place the chipping is limited to the edges of the slabs; there are no known instances in which the flint has been artificially flaked into the form of the weapon; the asserted human workmanship is limited to chipping of the edges of naturally-shaped flints.

Mr Harrison maintains that the chips were forced off by an agent which worked only from one face of the flint slab.[1] He regards this feature as an argument in favour of the artificial nature of the chipping. Why eolithic man should have worked only on one surface of the stone is not explained. But it is really rare to find an example that was chipped on one side only. Palaeolithic man certainly never allowed the utility of his tools to be limited by any such restriction.

The chipping was evidently due to some pressure which acted more or less at right angles to the flat surface of the flint slab. The pressure and crushing that take place during movements of frozen gravel would, it seems to me, be quite sufficient to account for all the chipping. Pebbles in the gravel would be pressed

[1] *Proc. Geol. Assoc.*, November 1893, vol. xiii. p. 162.

against the upper edges of the flint slabs and force off small flakes. Then if the flint slab be moved and inverted, renewed pressure would similarly crush the other edge, and thus account for the cases in which both edges have been chipped.

A fact which seems to me conclusive proof that the chipping was due to pressure by some yielding material from above is supplied by hollow flints. The decay of a sponge often leaves a hollow in the middle of a flint block; the edges of such hollows in plateau flints are chipped in precisely the same way as the edges. The artificial chipping of such edges by blows from another stone would be difficult, and the work would have been absolutely wasted to eolithic man. But the forcing of the finer constituents of the gravel across the hollows under the pressure from overlying material might easily have produced these crushed and apparently worked edges.

The view that the plateau flints show no sign of human workmanship I am glad to find supported by the high authority of Sir John Evans, who, in a letter of April 1896, said: "I see nothing upon them that is undoubtedly the result of human work or use; on the contrary, the rolling and wearing of the edges seem to me more probably caused by natural agencies—I see nothing but the hand of nature upon them." After a careful study of specimens lately selected as the most convincing by Mr Harrison himself, I am in absolute agreement with this opinion. And as the chipping of the flints was apparently caused by the action of extreme cold upon the gravels, movements in which were produced by the action of ice, I propose for these shaped flints the name of 'Glacïoliths.'

WM. CUNNINGTON.

V

Evidence of the Antiquity of Man in East London, Cape Colony; with a Note on the Castor-Oil Plant.[1]

ABOUT the year 1857, in opening up a quarry on the left bank of the Quigney River, at its junction with the Buffalo, a shell mound was discovered, forming a rounded bluff roughly measuring 150 by 150 by 40 feet deep. The mound was clothed by 18 inches of made soil, masked by vegetable growth on the surface, and contained abundance of shells of recent mollusca (*Patella, Mytilus, Ostrea, Haliotis,* etc.), with bones of fish, birds, antelopes, hippopotami, and other mammalia, layers of ash, fragments of charcoal, and pieces of coarse pottery. No other implement of any kind was found, but burnt stones were very common; most of the deposit was removed to fill up a lagoon behind the East Training Wall of the Buffalo River.

The locality had remained unaltered since 1827, for in 1867 the writer accompanied the Rev. W. R. Thomson on a visit there, and heard him say that the spot was quite unchanged. The same trees and the same track remained, and but for the impediment caused by the construction of the West Training Wall of the river Mr Thomson would have undertaken to drive a bullock waggon along the same track as he had done in 1827. This track, it may be of interest to mention, ran in a straight line from where Mr A. Webb's house now stands, to the right bank of the Quigney at its mouth; then skirting the mound, it proceeded for one hundred yards along the Buffalo River towards the mouth, and from thence on the east bank it crossed the river diagonally to the ravine at the large quarry on the west bank.

So far as is known this kitchen-midden is the most recent trace of primitive man at East London, and yet must be in itself of vast antiquity.

Passing to the back of the new jail one sees a small excavation in the railway cutting, which in 1887 was covered with castor-oil

[[1] The following personal observations by Mr Geo. R. M'Kay, relative to discoveries of ancient man in East London, have been forwarded to the editor by Dr Schönland. They formed part of a lecture delivered in 1887, and the editor is glad to put them on record.]

plants.[1] This was dug out for material to construct the railway embankment close by, and when undisturbed was clothed with the ordinary dense bush of the district. The section showed from above downwards 4 to 5 feet of stiff clay overlying a foot of rolled gravel, the gravel resting on decomposed rock. The gravel had all the appearance of shot of all sizes, from buckshot downwards. A little above the gravel a large number of flint implements were found, the bulk of which appeared to be "rubbers" possibly used for dressing skins or similar purposes. There were a few spear heads, some fragments of coarse pottery, and a few limpets.

This excavation is forty feet above the present level of the Quigney, and the gravel probably belongs to that river; the clay is assumed to be the accumulated wash from higher levels. The place appears to have been a workshop, for its position would afford the warmth of the morning sun with the coolness of shade in the afternoon.

On the north bank of the Buffalo, about twenty chains from the river, and just W. of the road to the Pontoon, is the residence of Mr Gately. The house and grounds stand on the rounded top of an isolated knoll, which is connected with East London *East* by a narrow neck of land. This neck is the watershed of two small water courses which unite at the S. of the knoll, and run S.W. into '1st creek.' On the top of the knoll there are from 2 to 3 feet of black mud, separated from the base rock by 1 to 2 feet of decomposed rock. The black mud is again seen to the E. of the road to the Pontoon. In this black mud on the knoll Mr Gately has found round pierced stones, stone flakes, spear heads, coarse pottery, and teeth and bones of hippopotamus. The deposition of this black mud was contemporaneous with the two water courses when they were at a level with the top of the knoll.

Fringing the whole of the south-eastern coast of South Africa there occurs in detached patches a peculiar wind-stratified calcareous sandstone. Cove Rock and Bats' Cave at East London, the 'bluff' at Natal, and the Sisters and Fountain rocks near the Fish and Kowie rivers, are conspicuous examples of the formation. At Cove Rock and Bats' Cave it abounds in fossils, especially at the latter. These consist of land and sea shells, mammalian bones, chiefly ruminants, and teeth of hippopotamus, with remains of fishes, apparently all of recent species. The rude and shifting nature of the stratification leaves no doubt that this is an aeolian formation, and comparable to that of the adjoining sandhills.

[1] An idea is prevalent that the castor-oil plant might be profitably grown as a producer of a cheap lubricant. My own experience is, that it flourishes only where rivers have cut deeply into their banks and exposed deeply seated soils, or where deep cuttings are made, or around the earth-holes of the porcupine and ant bear. It springs up on this new soil with amazing rapidity, and crowds out every other plant; but something ails it, as after a few years, whether cultivated or not, it dies down and disappears.

Out of this deposit I have myself taken three well-formed stone spear heads of the 'Moustier' type. They were at the level of low water, and in a position where they must have at one time been covered by 180 feet of the deposit. One of these is still in my possession, one is in the Albany Museum, and the third was given to a friend, who subsequently sent it to Sir John Lubbock. These aeolian deposits extend, so far as my own observations go, to a distance of certainly one mile into the ocean from the present shore. Between Sand Hill and Bats' Cave the low water platform has been cut back in many places for a width of over 200 yards by wave action. A visit to Cove Rock on a calm day will verify this if one stands on the larger of the two masses forming the rock-face on the south, for there one will see a similar platform, only at a much lower level, extending as far as the eye can follow it. The submerged reef off Nahoon Point is of this aeolian formation, and the sea has been observed to break on it upwards of a mile from the shore, while the Sisters and Fountain Rocks are really small isolated masses three-quarters of a mile from present low-water line. About one-third of the distance from the Sand Hill to Bats' Cave there is an isolated mass of this formation which becomes an island at high water; on the land-face of this mass there is a heap of shells embedded 12 feet above high-water mark. This is about 12 feet long by 3 feet thick, and contains, besides shells, fish bones and *splintered* bones of mammals; and although there are no traces of ashes or charred wood, I am satisfied myself as to its artificial deposition.

The facts concerning the aeolian deposit enumerated above leads us to the following conclusions. Since the three spear heads were lost we have to account for a depression of the land and advance of the shore-line for at least a mile, and from the average inclination of the bed of the Indian Ocean at this point, that means a land depression of over 50 feet. Then we have to allow for the gradual emergence of the land, which has enabled the sea to cut the rock back for a distance of a mile, up to its present level, which it has maintained long enough for the sea to cut a platform over 200 yards in width.

GEO. R. M'KAY.

EAST LONDON, W., SOUTH AFRICA.

VI

The Seed Production of Cut Flowers

IN the second number of the *Botanische Zeitung* for this year (Jan. 16th 1897, p. 17) Ludwig Jost brings to light a very curious fact in historical botany. He points out that at the close of 1896 H. Lindemuth has re-discovered a phenomenon, which has already twice before been described as new.

It is a well-known feature of many bulbous plants that their flowers are normally sterile, and that their reproduction takes place exclusively by the vegetative process of bulb formation. More than three hundred years ago (1577) Konrad Gesner noticed that if the flower stalks of these plants be separated from the bulb, the flowers will set their seed. This observation, however, fell into the general oblivion which overshadowed the whole of Gesner's work. In 1790—two hundred years later—Medicus re-discovered the fact, and wrote of it in his paper "Ueber Saamenansezen an abgeschnittenen Blüthenstengeln einiger Zwiebeln und Knollengewächse" (Römer and Usteri's *Magazin für die Botan.*, vol. xi., p. 6.). He was examining the tubers of *Stellarioides canalicuta* (? *Anthericum*), and in doing so cut off the inflorescence, which he stood up in a corner of the greenhouse for the gardeners to clear away. Returning to the house a few days later he saw that the flower still remained where he had left it, and that, moreover, it was still fresh and unwithered. This interested him, and he determined to see how long it would last thus "cut off from its bulb and standing in a dry position exposed to the sun heat."

Stellarioides had been grown and flowered in this greenhouse during the three previous years without once setting seed. "I was no little surprised, therefore," he says, "to find that in due course of time the older flowers of this inflorescence, which had been separated from its bulb, formed true seed capsules." "This really remarkable and quite unexpected result," he continues, "led me at once to other experiments. For twenty years past *Amarillis reginae L.* had bloomed in this greenhouse without once setting seed; as soon as the flowers drooped, it was seen that their ovaries and all they contained withered likewise." Medicus next proceeded to cut off an inflorescence, including three flowers, and to leave this standing in the greenhouse. After a time all three flowers formed seed capsules. The same phenomenon was seen in *Amaryllis formosissima.*

In discussing these observations Medicus writes:—"Those plants which have the property of reproducing themselves by roots, especially marked, are most unfortunate in setting seed, although no observer can deny the presence or completeness of the sexual organs. The true cause of this seems to be that these plants expend all their energy in increasing their roots and concentrate their nutritive activities on these parts, and so leave none over to contribute to the formation of seed. Annuals, or plants with a limited existence, on the contrary, for the most part, set seed, because they have little or no power of multiplying by their roots, which decay as soon as the seed is formed, and their allotted span of two to five months passed." Referring again to the complete sterility of *Amaryllis reginae*, under ordinary conditions, he adds that "scarcely, however, have we separated the inflorescence from its root, and laid it aside without moisture, than it forms large seed capsules, and clearly shows us that these would always be produced if the vigorous root formation did not rob them of all nourishment."

With the exception that we now regard bulbs and tubers as stem rather than root structures, these words have a very modern ring about them, and plainly show that what we now call correlation between the different organs of a plant was already then recognised by Medicus. One thing is very noticeable about his writings, and that is the charm of his literary style, an item which by no means graces too many of the scientific essays and memoirs of the present day.

Medicus' paper was written in May of 1790, and in the century which has elapsed since that time both Gesner's original observation and Medicus' re-discovery have been so completely forgotten that in 1896 Lindemuth published an account of the same phenomena without any idea that it had ever been noticed before. Thus, as Jost's paper in the *Bot. Zeit.* points out, we have here a fact which has three times been discovered as new, after having been twice completely forgotten. H. Lindemuth (*Berichte der deutsch. Bot. Gesell.*, pt. 7, vol. 14), after describing precisely similar facts to those which Medicus had already set down, using, however, *Lachenalia luteola* and *Lilium candidum* as his experimental objects, proceeds to recount some facts which go beyond those which his predecessors had seen. These he embodies in a second paper, contained in the same number of the *Berichte.* On 25th March he cut off forty inflorescences of *Lachenalia luteola*, and placed them in water. About three weeks later he noticed that the lower part of the stem which was under water was now curiously granulated. Here and there a granule had become larger than its neighbours, and was easily recognised as a bulbil. At first these granules are covered by the green epidermis of the flower-stalk, but as they gradually increase in size they burst through this as little white lumps. Microscopic examination shows that these bulbils are always exogenous in their origin. Inflorescences of hyacinth which were cut off close to the bulb and placed in water then had their flowers also removed, so that nothing but the peduncle remained. When examined nearly two months later, it was found that bulbils had developed close to the places where the flowers had been situated. In this case it seems that the food-stuff in the peduncle was first cut off from the bulb, and so travelled towards the flowers, but finding its passage blocked here also by the removal of the blooms, it expends itself in forming bulbils close to their remains. Finally, Lindemuth ends his paper with practical conclusions for the culture of bulbous plants drawn from these experimental data.

This brief English note has been written in order to call the attention of those into whose hands the *Bot. Zeit.* does not usually fall to the services which Medicus rendered to plant biology. To glance through the pages of Professor Sachs' History of Botany, the only knowledge that we can gain of this older observer is in a few, scant, depreciating references. Granted that the light of genius did not lead him into the right path in one section of botany (anatomy) we still should not allow the memory of an enthusiastic and careful observer in other departments of the same science to be altogether forgotten, or, worse still, to be alone remembered for the errors into which he fell. Most of us, even to-day, are not always in the right, and this should teach us to "render the deeds of mercy" towards the memory of others who, living at a less enlightened period, sometimes went astray with their fellows, and did not rise above their times, but who on other occasions saw things with an "inward light" which was denied to their contemporaries. It is no doubt the extraordinary work of Professor Sachs himself and his school which has quite placed in the shadow all older writings upon plant physiology.

RUDOLF BEER.

SOME NEW BOOKS

The Flora of North America

Synoptical Flora of North America. Vol. I., Part I., Fascicle II. By Asa Gray, continued and edited by Benjamin Lincoln Robinson. Imp. 8vo, pp. ix. to xv., 207 to 506. New York: American Book Company, June 10, 1897. Price, 11s.

Those of our readers who are interested in North American botany will remember that Dr Gray himself published the portion of this flora dealing with the Gamopetalous orders of Dicotyledons. The two parts which appeared in 1878 and 1884 were re-issued by the Smithsonian Institution in 1886. For some time before his death Dr Gray was engaged in monographing the earlier orders of the Polypetalae, and after his death the work was continued by Dr Sereno Watson and then by Dr Robinson. The first fascicle of the present part was issued in 1895 and contained an account of the orders beginning with Ranunculaceae and, following the system of Bentham and Hooker's Genera Plantarum, ending with Frankeniaceae. The second fascicle now before us carries the work on as far as Polygalaceae. It has been printed from Dr Gray's manuscript, continued and edited by Dr Robinson, with the collaboration of Professors Trelease, Coulter and Bailey. A third fascicle to include the Leguminosae is in preparation. The work forms a concise but complete and carefully elaborated account of the flowering plants of North America (north of Mexico). The descriptions, which are sufficiently full and clear, are in English; the synonymy and bibliography of genera and species are included, and the geographical range of species and varieties is indicated. Identification of the plants is facilitated by the introduction of generic and specific keys. We congratulate Dr Robinson and his colleagues on the portion already done and wish them a speedy and successful termination of the Flora.

Mollusca

Traité de Zoologie publié sous la direction de Raphael Blanchard. Fasc. xvi., Mollusques. Par P. Pelseneer. Pp. 187, figs. 8vo. Paris: Rueff et Cie., 1897.

This, which we understand is one of the first fascicules published of what promises to be a most important and valuable work, contains, in addition to the subject announced on the title, a two-page appendix on *Rhodope*. Otherwise it amounts in fact to a second edition of Pelseneer's "Introduction à l'étude des Mollusques," to which we called attention on its publication (*Natural Science*, iv., 1894, pp. 387-388).

It is, however, so much added to and revised, that it almost amounts to a new work. At the same time, we very greatly regret to see that the useful bibliographies formerly given at the end of each section have now been omitted. On the other hand, certain omissions to which we called attention have been supplied. *Chlamydoconcha* finds a place, and some of the more important fossil families are inserted in the systematic part, which otherwise remains unaltered. A revised phylogenetic tree is also added. When Dr Pelseneer comes to study the gastropoda as systematically as he has done his own favourite bivalves we are convinced he will abandon the classification of the Prosobranchiata that he at present takes from Bouvier (*Ann. Sci. Nat.*, ser. vii., vol. iii., 1887), which classification is founded solely on the nervous system. He will also add some notice of *Thyrophorella*.

The principal shortcoming, however, of the work as a whole is, we think, the scanty reference to the shell, which is after all an important feature of the mollusca, and in a treatise on zoology merits a place. In this respect the work is an exception to the generality of such productions where the animal is neglected. Curiously enough it is the Pelecypod shell that is the most curtly dismissed, and this in the light of Bernard's researches is the more to be regretted.

In the development of Pelecypods, too, we miss all reference to the second origin of gill filaments, by the splitting up of a previously existing lamella, as shown to occur in *Cyclas* and *Teredo* by Korschelt and Heider, and again in *Sciobcretia* by Bernard. Likewise our author appears to have overlooked the fact that the glochidial stage is not peculiar to the Unionidae, it having been found by Dall to occur in *Philobrya*. Other minor points for criticism are doubtless to be found by those who care to make diligent search for them, but the work all the same merits and will attain a high place in the estimation of those most competent to judge of it, and the praise we ventured to bestow on the first edition is yet more merited in the present one. It will be the author's fault if subsequent editions do not carry us far towards an ideal work on the subject.

It is only fair to add that a word of praise is but due to the printers and publishers for the excellent way in which they have carried out their share of the undertaking. The illustrations are, of course, those of the previous edition, enlivened in some cases by touches of colour to bring out the salient features they are intended to illustrate. There are two good indexes at the end of the part.

(BV)[2]

The Vivarium

The Vivarium, being a Practical Guide to the Construction, Arrangement, and Management of Vivaria. By Rev. Gregory C. Bateman. 8vo, pp. 424, with plates. London: L. Upcott Gill, 1897. Price, 7s. 6d.

For very many years Mr Bateman has kept living batrachians and reptiles as pets, and the beautifully got-up little volume now before us embodies the results of his experience. The work, however, is far more than a practical handbook. The author has added to his own personal observations several illustrated chapters, in which the more striking forms of batrachian and reptilian life are described in a popular manner. He thus appeals to a much wider circle of amateur naturalists than those who keep vivaria. We can thoroughly recommend the book to the general reader who desires a reliable, well-written, and non-technical account of the much neglected animals of which it treats. Our only complaint is that the illustrations are of very unequal merit, in many cases, indeed, far from accurate; and this is all the more to be regretted, since most of them are newly drawn, and might have been made admirable by a little more supervision of the artist.

Bibliography

The Theory of National and International Bibliography, with special reference to the introduction of system in the record of modern literature. By Frank Campbell. 8vo, pp. xvi. and 500. London: Library Bureau, 1896.

The main object of the present work is to demonstrate and enforce the responsibility of the government of each nation in the matter of cataloguing the literature published within its boundaries. As the author points out, it is impossible for any other body to do this work thoroughly and economically, because no other can bring pressure to bear upon the publishers. There is also this further reason for urging upon governments to discharge this obvious duty, that it is just in the department of State papers that the greatest confusion reigns and the labours of the bibliographer are most difficult. Witness Mr Campbell's imaginary, but most lifelike, conversation between a librarian and a reader in some large public library.

This main theme is treated in a series of papers which have for the most part been read before the Library Association and other bodies, and published in their journals, and in addition there are essays upon various collateral topics, such as "the influence and functions of the learned societies in regard to bibliography"—a chapter which we wish their councils would all "read, mark, learn and inwardly digest."

Mr Campbell's book is eminently suggestive, and his schemes if carried out would reduce confusion to something approaching order. With the form of his work we are not so satisfied; the plan he has adopted of reprinting essays leads to much repetition, and he often sins against his own 'theories of compilation;' but as he explains in the preface that illness prevented him from carrying out all his intentions, it would be ungenerous to dwell upon these defects, which are small in comparison with the solid value of the book.

W. E. H.

Flight and Flying Machines

The Aeronautical Annual for 1897. Edited by James Means. 8vo, pp. 178, pls. xviii. London: W. Wesley & Son, 1897. Price 5s.

The "Aeronautical Annual" for 1897 contains much that is valuable and interesting, since the contributors are nearly all of them men who are actively engaged in solving the great problem how flight may be made possible for men. First among these must be mentioned Mr Langley, who contributes an account of the experiments which, after many disappointments, ended in the manufacture of an aerodrome which actually rose in the air and continued rising and advancing for about one and a half minutes, after which it alighted rather than fell. As in the case of Mr Maxim's flying machine, screw-propellers driven by steam-power were employed. The action of the propellers is to drive the machine onward: the spreading wings have a slight upward slope, so that the force is resolved, and there is progress not only onward but upward. This aerodrome, as compared with Mr Maxim's, had the great advantage of being light, weighing, in fact, only about

25 lbs., so that experiments with it were easier and much less costly.

Mr Langley has shown, then, that flying machines can be made which will rise in the air, fly for a short time, and descend without mishap. But before the problem of flight is solved many difficulties must be got over, the difficulties of (1) launching without any elaborate apparatus; (2) maintaining equilibrium in gusty winds; (3) carrying sufficient fuel for long flights; (4) alighting safely even when the circumstances are not specially favourable. An oil engine is now being made to be affixed to the gliding machine. As an aeronaut will be on board, a distinct advance on Mr Langley's aerodrome, which had no living pilot, is contemplated.

The article on sailing flight by Mr Chanute reviews the various theories on the subject judiciously. There is no doubt, as he says, that in very many cases birds soar by the help of ascending currents of air. But it is probable that sometimes when there is no such current available, they nevertheless succeed in rising without a beat of their wings. Over level ground in Egypt, covered with green crops, where great heating or unequal heating of the surface seemed out of the question, so that there was nothing to start an upward current, the present writer has seen kites soaring with perfect ease.

Mr Chanute thinks that birds can soar by the help of a 'nearly uniform' horizontal breeze, but happily he does not, in contempt of dynamics, hold that an absolutely uniform horizontal breeze can lift them. He makes some good remarks on the question why some birds frequently soar, whereas others, and among them very good flyers, never do. He thinks that soaring depends on the arching of the wing, and it is quite possible that this may be the case. In the rapid pulsations of 'rowing' flight the front margin of the wing does the main work. In the stately gyrations of the soaring adjutant it is probable that the ample concavity is of more service. But far more cross-sections of wings than he gives are wanted before it is possible to accept this explanation, and in particular small birds should be well illustrated. It is remarkable that no bird so small as a thrush ever soars.

The subject of flight does not so often as formerly give rise to utterly wild theories. The *Annual* as a whole is remarkably free from wild writing. But the old tendency shows itself in Mr Huffaker's theory that a bird in soaring causes a down-current, which in turn starts an up-current, which supports him as he rises!

F. W. HEADLEY.

CONTEMPORARY PSYCHOLOGY

THE PSYCHOLOGY OF THE EMOTIONS. By Th. Ribot. 8vo, pp. xix+455. London: Walter Scott (Contemp. Sci. Ser.), 1897. Price, 6s.

THE NEW PSYCHOLOGY. By E. W. Scripture. 8vo, pp. xxiv+500. London: Walter Scott (Contemp. Sci. Ser.), 1897. Price, 6s.

THESE two volumes of the Contemporary Science Series exhibit Psychology in very different aspects. Prof. Ribot's work on the emotions is characterised by breadth of view, wide range of knowledge, admirable lucidity of presentation, bold and yet critical use of hypothesis, and a distinctly genetic motive. The subjects with which it deals are of necessity vague in outline and indefinite in limitation. The social and moral feelings, the religious and aesthetic sentiments, the emotions associated with intellectual endeavour—all these present us with such a variety of factors, such an interlacement of the threads of our mental life, so much overlap and so much that is dependent on individuality of character, that anything like mathematical exactness or precision of measurement is at present, and is likely long to remain, impossible.

On the other hand, Dr Scripture's *New Psychology* is characterised by limitation of field, a devotion to exactness of measurement, a love of mathematical and formulated presentment, an impatience of hypothesis, and a refusal to look beyond the formulated facts. The subjects with which it deals are carefully restricted to those which lend themselves to physical measurement. The new psychology—a remarkably unsatisfactory and somewhat arrogant title—deals with a comparatively small area of the field of mental endeavour, and one in which there is no luxuriant profusion of mental products. But it endeavours to deal with this small area with an exactness and precision which is in itself wholly praiseworthy. And if the results attained by the large expenditure of time, money, and energy in the well-equipped psychological laboratories across the Atlantic would seem at present scarcely commensurate with the cost, this will not, we trust, damp the ardour of enthusiasts like Dr Scripture. Psychology is a great subject of which we are only just beginning to realise the importance. There is plenty of scope both for the breadth of treatment we find in Prof. Ribot and for the patient experimentation of those whose work is described in the *New Psychology.*

A Blind Guide

Nature-Chat. By Edward A. Martin, F.G.S. 8vo, pp. 141. London: R. & A. Taylor, 1897. Price, 1s.

There are numbers of folk who derive much innocent enjoyment from natural history, and who have a harmless enthusiasm for 'Nature.' No one would wish to interfere with their amusement; but the matter assumes a different aspect when their trivial observations and inaccurate assertions are obtruded upon the public. This is the case in the latest addition to the *biblia abiblia* which are now so freely issued from the press—"Nature-Chat," by Mr Edward A. Martin. Mr Martin is a leading light of the Selborne Society; he guides the members in their summer rambles, and lectures to them on winter evenings. He has written a bibliography of White of Selborne, and seems to think he follows the methods of 'Gilbert,' as he affectionately styles him.

Disraeli said that the critics were men who had failed in literature; Mr Martin makes it clear that a teacher may be one who has never succeeded in learning. He has brought together in this volume a number of paragraphs, many of which, we believe, have appeared in a local magazine or newspaper; others are "parts of letters addressed to friends," who must, we think, be somewhat bored by the honour of Mr Martin's correspondence. We look in vain for a single addition to knowledge in this collection of trivialities.

The following paragraph may be quoted as exemplifying at once Mr Martin's scientific attainments and his literary style:—

"To be thoroughly acquainted with the beautiful grasses of Great Britain is to possess a knowledge of which I am ambitious. As I was collecting grasses on my walk, I pulled some concerning which I was at a loss to know whether they were of identical species, or whether they were distinct. Almost as the thought passed through my mind, my eyes lighted upon a grass of which the lower half of the blossom was fully expanded. This showed the features of one specimen, whilst the upper part, which evidently was yet to expand, showed the features of the other. Thus Nature answered her own problem which she had put to me." (P. 34.)

We are tempted to ask why Mr Martin has not acquired the "knowledge of which [he is] ambitious"? The number of common British grasses is not large; every manual contains their description. But it is only too clear that Mr Martin has not even a slight acquaintance with grasses, for he talks of 'the blossom' when he means the inflorescence, just as he speaks of a 'specimen' when he means a species. And what was the plant after all? He tells us that "Nature answered her own problem," but does not give us her reply. We feel inclined to imitate Nature in "putting a problem" to Mr Martin —What useful purpose can be served by printing paragraphs of this kind?

Mr Martin made the strikingly original observation that the flowers of the everlasting pea turn blue when fading. He then "watched the creeper closely, with the result that it has borne blossoms which were blue in the first place." This curious consequence of Mr Martin's vigilance leads him to say—"the seeds it will be well to collect": but he was counting his chickens too soon, for on p. 77 we read: "In a former letter I referred to the blue blossoms of the everlasting pea which had appeared in my garden. Neither of them have been succeeded by the usual pods of seeds, so that I shall not have the satisfaction of rearing seedlings from them, as I had anticipated." Can Mr Martin suppose that this kind of thing adds to our knowledge?

But the author rises to higher flights than these. "You know," he says, "the *yucca*, which is said [inaccurately, but this Mr Martin does not know] to flower but once in a hundred years. Then comes such a burst of brilliance that it requires a period equal to that which allows our orb to roll its ponderous body along its tremendous path around the sun, a hundred times or thereabouts, in order to recover its flowering energy. What an act of self-denial is this: what an act of self-immolation, in order that its duty may be fulfilled!" The only parallel to this reflection is one which occurred some years since in the catalogue of a picture exhibition in South London—"Scene in Ceylon: Elephants bathing. How much the elephants in the Zoo have given up for our sakes!"

The book is full of inanities and ineptitudes, and the literary style is in harmony with the subjects discussed. If a fly tumbles into the milk-jug, Mr Martin speaks of its 'unwelcome last sad bath.' Dead nettles are 'magnificent' and 'gorgeous.' Certain flowers smell like 'a glass of sherry'; two plants in the same paragraph 'rejoice in' their names. The author speaks of his 'legal brother,' as if he had another brother who was not legal, though we think he only means to say that the gentleman in question is a solicitor. And what are we to make of the following sentence :—" We are so matter-of-fact in our science—too matter-of-fact—to thoroughly appreciate the science which has become science, become what is known (*scio*, I know) by the labours of the great men of the past, who alone, perhaps, when discovering, were able fully to realise the poetry of their own discoveries." It is perhaps our own fault, but we are unable 'to thoroughly appreciate' either Mr Martin's facts or his style.

John Hunter

John Hunter: Man of Science and Surgeon (1728-1793). By Stephen Paget. With Introduction by Sir James Paget. 8vo, pp. 272. London: T. Fisher Unwin, 1897. Price, 3s. 6d.

Mr Stephen Paget's small volume on John Hunter inaugurates a new series, entitled "Masters of Medicine," edited by Dr Ernest Hart, and published by Mr Fisher Unwin. It is not a technical work appealing merely to the medical profession; it is a well-written and highly entertaining account of one of the greatest students of biology in the last century, full of interest to the general scientific reader. The volume does not contain much new matter; but Mr Paget has spared no pains to go to the original sources for information, and he has made good use of the letters and records preserved in the Royal College of Surgeons, London.

John Hunter, as a great pioneer in biology, is perhaps too much neglected by the present generation. His infusion of purely scientific methods into the profession of surgery led to results of such moment that his labours in other directions are apt to be overshadowed and forgotten. Those, however, who are familiar with such of his biological and geological writings as were recovered and edited by Sir Richard Owen in 1861, can estimate the old surgeon at his true worth; and it is of no little importance that his memory should be kept green in the minds of those who are now following in his footsteps. Mr Paget's delightful chapters can scarcely fail in this purpose, and we urge all who have not yet realised the extent and bearing of John Hunter's researches and the influence of his personality, to read the new biography at once. His favourite maxim was—"Don't think, try; be patient, be accurate." The story of his life will be found inspiriting by any plodding student.

Pengelly

A Memoir of William Pengelly, of Torquay, F.R.S., Geologist, with a Selection from his Correspondence. Edited by his daughter, Hester Pengelly. With a Summary of his Scientific Work, by the Rev. Professor Bonney. 8vo, pp. x. 341, portrait and 10 illustrations. London: John Murray, 1897. Price, 18s.

Not only the numerous personal friends of William Pengelly but also all who are interested in the progress of our knowledge of the antiquity of man and allied subjects, will be glad to learn that Miss Hester Pengelly has published an account of her father's life. The handsome volume of over 300 pages which we have just received, contains an ample selection from his correspondence arranged in chronological order, with paragraphs welding them into a connected story; and this is followed by an interesting chapter by Professor Bonney on the general character and value of his scientific work.

Pengelly's genial personality was so widely known and beloved, not only by men of science but by many devoted to other intellectual pursuits, that his biography will find no lack of enthusiastic readers. The story of his early life as a Cornish sailor; of his career as a schoolmaster at Torquay; of his growing fame as a geologist, his discoveries of fossil fish-remains in the Devonian rocks of Cornwall, and so forth; of his share in the exploration of Brixham cavern in 1858; and finally, of his great work in arranging and personally superintending the excavation of Kent's cavern—all this is told in an entertaining manner in his letters. The various little incidents in his career are recalled as we read, and those who knew the man himself will recognise his characteristic traits and modes of expression. We have only one criticism to offer, namely, that far too much of the correspondence is printed. A considerable proportion of it is very trivial and ephemeral, of no interest to anyone except his immediate family, for whose private view alone it was intended. Some other letters, we think, are inserted with rather questionable taste, as, for example, one in reference to a candidate's touting for the fellowship of the Royal Society on p. 282. Biographers ought to distinguish between strictly personal matters and those bearing upon the progress of a life's work. The latter alone are worthy of being preserved in a memorial volume.

Professor Bonney's summary of Pengelly's researches, occupying only thirty pages, is an admirable contribution. It is both concise and clear, and written in a style readily comprehensible to the general reader. The first section deals with the geologist's first serious undertaking, the investigation of the Tertiary deposits of Bovey Tracey; the next part is concerned with the examination of the caverns, in association with which the name of Pengelly will be longest remembered; and the third or final part includes a number of miscellaneous geological studies of the south-western district of England. We will not attempt to analyse this chapter; it must be read in its entirety to be appreciated.

Miss Pengelly's biography appropriately concludes with a list of her father's writings, more than 100 in number, and it is prefaced by a very successful copy of A. S. Cope's portrait of Pengelly, which was painted in 1882.

The Vertebrate Skeleton

The Vertebrate Skeleton. By Sidney H. Reynolds, M.A. 8vo, pp. xvi. 559, with 110 illustrations. Cambridge: University Press, 1897. Price, 12s. 6d.

The latest volume of the Cambridge Natural Science Manuals (Biological Series) is a useful small compendium of osteology by Mr S. H. Reynolds, Lecturer and Demonstrator in Geology and Zoology at University College, Bristol. For the most part the work is a laborious compilation, which sometimes rather detracts from its interest and value; but the actual descriptions of certain typical skeletons are based upon personal observation and research, while the clear diagrammatic illustrations are nearly all refreshingly new, many of them taken from the beautiful preparations in the central hall of the British Museum (Natural History), others from specimens in the Cambridge University Museum of Zoology.

The plan adopted by the author is to give first an account of the general skeletal characters of the group of which he is treating, with the characters of its several sub-divisions; secondly, to describe in detail the skeleton of one or more selected types; and thirdly, to treat the skeleton as developed in the group in question, organ by organ. The account of each type skeleton is made complete in itself, so that the elementary student can, if he wishes, use the book merely as a laboratory guide to the few leading forms of skeleton to which he ordinarily confines his attention.

The author is, of course, a teacher, and he presumably knows the requirements of his students; but we are inclined to think that the handbook he has produced is far from well-arranged for practical purposes. The information is admirable, usually up-to-date, and not often faulty—though a work of such wide scope must necessarily have its imperfections; but there are endless repetitions as we turn over the pages, the facts concerning a single structure or phenomenon are sometimes inconveniently scattered, and there is a lack of some fundamental idea to unite the various parts of the work into one harmonious whole. The facts of embryology may sometimes be of doubtful import, and our present knowledge of palaeontology may encourage many fanciful notions and speculations. But if both these aids to formulating a scheme be rejected, there is still the good old-fashioned method of Comparative Anatomy, which (in our opinion, at least) is more useful for teaching purposes than the disconnected mode of treatment in the handbook before us. We have noted similar want of coherence in Cambridge biological teaching before. Since the days of Francis Maitland Balfour, the philosophy of the subject seems to have become gradually neglected, while the dry facts have been more and more constantly presented in unattractive array. Mr Reynolds is likely to have the opportunity of revising his manual in a new edition very soon—for it fills a decided gap, and will be helpful to many who have hitherto been compelled to turn to numerous and varied abstruse treatises for guidance. We would therefore urge him to consider these important points, and render his work more worthy of the great labour he must have bestowed upon it.

Two New Editions

Lessons in Elementary Biology. By T. Jeffrey Parker. Third edition. 8vo, pp. xxiii. 503, with 127 illustrations. London: Macmillan & Co., 1897. Price, 10s. 6d.

Elements of the Comparative Anatomy of Vertebrates. Adapted from the German of Dr Robert Wiedersheim. By W. N. Parker. Second edition, founded on the third German edition. 8vo, pp. xvi. 488, with 333 illustrations. London: Macmillan & Co., 1897. Price, 12s. 6d. net.

These two text-books by the brothers Parker are too well known and widely appreciated to need any recommendation here. It suffices to record the publication of a new and revised edition of each of them.

Prof. Jeffrey Parker's "Elementary Biology,' in its new form, differs from the preceding editions in the increased attention devoted to the higher animals and plants. The general chapter on the higher animals has been expanded into four beautifully-illustrated chapters, dealing respectively with an outline-classification, the starfish, the crayfish, the fresh-water mussel, and the dogfish. The additional botanical matter refers in the same style to *Equisetum*, *Salvinia*, *Selaginella*, the Gymnosperms, and the Angiosperms. These supplementary sections will indeed, as the author himself remarks, contribute much to the usefulness of the book.

Prof. Newton Parker's second English edition of "Wiedersheim" may almost be described as a new book. The original descriptions and arrangement are retained as far as possible, and most of the old figures are reproduced, although a few have been replaced and others added. Prof. Wiedersheim has also revised the whole. But to bring the work up to date, and at the same time not increase the size of the volume, it has been necessary to abridge much of it and recast other portions; while the useful bibliography in the appendix has been considerably expanded to increase its usefulness to advanced students. We only notice one serious blemish, namely, the unreliable character of some of the references to the skeleton in extinct animals. The figures of the pelvic arch of *Plesiosaurus* on p. 115 are quite erroneous, and might easily have been replaced. "*Labyrinthodon ruetimeyeri*" is certainly not a labyrinthodont. The wing-finger in the Pterodactyles is not the fourth but the fifth digit. Before the next edition we would suggest that this section of the work be submitted to someone skilled in vertebrate palaeontology for revision.

Scraps from Serials

Mr Adrien Dollfus has paid considerable attention to the terrestrial Isopoda, the wood-lice of the world, and has published a paper in the October number of *La Feuille des Jeunes Naturalistes* showing the wide range of some of these little animals. Considering the mode of life of these Isopoda, it is not at all surprising to find certain species have a wide distribution, but it is interesting to have this distribution put on record. The species dealt with are *Armadillo murinus*, Brandt; *Armadillidium vulgare*, Latr.; *Porcellio scaber*, Latr.; *P. laevis*, Latr.; *Metoponorthus pruinosus*, Brandt; *Ligia exotica*, Roux. Of these five the *Metoponorthus* has the widest range, being recorded for America (N. to S.), Azores, etc., the whole of Europe, Africa (N. to S.), Asia (E. to W.), and Australia (N. Caledonia and Marianna Islands); *Porcellio laevis* is the next most widely distributed, having much the same range as the *Metoponorthus*, but found also in the Bermudas, Melbourne, and various islands of Oceania, and restricted to the N. of Africa. A sketch map accompanies the paper, which has an additional value in that it mentions the collections in which the particular specimens recorded are to be found.

The *American Journal of Psychology*, vol. viii. No. 3, contains a 'Study of Apperception,' based on experimental work on the reading of words by Dr Pillsbury. Typewritten words photographed on lantern slides were projected on a ground-glass screen, and exposed for twotenths of a second. They were mis-spelt by the omission of a letter, the substitution of a wrong letter, or the blurring of a letter by printing an x over it. The object was to determine the relative influence of the objective factor in the visual stimulus, and the subjective factor through association and apperception. The experiments were, so far as possible, carefully tabulated, and it required not a little skill (and some imagination) to educe results of any decisive value. A good *resumé* of Prof. Wundt's views on apperception is given, and the conclusions reached are in line with those obtained in a quite different way by Dr Stout in his recent work on *Analytical Psychology.*

The New Age, which was started in April, is published on the 15th of each month by the proprietor, 68/2 Shikdar Bagan Street, Calcutta. The annual subscription is Rs. 2.12. The editor is S. C. Mukhopadhaya, M.A. There is some want of discrimination shown in the selection of tit-bits for this "Journal of universal information," but it will probably be of interest to the readers for whom it is intended, and it interests us as an expression of the views of the educated Hindu community.

We regret to learn that with the October number the *International Journal of Microscopy and Natural Science*, for sixteen years the organ of the Postal Microscopical Society, has ceased to exist from lack of adequate financial support.

The *Psychological Review*, vol. iv. No. 5, contains a suggestive paper by Prof. Mark Baldwin on the "Psychology of Social Organisation." The author is one of those who are in sympathy with biological studies, and who seek to correlate the biological and the psychological factors in the development of social life from its pre-social beginnings. The same writer gives in the "Princeton Contributions to Psychology," reprinted from the preceding number of the *Psychological Review*, a discussion of "Determinate Evolution," which should prove of interest to biologists.

In the *Albuquerque Morning Democrat*, Prof. Cockerell gives a report on the Mexican dietary as studied by Prof. Goss. He finds that the principal food of the Mexican peasant is flour, corn meal, and chili, and that he is using more carbo-hydrates and less proteids than is desirable. The Mexican gets most of his proteids from frijoles, and for the better nourishment of the peasant Prof. Goss suggests a larger use of frijoles in proportion to the flour and meal. The work is being carried on at the Mesilla Park experiment station, and the object aimed at is an improved dietary after examination of soils and improvement in agricultural produce.

In the *Transactions of the Perthshire Society of Natural Science*, vol. ii. pt. 5, Colonel Duthie writes on the British abode of the Crested Titmouse (*Parus cristatus*); Messrs Coates and Macnair on a banded Hornblende Schist at Balhoulan Quarry, Pitlochry; and Mr Macnair on Rocks of Highland Perthshire. The president, Mr Coates, also gives a presidential address entitled The Origin of Soils, with special reference to the soils of Perthshire; making altogether a good geological part of the *Transactions.*

Timehri for June contains Nesting of some Guiana Birds, by C. A. Lloyd; Tobacco and Cotton Cultivation in the British West Indies, by W. H. Burnley; Result of Recent Scientific Researches into the Agricultural Improvement of the Sugar Cane, and on other Sugar Cane Experiments, by J. B. Harrison.

New Serials

The Institute of Jamaica has now begun to issue *Annals* in addition to its well-known *Journal.* We have received the first part containing a list of the Decapod Crustacea of Jamaica by Mary J. Rathbun of the U.S. National Museum.

The *Archives de Parasitologie*, under the direction of Raphael Blanchard, is a new review announced to appear in Paris next January. The parts will be published at variable intervals, each containing about 160 pages, and four will constitute a volume, price 30 francs. The journal will comprise both original articles and reviews of progress.

The *Archives of Skiagraphy* has changed its title to *Archives of the Roentgen Ray.* It is edited by W. S. Hedley and Sydney Rowland, and is published by the Rebman Co. The illustrations are chiefly of medical and surgical interest.

Further Literature Received

Vorlesungen über Bacterien, A. Fischer: Gustav Fischer. An Introduction to Geology, W. B. Scott: Macmillan. Traité de Botanique, L. Courchet: Ballière. The Mathematical Psychology of Gratry and Boole, M. E. Boole: Sonnenschein. In Northern Spain, Hans Gadow: Black. Catalogus Mammalium, fasc. iii., E. L. Trouessart: Friedländer. Darwin and After Darwin, III. Post-Darwinian Questions, G. J. Romanes: Longmans. Familiar Wild Flowers, F. E. Hulme: Cassell.

An Address on Acquired Immunity, G. Archdall Reid: *Lancet.* Notes on the Coccidae, T. D. A. Cockerell: *Rev. Mus. Paulista.* On the Nature of the Röntgen Rays, Sir G. G. Stokes: *Manchester Lit. and Phil. Soc.* Harvard University, Dept. Zoology, 1897-98.

Timehri, June; Amer. Geol., Oct.; Amer. Journ. Sci., Oct.; Amer. Nat., Oct.; Ann. Inst. Jamaica, Vol. i., No. 1; Annot. Zool. Japan, Vol. i., Part iii.; Botan. Gaz., Sept.; East Asia, Oct.; Feuilles des Jeunes Nat., Oct.; Irish Nat., Oct.; Journ. School. Geogr., Sept.; Knowledge, Oct.; Literary Digest, Sept. 11, 18, Oct. 2, 9; Naturae Novit., Sept.; Naturalist, Oct.; Nature, Sept. 23, 30, Oct. 7, 14; Nature Notes, Oct.; Naturen, Sept.; New Age, Sept.; Photogram, Oct.; Rev. Scient. Sept. 18, 25, Oct. 2, 9; Science, Sept. 10, 17, 24, Oct. 1; Sci. Amer., Sept. 11, 18, 25, Oct. 2; Scot. Geogr. Mag., Oct.; Scot. Med. and Surg. Journ., Oct.; Victorian Nat., Aug.; Westminster Review, Oct.

OBITUARIES

WILLIAM ARCHER

BORN MAY 6, 1830. DIED AUGUST 14, 1897.

WILLIAM ARCHER, who died in Dublin on August 14th last, was born on May 6th, 1830. He devoted himself for many years to the investigation of the lower plants and animals, especially the Desmids and certain groups of the Rhizopods and Infusoria. From 1876 till 1880, he acted on the editorial staff of the *Quarterly Journal of Microscopical Science*, in which most of his important work was published. Many of his valuable papers, however, were issued by the Dublin Natural History Society, now extinct, whose proceedings are, unfortunately, very scarce. He was an original member, and for many years secretary of the Dublin Microscopical Club. His eminence as a microscopist led to election into the Royal Society in 1875. In 1876 he became librarian to the Royal Dublin Society, and when the bulk of the collection was transferred to the Government to form the National Library of Ireland, Archer became head of the new institution. His later years were busily occupied in the duties of this office, and he laboured unremittingly in the transfer of the books to new quarters, and their arrangement and cataloguing on the Dewey system, of which he was an enthusiastic advocate. Two years ago he was compelled to retire, having reached the age of sixty-five. His familiar figure will be sadly missed among Dublin men of science, whose respect for his wide learning was accompanied by hearty admiration for his personal worth. G. H. C.

The following deaths are also announced :—KARL VOGEL and WILHELM LIEBENOW, eminent German cartographers ; KARL WILHELM PETZOLD, the physical and astronomical geographer ; J. H. TRUMBULL, philologist and member of the National Academy of Sciences, U.S.A.; ERNEST HUTH, professor in Frankfort and well known as a populariser of science ; EMIL SCHMIDT, a teacher of zoology in Berlin ; EDGAR MACLURE, professor in the Oregon State University, recently killed by a fall on Mount Rainier, which he was exploring with a party ; at Port Antonio, Jamaica, Dr J. E. HUMPHREY, associate professor of botany in Johns Hopkins University, Baltimore ; C. S. ROY, professor of pathology in the University of Cambridge, aged 43 ; AUGUST MOJSISOVICS, professor of zoology and comparative anatomy in the University of Graz ; Dr HOLMGREN, professor of physiology in the University of Upsala, aged 66 ; and Dr WELCKER, professor of anatomy in the University of Halle.

NEWS

THE following appointments are announced :—Dr Lehman Nitsche to be keeper of the department of Anthropology in the La Plata Museum, in succession to Dr Ten Kate ; Dr P. Zwaardemaker to be professor of physiology in the University of Utrecht ; Dr Carl Zelinka to be professor of zoology in the University of Czernowitz ; W. L. Bray to be professor of botany in the University of Texas ; H. L. Jones to be associate professor of botany in Oberlin College ; Dr Hans Reusch, director of the geological survey of Norway, to be Sturgis-Hooper professor of geology in Harvard University, for the season 1897-98.

WE have had occasion to refer to the good work that has lately been done in the Bootle Museum. The Committee did well when, some three years ago, they engaged Mr H. C. Chadwick as museum assistant. We therefore regret to learn that they can no longer afford to retain his services.

THE new Museum of the Brooklyn Institute of Arts and Sciences was opened early in October, when President Eliot of Harvard delivered an address.

DR ALEXANDER HILL, Master of Downing College, has been elected Vice-Chancellor of the University of Cambridge.

THE Eighth International Geological Congress is to meet in Paris in 1900, the year of the Exhibition. The visit to Vienna, which was to have taken place in that year, is postponed to 1903.

REUTER announces that Nossilor has arrived at Tiumen from the Kara Sea. He has explored the Yalmal peninsula, and discovered a shorter waterway between Siberia and Europe, and one free from the sea ice.

IT is proposed to decorate the Zoological Park at Washington with bronze groups of Indians and wild animals. Mr Edward Kemeys will probably receive this commission, which will put on record many vanishing types of animal life.

THE *Botanical Gazette* announces that the Smithsonian Institution has appointed a commission, with Dr V. Havard as chairman, to collect information concerning the medicinal qualities of the plants of the United States of America.

MR R. C. CHRISTIE has presented to the Owens College, Manchester, his share of the estate of the late Sir Joseph Whitworth, estimated at about £50,000. The fund is to be devoted to a new building with which the name of Whitworth can be associated.

ACCORDING to the *Athenæum*, it is proposed to establish at Swansea a branch University College in association with either Cardiff or Aberystwyth. This town already possesses an important library and museum in the Royal Institution of South Wales.

THE Academy of Sciences of Berlin has granted a sum of 3000 marks to Prof. B. Hagen, Frankfurt, for the publication of an anthropological atlas ; 1500 marks to Prof. Kohen, Greifswald, for mineralogical researches ; and 800 marks to Prof. R. Bonnet, Greifswald, for anatomical work.

Science states that plans have already been made for the new building of the American Geographical Society, New York, although the site has not yet been decided upon. The Society owes its present flourishing condition to Judge Daly, who has been its president for thirty-three years.

A collection of horns of mammals made in South Africa by Mr J. Rosen is now being exhibited on loan in the Brighton Museum. Most of the known species are represented, and many of the specimens are remarkably fine. One pair of horns of a koodoo measure 47 inches in length.

The Louisiana Society of Naturalists was founded on July 22nd. It starts with forty-five members under the presidency of Prof. J. H. Dillard of Tulane University, the secretary being Mr E. Foster. According to *Science* the Society will establish a museum and library, and will publish proceedings.

The Museum of the Perthshire Society of Natural Science is progressing favourably, though hampered by want of funds. We trust the town authorities will help in the matter after so much energy has been spent in getting things into shape. The Society's library has received a set of the 'Challenger' Reports from the Government.

Next year Mr J. E. Spurr will lead an expedition to Alaska to make a further survey of the gold resources. $5000 has been appropriated by Congress for the purpose, but according to *Science* an effort will be made to increase the appropriation to $25,000 in order that a complete survey may be made and a geological map of the region prepared.

During the present session Prof. Boyd Dawkins will give a series of twelve short addresses on geological subjects in the Manchester Museum. These are to be delivered alternately on Saturday and Sunday afternoons. We are glad to note that the Museum has just received a donation of £1500 from Mr Edward Holt towards the building fund.

According to the *American Journal of Science*, the Geological Survey of Canada has recently acquired a mass of meteoric iron from Thurlow, Hastings Co., Ontario. It is an irregularly-shaped, truncated pyramidal mass, with a more or less rectangular base, measuring 0·16m. by 0·135m., and weighs 5·42 kilos. It is to be named the Thurlow meteorite.

The International Ornithological Congress will meet this year at Aix on November 9. The International Congress of Zoology will meet at Cambridge on August 23, 1898. Sir William Flower has issued a circular letter asking for co-operation with the general committee in raising a fund to defray the necessary expenses of the meeting. The committee has just met in London to arrange the preliminaries.

Prof. Michael Foster has been delivering lectures in Baltimore during October, and he now proceeds to deliver a course of Lowell Lectures at Boston. In the middle of the month, Dr Nansen passed through London on his way to America, where he has many engagements. *Science* announces that at the close of his first lecture at New York a medal will be presented to him by the American Geographical Society.

At the annual meeting of the Hull Scientific and Field Naturalists' Club, held on September 29, it was reported that great progress had been made during the year. Thirty-eight new members had been elected, and the attendance at meetings had increased by 50 per cent. The Society wisely arranges its programme so that lectures of general scientific interest alternate with its more technical and original local work.

The Trustees of the British Museum have been approached by the Council of the British Association with regard to the establishment of a Bureau of Ethnology for Greater Britain. If this arrangement can be come to there is no doubt that the information obtained would be of great service to science and utility to the Government. Sir John Evans, Sir John Lubbock, Mr C. H. Read, and Prof. E. B. Tylor made the report which was placed before the British Association.

Sir Joseph Hooker has finished the "Flora of British India," begun twenty years ago, and has received, according to the *Kew Bulletin*, a despatch from the Government of India, through Sir George Hamilton, recognising his services to India in cordial and sympathetic terms. Sir J. Hooker has, we are also glad to learn, offered to undertake the preparation of the remaining volumes of the late Dr Trimen's "Handbook to the Flora of Ceylon," and the necessary material and specimens have already arrived at Kew from Peradeniya.

Among the Russian geologists who took part in the Oural excursion, none endeared himself more to his fellow-travellers than the young candidate in Natural Science, L. Spendiarow. He died suddenly of heart disease almost immediately after the return to St Petersburg. His father has given to the International Geological Congress the sum of 4000 roubles, the triennial interest of which is to be awarded by the President of each Congress as a prize for the best geological work done during the preceding three years.

We learn that the Government have presented a set of the 'Challenger' reports to certain local scientific societies. This is a very excellent stimulus, no doubt, but we hope the familiar notice that is to be found inside the Record Office publications deposited in our Free Libraries is to be found also in these scientific reports. Local societies exist by the enthusiasm of the few, and when they fall into decay such volumes might well be taken from them and passed on to another centre where they will be more appreciated.

The Bulgarian Government has received by bequest from Eulogius Georgieff, the founder of the Sofia University, the sum of 20,000,000 francs for public purposes. This includes 6,000,000 francs for a technical school for Sofia. The University of Lyons will devote 42,000 francs to complete the biological laboratory of Tamaris, near Toulon, and will probably endow it to a moderate extent. Indianapolis will receive fifty-six acres of land for a botanical garden and ornithological preserve from Mr W. W. Woolen. Yale University has received $5000 by the will of Miss Julia Lockwood for the foundation of a scholarship.

The University Extension Lectures in London for the coming session were listed by *The Echo* on October 5. The following may interest some of our readers :—The Geography of Britain and the British Seas, by H. J. Mackinder, at Gresham College ; the World's Great Explorers, by H. Yule, Oldham, at Toynbee Hall ; Evolution and Darwinism, by E. O. Paskyn, at Lewisham ; Physiology of Plants, by E. O. Paskyn, at Morley College ; the Earth, by F. W. Rudler, at Croyden and Toynbee Hall ; Our Common Minerals, by F. W. Rudler, at West Ham ; and Human Anatomy, by Chalmers Mitchell, at Toynbee Hall.

Miss Kate M. Hall, Curator of the Whitechapel Museum, has made arrangements to co-operate with the teachers of the elementary schools in the district in demonstrations to their classes when they visit the Museum. As is well-known, the Code of 1895 allows visits to be paid during school hours under proper guidance to museums, art galleries, and other institutions of educational value, such visits being counted as "attendances." At Whitechapel each headmaster or mistress is invited to bring one assistant and forty-five pupils on each occasion. The pupils are divided into three groups, the teachers and Miss Hall each taking one. Such specimens as may be removed from the cases are arranged on three tables.

Each group spends a quarter of an hour at each table, and the rest of the hour is devoted to questions or to a general survey of the Museum.

The meeting of the Botanical Society of America, which was held at Toronto at the time of the visit of the British Association, seems to have been a great success. Dr J. M. Coulter was the president, and there were present a large gathering of English and Foreign botanists. N. L. Britton of New York was chosen president for 1898, which session will be held in Boston, an invitation from the Missouri Botanic Garden for the spring of that year having been reluctantly declined. The chief papers were—A case of ecblasteris and axial prolification in *Lepidium apetalum*, by B. L. Robinson ; Movement of protoplasm in coenocytic hyphae, by J. A. Arthur ; Pollen grains and antipodal cells, by J. M. Coulter ; The transition region of the *Caryophyllales*, by F. E. Clements ; A revision of the species *Picea* occurring in North-Eastern America, by D. P. Penhallow ; Bibliographic difficulties, by E. L. Greene ; and the botanical gardens of Jamaica, by W. Fawcett.

CORRESPONDENCE

INHERITANCE OF ACQUIRED CHARACTERS

In view of the doctrine which constitutes the corner-stone of Prof. Weismann's theories of heredity—the non-inheritance of acquired characters—any exceptions to the rule which he lays down may be worth notice. I desire to submit to your readers for consideration one which has appeared to me to bear upon this question—viz., the direction in which the hairs slope on the extensor surface of the forearm in certain hairy quadrupeds and in man. The course which the hairs take on this small area varies in different animals, but, as far as I can learn, in two main directions only. In the first of these, which one may look upon as more 'normal,' the slope is on the whole directed in the axis of the limb towards the distal extremity, and is thus in keeping with the general slope of hair on the other areas of this and the posterior extremity of the animal, as in the great Ungulate order and a few species of monkeys. The second type of direction presents a certain reversal of the slope—viz., that the hairs after a manus-ward course on the flexor surface curve round the lateral borders of the fore-arm, and when they reach the extensor surface they pass in a reversed direction, in some animals on the whole area, in others on the proximal portion only, towards the trunk. This second type is seen clearly in man at all ages, in the anthropoid apes, especially in the long-haired Orang, to an extent exceeding any other animal I have seen, in most lower monkeys, and in the Carnivores. In the last-named order the direction is visible in the terrestrial Carnivores, Arctoidea, Cynoidea and Aeluroidea, especially in the shorter-haired forms, but in those with longer hair on the limbs the general set of hair on this area is towards the trunk, even if indistinct in some. It is the second of these types to which I would draw attention as being a departure from the first, which is seen in Ungulates, and which is more in accordance with the natural arrangement of hair. In this great group of animals, the Ungulates, there is a very general habit of flexing the fore-limb in the attitude of repose, as one sees commonly in a herd of cows and horses grazing, when these limbs lie doubled up and the hoofs are resting under the fore part of the trunk. The same position can be observed in other families of this order in confinement. A similar slope of hair is also seen in certain Marsupials, Kangaroos, for example ; and in these animals, which almost always lie on their sides when at rest, the position would be indifferent as to influence upon the slope. In Ungulates the pressure thus exercised when the limb is in acute flexion would act at right angles to the axis of the limb, and, except in so far as it would confirm the original slope of the hair on the extensor surface, it would be indifferent in its effect on the hair-slope. But in the Primates, Carnivores, and certain other animals in which the surface of this limb-segment in question is exposed to pressure acting in a different manner, one is not surprised to find a different slope of hair. In Man it is frequently subject to pressure against some underlying fixed surface. In apes and monkeys of all kinds it is extremely common in their sitting posture to see their upper extremities strongly flexed at the elbow and resting against their lower limbs. In terrestrial Carnivores the normal attitude of repose is, except on the occasions when they lie asleep stretched out on their sides, that the fore-limbs are planted in front of the trunk, seen most noticeably in the 'couchant' position which they commonly assume. In all these instances it is obvious that there is a slowly acting mechanical force in the downward and forward direction by reason of the weight of the limb itself, and the fore part of the trunk which is supported by it. The effect of this pressure would be to cause the hairs to slope towards the trunk, as is found to be the case in these animals. There are of course many apes and monkeys, notably the chimpanzee, gorilla, and orang, in which the same direction is taken by the hairs on other aspects of the same limb on regions where no pressure can influence it. In these the general set of hair towards the trunk is probably due in part to the effect of gravitation on the long hair, and perhaps, as Mr Wallace has suggested, to the influence of heavy rain in tropical forests when the hair would act to the body as thatch does to the roof of a house,

The direction of hair-slope on this area is, of course, congenital ; it is therefore a primary character, not one acquired by the individual through secondary forces. It is congenital in the human infant, in the young monkeys and young Carnivores which I have been able to examine, and presumably it is so in all in which it is found.

The consideration of this point leads us to the conclusion that the character before us is one which it is impossible to look upon as having any 'survival-value,' or as due to any form of selectional processes within the germ acting under adaptive requirements. An adequate secondary cause is suggested for its original production in the ancestors of those which now exhibit it. Perhaps someone more qualified to speak could determine whether or not this is one of the exceptions to Prof. Weismann's rule, the non-inheritance of acquired characters.

WALTER KIDD.

12 MONTPELIER ROW,
BLACKHEATH, S.E.

INSECTS AND FLOWERS

WITHOUT at this time attempting to discuss the general arguments used by Mr Bulman in his interesting paper in your August number, I should like to draw attention to the following paragraph (p. 103):—

"Again, if our native flowers are the result of the selective action of our native bees, and those which they have specially chosen for countless generations, how is it that bees take so readily to many flowers of very different forms introduced into our gardens from abroad? For such introduced plants are in many cases freely visited by native bees."

I think we need more information about this matter. I have found, here in New Mexico, that garden flowers do not as a rule attract many species of native bees, unless they are very closely related to native flowers. A flower garden is nearly always disappointing as a hunting-ground, the bees found there being mainly certain widely distributed types which visit very many species of native plants. In a luxuriant garden at Santa Fé the best collecting is on the weeds, not at all on the cultivated flowers.

Solidago canadensis is a native plant very common in New Mexico. It is also grown in gardens in Europe, and a small list of common flies visiting it there is given by Hermann Müller. In Las Cruces, on Aug. 30 of this year, Prof. C. H. T. Townsend was sweeping *S. canadensis* for flies. He swept at the same time a lot of hymenoptera, which he handed to me. I have sorted out the fossores and bees, and here is the list:—

ANTHOPHILA

Agapostemon melliventris, Cress. ♂.
,, *radiatus*, Say. ♂.
Panurgus rhodoceratus, Ckll.
Colletes americana, Cress.
Halictus stultus, Cress. vel peraff.
,, *ligatus*, Say.
,, sp.
Nomia nevadensis, Cress.
Melissodes agilis, Cress. ♂.
Perdita sphaeralceae, Ckll., one ♂.*
Epeolus lunatus, Say vel peraff.
Podalirius maculifrons, Cress.

FOSSORES

Microbembex monodonta, Say.
Philanthus ventilabris v. *frontalis*, Cress.
,, sp.
Steniolia duplicata, Prov.
Oxybelus quadricolor, Ckll. and Baker, ♂.
,, *sparideus*, Ckll.
,, *abdominalis*, Baker. ♂. (new to N. M.).
,, sp.
Plenoculus cockerellii, Fox.
Stizus godmani, Cam.
Aphilanthops laticinctus, Cress.
Cerceris acanthophilus, Ckll.
Myzine frontalis, Cress. M. S. ♀.
,, *hyalina*, Cress. ♂.
Anacrabro boerhaaviae, Ckll.
Paratiphia, sp.
Scolia, 2 spp.

I have on former occasions done equally good collecting from the *Solidago*. Who can show a similar list in Europe collecting from the same plant? I doubt if it can be done, yet the plant belongs to a European genus, and is much less specialised for insects than many others.

T. D. A. COCKERELL.

MESILLO, NEW MEXICO, U.S.A.,
Sept. 3, 1897.

* I suspect that this came from a *Sphaeralcea* growing among the *Solidago*, especially as there are in the lot two beetles which breed on *Sphaeralcea*. These may, however, have strayed from their normal plant.

WOMEN VERSUS BIRDS

In *Natural Science* for August (p. 77), the question is asked as to what more we can do besides attempting to influence women in the home-circle so as to prevent their barbarian slaughter of the birds for decoration of themselves. I think we could do a great deal more, and will instance this by an account of what occurred here some years ago. A Frenchman settled down in the neighbourhood of Mount Ophir in Malacca as a collector of bird skins for the trade. He employed a large number of native hunters, and the slaughter of our most beautiful birds—sun-birds, trogons, fairy bluebirds, and many others—was horrible. However, the Government, on learning this, passed an ordinance forbidding the killing of birds in the colony without a licence. The man then moved to one of the native states out of colonial jurisdiction, but the law was immediately introduced there. Then it is said he sold the goodwill of the business to an unsuspecting native and disappeared. The native, of course, on attempting to carry on the business was arrested. Since then our birds have returned to their desolated haunts and are as plentiful as ever, and no further attempt has been made to establish the trade in this country. Indeed, I can hardly remember a case where it has been found necessary to put the law in force, though, of course, a few birds are now and then illegally killed by shooters. Of course we are much assisted by the licensing of guns. Natives, for excellent reasons, are only allowed gun licences for purposes of defence against tigers, robbers, etc., or to keep away wild pigs or other destructive animals, and this checks bird-killing very well.

In Borneo, under British rule or influence, the Mias is also protected, no person being allowed to kill one except by special permit, only granted for scientific purposes. And licences to collect orchids are also issued there to check the wholesale destruction of these plants.

The forests of the Dindings, especially one jungle round a hill called Gunong Tungul, are tenanted by rhinoceroses, which the natives used to trap in pitfalls. As the animal is perfectly harmless, and a very interesting beast, the District Officer, Mr W. C. Mitchell, endeavoured to prevent its destruction, and though the Government did not see their way to legislating to save it, he succeeded in preventing any more from being killed. The Rhinoceros is considered by the Chinese as a very valuable medicine, or rather collection of medicines. They pay large sums for its carcase, and dry and prepare every bit of it to ship to China. This was the only inducement to the Malays to kill it. Thanks to this action there are still plenty in this district, where I have more than once come across them, or heard them gallop off snorting like a pony.

Now, for the trade in bird-skins in colonies or countries under British influence there can be no excuse. It can and should be stopped by local ordinance. The difficulty lies in countries not under our control, such as Mexico, Brazil and New Guinea. In nearly all these places, however, there are naturalists, and often Natural History Societies. It should be the duty of all scientists in these countries to bring pressure to bear upon their Governments to check or prevent the trade in bird skins. Part of New Guinea is under British influence, and any destruction of Birds of Paradise here could be prevented. The naturalists of Holland and Germany might also be asked to approach their Governments on the subject, and to stop the trade in their Colonies.

In the list of birds destroyed given in the article in *Natural Science*, it is not altogether easy to guess from what part of the world the skins and plumes are derived. Some are evidently Indian—*e.g.*, Peacock, Indian Parrots, Impeyan Pheasant, &c. ; others Tanagers and Humming Birds from South America. Let our naturalists at home first trace up the sources of the supply, and then those in the countries whence the skins come can be appealed to to bring the matter before their respective Governments. This plan, though it will probably not entirely stop the trade, will at least save the birds of some corners of the world, and will strike a blow at the fashion which I trust it may never recover. H. N. RIDLEY.

SINGAPORE, *April* 25, 1897.

AUSTRALIAN NATURAL HISTORY

As an Australian worker I trust that you will allow me to protest against the hostile criticism of an Australian student, as such, by a London authority, as such, on p. 5 of this volume. Criticism of Australian work upon its merits will always be welcome, but when a reviewer is invoked to write down a paper because if a certain arrangement "was not made it ought to have been" then fair play is disregarded and the honest aims of criticism made subservient to less worthy ends. For it is evident that under ordinary circumstances the brilliant writer of your editorial would not have stooped to crush a weaker brother on the score of a few misprints, and the difference which will ever exist in their view of what constitutes a 'species' between a 'splitter' and a 'lumper.'

More than a year ago three naturalists gathered each a miscellaneous collection from a Pacific Atoll; two of these went direct to London, the third to Sydney. Not a word has as yet been written on the material sent to England. A generation since Darwin complained, in sorrow and surprise, that he could not secure specialists to work out the results of his South American journey. To-day boundless wealth of material pours into London, but what proportion of it is ever studied? There is no lack in London of material, of Pacific material, even of Funafuti material for students with an appetite for work: yet is it touched? But if a local student makes an honest attempt to further the cause of science, a bitter cry arises from the British Museum specialist—defrauded of his rights! J. DOUGLAS OGILBY.

LIVINGSTONE ROAD, PETERSHAM, SYDNEY,
25th August 1897.

FUNAFUTI

IN the first paragraph of your review of "Australian Museum Memoir III. (on Funafuti Atoll), part 2," which appeared in your July number, is a statement of your impression that a stipulation had been made for the Royal Society to have the right of prior publication, and that if such an agreement was not made it ought to have been.

I am directed by the trustees of the Australian Museum to put you in possession of the actual facts of the case; and, as your statement has been made public, to request that this also might be given the same prominence.

On 7th April 1896 the Local Committee in Sydney, representing the Royal Society of London, asked the trustees to nominate an officer to accompany Prof. Sollas on the Expedition to Bore a Coral Reef, and Mr Charles Hedley was appointed. The trustees were informed in a letter, signed by the Chairman of the Local Committee, that "in regard to the secondary objects, that is, the Collection of Specimens of Natural History, each member of the expedition will be at liberty to retain or exchange anything he may obtain. The expedition as an undertaking, therefore, does not interfere with the collections of [Mr Hedley]. Your trustees are thus at liberty to impose what conditions they think best upon their representative in this respect." In addition to this, the question of publication was raised at a meeting of the Local Committee, at which one of the trustees of the Museum and the curator were present as well as Prof. Sollas, and the reply was that no restriction would be placed on it.

It will thus be seen that, in taking part in the expedition, a stipulation was made beforehand for the right of publication of the results obtained by the trustees' representative.

S. SINCLAIR,
Secretary to the Trustees of the Australian Museum.

SYDNEY, *8th September* 1897.

[We deeply regret that information which we had from two independent sources, believed to be authoritative, should have led us to make any suggestion of unfairness on the part of our friends and fellow-workers in Sydney. A correction of our error was published before the various Australian protests reached us. The rest of our remarks, whether of praise or blame, remain absolutely unaffected by this correction.—ED. *Nat. Sci.*]

NOTICE

TO CONTRIBUTORS.—All Communications to be addressed to the EDITOR of NATURAL SCIENCE, at 67 St James' Street, London, S.W. Correspondence and Notes intended for any particular month should be sent in not later than the 10th of the preceding month.

TO THE TRADE.—NATURAL SCIENCE is published on the 25th of each month; all advertisements should be in the Publishers' hands not later than the 20th.

TO OUR SUBSCRIBERS AND OTHERS.—There are now published TEN VOLUMES OF NATURAL SCIENCE. Nos. 1, 8, 11, 12, 13, 20, 23, 24 being OUT OF PRINT, can only be supplied in the set of first Four Volumes. All other Nos. can still be supplied at ONE SHILLING each.

Price of Set of Vols. I., II., III., IV.	£2 10 0
,, ,, ,, V., VI., VII., VIII.	1 4 0
,, ,, ,, I.—VIII.	3 10 0

One Shilling each Number of any Bookseller.

Annual Subscription, payable in advance to J. M. DENT & CO., 67 St James' Street, London, S.W., Thirteen Shillings ($3·50), post free.

NATURAL SCIENCE

A Monthly Review of Scientific Progress

No. 70—Vol. XI—DECEMBER 1897

NOTES AND COMMENTS

The Fauna of the Deep Sea

One of the most important publications of the past month is Dr Günther's Presidential Address to the Linnean Society of London, just issued in this Society's *Proceedings*. It is a critical review of our present knowledge of the depths of the sea, by one who has made a life-long study of the subject, and added no small contributions to the solution of the problems in question from the biologist's point of view. Dr Günther treats the subject under geographical headings, being of opinion that this arrangement brings out some points of interest more prominently than a general historical statement would do. He is careful, however, to emphasise the fact that he still believes the deep-sea fauna to be one indivisible whole; such types as seem to be characteristic of some particular region of the ocean being "accidentally or ignorantly imported into the deep-sea fauna," or else forms of which the wide range has not yet been ascertained.

Among the many interesting points discussed, there are two expressions of opinion of great importance from one who has so many qualifications to speak authoritatively. The first relates to the question of the permanence of the abyssal ocean depths; the other refers to the presence or absence of life in the middle-depths of the oceanic waters.

Dr Günther is emphatically in favour of the idea that the abysses of the ocean are not permanent, but may well have changed many times in the past. He declares that the opposite view "cannot be accepted by the student of the terrestrial fauna." He remarks: "I cannot help thinking that our knowledge of the nature of the rocks at the bottom of the sea is, at present, to use a mild expression, most imperfect. Is it not possible that continental rocks at the abyssal sea-bottom are so hidden under the deposit which has been in progress of formation for untold ages, as to prevent us from penetrating to them? Possibly the day may come when borings or some similar operation will be successfully carried out in the abysses, entirely upsetting our present ideas of the geological nature of the sea-bottom. Besides, we have no other means for accounting for the distribution of the terrestrial fauna, more especially in the southern hemisphere, except by assuming that great changes have taken place in the extent and position of continental land, and, moreover, that these changes were still in progress at periods at which our present fauna, or at least part of it, was already in existence."

The question of the existence of a "mid-water fauna" is considered by Dr Günther to be still an open one, to be "decided only by continued tow-net experiments in great depths of the open ocean, with a bottom of clean ooze or mud." Agassiz, as is well known, maintains that there is no such fauna. Dr Günther, however, observes that the experiments of the distinguished American naturalist "prove too much. His tow-nets came up always empty from the intermediate zones. It is very singular that he should not have caught even some of the dead bodies which, like rain, drop constantly from the surface to the bottom. Further, so far as fishes are concerned, there is no reason why certain forms should not permanently inhabit intermediate zones, inasmuch as also pelagic fishes are undoubtedly free swimmers for nearly the whole of their life, without being tied to the proximity of *terra firma.* The ova of many species which live in the mature stage at the bottom of great depths are pelagic, and hatched at or near the surface. The young continue to live for some time under pelagic conditions (*Plagusiae, Leptocephali, Polyprion*), but as they grow they descend to the deep sea. It is very improbable that this descent is rapid; it must be gradual in order to allow the physiological functions to get used to abyssal conditions; or, in other words, these fish must live for some time in mid-water."

Finally, Dr Günther adds an appropriate plea for more deep-sea work in the Indian Ocean and the Antarctic regions. The ordinary survey of the seas round British India is now nearly completed, but researches on the fauna have only proceeded just far enough to demonstrate their interest and biological importance. "It seems a pity," as Dr Günther remarks, "that while the experience gained on board the 'Investigator' is at least still partly available in the service, no further benefit should accrue from it for science." In reference to the Antarctic Ocean, he points out that our knowledge of its abyssal life rests merely on six trawlings of the 'Challenger.' We trust that ere long the British Government will be induced to help further in this important biological work. To use Dr Günther's words, "the beneficial influence which every purely scientific undertaking exercises upon mankind reaches far beyond its immediate aim." Science "is the mother as well as the daughter of peace."

The International Zoological Congress, 1898

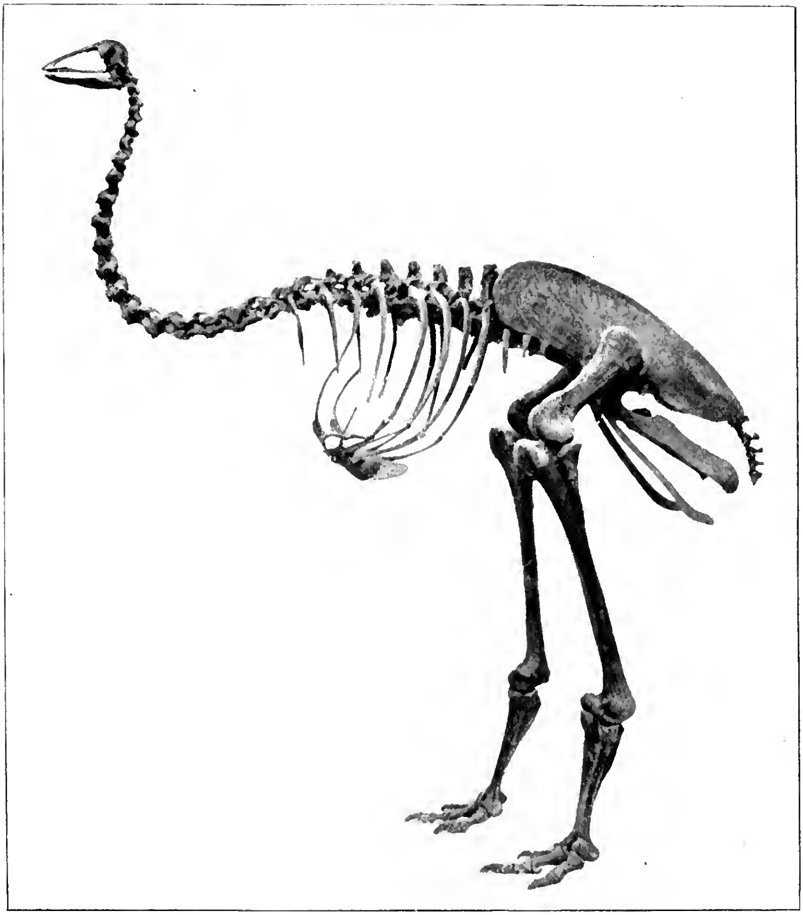
The meeting of the General Committee appointed to arrange for the next meeting of the International Zoological Congress was marked by an unfortunate lack of union. One well-known zoologist described it as the most turbulent meeting he had ever attended. The Congress is to meet in England next August, and the original arrangement was that it should be held in London, which, for various reasons, has now been altered to Cambridge. The success of a similar Congress in America in 1891 was seriously affected by a change in the place of meeting, which led to the abstention of a great number of American men of science, who objected to the alteration. Cambridge does not now occupy the position in the English zoological world which it did in the days of F. M. Balfour. There are no doubt strong reasons for the selection of Cambridge, although the town is not central. But remembering the consequences of the change of locality of the American Congress in 1891, the advocates of Cambridge might have done their best to conciliate provincial representatives. Their attitude was decidedly the reverse. When, for example, Colonel Wardlaw Ramsay proposed that in order to secure one Scottish member on the executive, Sir William Turner should be elected a vice-president, Professor Newton formed the minority of one who voted against it. Professor Poulton, also anxious to make the committee more representative, proposed that the presidents of the Linnean and Entomological Societies should be *ex officio* vice-presidents, which secured at once the warm support of the meeting. The chairman, however, expressed himself confident that Mr Poulton, on thinking the question over, would see the advisability of withdrawing his resolution and allowing the executive committee to select itself the additions to its number. But Mr Poulton remarked that he did not see the advisability of his withdrawing his resolution, and thought it much better not to trust the executive committee to make the additions recommended. The resolution was carried by an overwhelming majority. So the cut and dried plans of those who had arranged the agenda were not accepted quite as they stood. A member who proposed one of the official resolutions read it out as "the resolution I have been 'instructed' to propose;" and then recommended it to the meeting by one hostile criticism. Dr Murie proposed that the executive committee should be composed of an equal number of members from England, Ireland and Scotland, a remarkable suggestion that only fell non-seconded because the zoologist who attempted the feat could not complete his sentence until the meeting was well advanced in the consideration of the next business. A few proposals of this sort, by claim for zoological independence for Wales and the Manxmen, kept the meeting merry. But it seems quite clear that the provincial and non-official London zoologists view the present committee with suspicion, as not sufficiently representative of British zoology. Sir John Lubbock has succeeded Sir William Flower in the presidency, and he may be trusted to prevent any further irritation of the majority by a tactless disregard of its manifest wishes. It is a source of the deepest regret to all British zoologists that Sir William Flower himself should be compelled, by need of rest, to refrain from any active part in the arrangements.

Stratigraphical Geology

In connection with Sir Henry Howorth's articles on Geological Nomenclature which we are now publishing, attention may be directed to a paper by Dr Charles R. Keyes, recently read before the St Louis Academy of Sciences, and abstracted in *Science* for October 29 (n.s., vol. vi., p. 655). Dr Keyes declares that "for more than a score of years that branch of geology called stratigraphy has been practically at a standstill. Its methods are the same that were used fifty to seventy-five years ago." At last, however, the problems of the correlation of sedimentary rocks can be attacked in a new way suggested by the field-work of many American geologists. Organic remains, it appears, may now be entirely omitted from consideration, and the relative age of the various strata can be determined solely by observing the succession of geographical changes in the various large areas under comparison. These new methods, Dr Keyes remarks, are more or less complex and far from simple; but he is hopeful that they will eventually lead to a really natural classification of the rocks and definitely put an end to what has been aptly termed 'parochial geology.' He is especially sanguine as to the value of the results to be obtained from a detailed study of the phenomena of mountain-formation. We cannot follow the whole argument from the brief abstract; but any advance in methods which will enable us to restore the geographical features of wide areas of the earth's surface at different successive geological periods will not only make a new era in geological science but also contribute most materially towards the solution of some of the perplexing problems of zoology.

The Geology of Patagonia

This leads us to refer again to the question of the Tertiary deposits of Patagonia and their remarkable mammalian fossils, discussed by Dr Florentino Ameghino in our October number (p. 256). Mr J. B. Hatcher, who has spent much time in studying this southern extremity of the American continent, now expresses the opinion that



RESTORED SKELETON OF AN EXTINCT STRUTHIOUS BIRD

Aepyornis hildebrandti

From a Peat-deposit, Sirabe, Central Madagascar

[*Obtained for the British Museum (Natural History) by* DR FORSYTH MAJOR] there is no geological basis for Ameghino's asserted discovery of the Cretaceous ancestors of the mammalia in that region (*Amer. Journ. Sci.* [4], vol. iv., pp. 327-354, Nov. 1897). He finds, apparently, Jurassic rocks there on the Mayer river; he also identifies the supposed Cretaceous series containing Dinosaurian bones. When in Patagonia, however, he never discovered either a mammal bone or a tooth in the deposits yielding Dinosaurian remains, and he arrived at the conclusion that the beds containing *Pyrotherium* were not only later than these, but probably more recent even than the Marine Patagonian Formation itself. His words are:—"It is certainly remarkable that in these beds containing Dinosaurian remains, associated, according to Ameghino, with the remains of mammals, some of them, as for example *Pyrotherium*, of immense size, only a little less than that of the elephant and consequently easily to be seen, I could have searched for weeks without ever finding a single mammalian bone, while every day I found Dinosaurian remains."

We await Dr Ameghino's observations on Mr Hatcher's results with great interest, for on the settlement of the Patagonian problem great issues depend. It is to be hoped that ere long some other geologist skilled in the modern methods of stratigraphy will investigate the subject and give us another independent opinion.

The Aepyornis of Madagascar

We have several times referred to the important results of Dr Forsyth Major's explorations and researches in Madagascar. We now have the pleasure of directing attention to the latest fruit of his labours in the form of a nearly complete skeleton of the extinct struthious bird, *Aepyornis*, which was mounted for exhibition last month in the public galleries of the British Museum (Natural History), South Kensington. The skeleton is shown in the accompanying photograph (Plate X.), for which we are indebted to the courtesy of the Editor of the *Geological Magazine*, and it was described last June in the journal just mentioned by Mr C. W. Andrews. The bird must have been about five feet in height when alive, so that it represents one of the smaller species of the genus. The bones do not belong to one and the same individual, but they have been selected from a very large series and have every appearance of giving the animal its correct proportions. The skull is imperfect in front, but the top of the brain-case is marked with rows of deep pits, which appear to indicate the original presence of a crest of large feathers. The mandible is very stout. The vertebral column, as reconstructed, consists of twenty true cervicals and eight vertebrae bearing free ribs; the fused pelvic vertebrae are about twenty in number. The sternum, or breast bone, is very short and broad, while the coraco-scapula is much like that of a cassowary. The wing is reduced to a mere rudiment, namely a small humerus and one other bone which seems to represent the whole of the rest of the limb. The hind-limb has three toes, and there also seems to have been a rudimentary hallux, though this has not yet been found. The specimen in the Museum is appropriately placed by the side of *Dinornis* and the ostrich, and is one of the most striking additions to the collection of fossil birds acquired during recent years.

Swedish Arctic Exploration

We are very glad to learn that the Swedish Arctic expedition, which Prof. A. G. Nathorst has been advocating for some time, is likely to take definite shape next year. The necessary means have at last been procured, thanks chiefly to the liberality of King Oscar and the late Baron Oscar Dickson. The main objects of the expedition, Prof. Nathorst informs us, are the scientific, and especially the geological, investigation of the east coast of Spitzbergen, which as yet is very little known; the charting and exploration of Kung Karl's Land; and, should time permit, of the little known islands between Spitzbergen and Franz Josef Land. It is proposed to take an observer specially familiar with the study of recent land movements and glacial action, a botanist, two zoologists, who amongst other things will, together with the hydrographer, make observations on the plankton; the hydrographer will also be a meteorologist. In addition there will be a combined cartographer and photographer, and a specialist in degree-measurement. An arc of the meridian will be carefully measured on the east coast of Spitzbergen, but during the course of one summer it will only be possible to make the preliminary studies for this work. Professor Nathorst himself will continue his previous studies in both the botany and geology of the region, as well as having the command of the expedition. He hopes to be able to take three scientific helpers in addition to those mentioned, but this will depend on the size of the vessel that is obtained.

With the notable exception of Andree's bold attempt, Sweden has of late not taken her usual share in Polar exploration, but the names of Torell, Loven, Nordenskiöld, and other Swedes are so intimately connected with Spitzbergen that the forthcoming expedition is but the natural sequel to a long series of scientific voyages. With the fresh forces now at his command for attacking the problems of the Arctic seas, Professor Nathorst may expect, and we sincerely hope that he will meet with, even more success than his predecessors.

American Fossil Brachiopoda

Despite the many valuable monographs that have been issued of late years by our American colleagues, the study of the fossil Brachiopoda has hitherto been a task to be undertaken with trepidation. The labours of James Hall, J. M. Clarke, C. E. Beecher, and Charles Schuchert have considerably changed our views as to the inter-relationship and classification of these animals, and have rendered necessary extensive revision of nomenclature. But while we had an uneasy feeling that the names in our text-books and the labels in our museums were of too ancient a kind, we shrank from the difficult duty of resorting and renaming. The magician prepared to substitute new lamps for old arises in the person of Mr Charles Schuchert, who essays the task for the American fossil species; and, since North America seems to have been the gathering-place of the brachiopod clans in Palaeozoic times, much of this welcome light is also available for European species.

The book that forms the necessary keystone to previous writings is entitled "A Synopsis of American fossil Brachiopoda, including bibliography and synonymy," and has just been issued from Washington as *Bulletin, No.* 87, of the United States Geological Survey. It has, however, been prepared after official hours, and represents the work of eleven years. The main part of the book is the "Index and Bibliography of American fossil Brachiopoda;" which occupies 227 pages and contains about 10,000 references. All names that have ever been applied, rightly or wrongly, to fossil brachiopods of North and South America, are here given in alphabetical order. The names accepted by the author, after careful research, as valid, are printed in bolder type, and under each is given the geological age, chief localities, and a list of synonyms. Under each generic name is quoted the name of the species, whether American or not, that served as the original type of the genus; Mr Schuchert calls this the 'genotype.'

The index is preceded by some useful and interesting chapters on general questions, accompanied by some elaborate tables. These chapters are:—"I. Geologic development and geographic distribution of American fossil Brachiopoda." "II. Brachiopod terminology, applied to fossil forms"—practically an alphabetical glossary of terms. "III. Biologic development of the Brachiopoda"—an exceedingly important chapter. "IV. Morphology of the Brachia," contributed by Dr C. E. Beecher. "V. Classification of the Brachiopoda," in which the point of chief importance is the entire dismissal of the old division into Lyopomata and Arthropomata (= Inarticulata and Articulata), as discordant with the facts of race-development.

While this book will, as we have said, be an important aid to students of the European fossil brachiopods, it by no means fills the gap that has long yawned before them. A corresponding European work is greatly to be desired. F. Béclard began something of the kind, but death has stopped his energetic research. In England we hardly take our fair share in the production of these useful bibliographic lists of recent or fossil organisms; and yet their publication would prevent many of the descriptions of supposed new species that are constantly being thrust upon us. Once upon a time there was a great flourish of trumpets over a new edition of Morris' "Catalogue of British Fossils" as a memorial to that geologist, but though the talkers were many the labourers were few. Surely the scheme might be undertaken by half a dozen really serious workers, and brought up to the end of the century.

To return to Mr Schuchert. We can do no more than thank him for the result of his labours: he has given us a key to the writings of the last two decades, he has simplified our researches, and has taken a heavy load off our minds. We congratulate him on the completion of his task, and are glad to place another solid contribution to zoological literature on our bookshelves.

Wasps and Weather

On two previous occasions (*Natural Science*, Vol. iii., pp. 273-275, and vi., pp. 178-179) Mr Oswald H. Latter has called attention in these pages to an apparent connection between abundance or scarcity of wasps and certain meteorological phenomena. Briefly, his conclusion then was—(1) that wasps were favoured by dry springs and early summer, while if these seasons were wet wasps were scarce in the later summer and autumn; (2) that low temperature during the winter and early spring had little or no effect. He now favours us with a supplementary communication, pointing out how his observations and enquiries during the present year confirm his earlier conclusions. "A complete survey," he remarks, "must begin with September 1896: this was the wettest September known for many years, the rainfall at Godalming amounting to 7·12 inches; from that month up to the end of May of the present year the rainfall for every month, except November, was considerably in excess of the average, while that of March amounted to 5·01 inches. The total rainfall for the nine months, September 1896 to May 1897, amounted to 30·38 inches, which is rather more than 2½ inches in excess of the annual rainfall. Thus the period of hibernation and nest-founding among wasps was extremely rainy. The temperature of these months, notably that of February and March, was decidedly above the average, and thus many Queens were early tempted from their winter quarters. Turning now to the numbers of wasps observed—in spring there were many 'Queens' to be seen, and many persons observed to me that we might expect a recurrence of the 'plague' of 1893. This prediction was entirely falsified, for in all parts of the country wasps were conspicuously absent during August and September. My own observations on this point were conducted in Surrey, Hampshire, Norfolk, Hertfordshire, and Kent, and I am informed by friends that the same was noticeable in Scotland, Lancashire, and Somersetshire. These facts seem to me sufficiently conclusive of the truth of my former conclusion, and I should esteem it a favour to be allowed to invite information from any of your readers whose experience may perhaps, during the past year, have furnished further evidence in the same or the opposite direction."

Models of Cells

Prof. A. L. Herrera has recently published in the *Memorias y Revista de la Sociedad Cientifica 'Antonio Alzate,'* Mexico, 1897, two interesting essays, in which he describes some attempts of his to make working models of the impact of forces upon cells and protoplasm. He points out that in the part of physiology dealing with the elaborate mechanisms of higher animals the construction of models, such as those to illustrate the flight of insects or the action of the valves of the heart, has been useful; and he attempts to apply the same principle to the fundamental phenomena of protoplasm. To a certain extent he has been anticipated by Bütschli and others, and we are bound to admit that his working models are of coarser texture and apparently less adapted to the delicate reaction of protoplasm than the oil foams of his predecessors. None the less, many of his experiments are interesting and ingenious, and may serve a useful purpose in kindergarten science. In the first essay, entitled 'Los Infusorios Artificiales,' he tries to explain vibratile movements of cilia by means of elastic tubes containing diffusible liquids and placed in other liquids. His idea appears to be that osmosis currents between protoplasm and water and the stresses produced in the elastic cell-wall set up the vibrations. In the second essay, written in French, he describes a series of experiments showing the reaction of elastic spherical bodies to pressure by the elastic surfaces. He obtained a number of results strikingly resembling known animal and plant forms. We commend his essays to the curious.

A New Scientific Serial from Jamaica

As we briefly announced last month (p. 351), Jamaica furnishes us with one more promising infant in an over-populated world of scientific literature! To such a new-born child it can scarcely be said, "weeping thou sat'st while all around thee smiled," for, while the infant chuckles, distracted naturalists shed inky tears. It might sound rude to say that all such babes must come to the workhouse. To the house in which the specialist works sooner or later they have to come. If every name in the atlas of the world insists upon having its own separate representative in serial scientific literature, the wasteful dissipation of energy will increase in a lamentable degree. The diligence of the student will be more and more exhausted in a vain attempt to garner all the scattered fragments of information, which may or may not be of value, concerning each strictly limited branch of enquiry. It is, therefore, only with a moderate rapture of welcome that we can greet this first number of the *Annals of the Institute of Jamaica.*

The opening number is entirely devoted to a list of crustaceans. Faunistic catalogues are not unfrequently a weary waste of misapplied industry. They often contain no guarantee whatever that the author knows what he is writing about. When the identifications are original, they are as likely as not to be wrong; when they are borrowed, they are not very unlikely to be the endorsement of some ancient error. Miss Rathbun's "List of the Decapod Crustacea of Jamaica" stands on a different footing, because she happens to combine with a very exact knowledge of the objects catalogued a full and accurate acquaintance with the literature of the subject. The list, therefore, is a critical list, and great confidence may be placed in the names and synonyms and geographical distribution of species which it records. But it also contains notes and descriptions of independent importance. Some of these are quoted at full length, though without marks of quotation, from earlier papers, while others contain corrections of previously published opinions. Surely in the interests of science the repetitions would have been better omitted, and still more surely in the interests of science the corrections would have better appeared in the *Proceedings* which published the original statements.

While placing the highest value on Miss Rathbun's knowledge and acumen, we cannot always accept her decisions on points of nomenclature. The name *Stenorynchus seticornis* (Herbst) should stand, whether Slabber were right or wrong in stating that his specimen came from the East Indies. He appears to have kept his East and West Indian crabs together, and may have made some confusion, and if not, as the present list shows, the same species of crab may occur in both the Indies. In naming *Uca heterochela* (Lamarck), the change from Lamarck's more scholarly *heterochelos* is unneeded, since the Greek ἑτερόχηλος applies equally to the masculine and feminine genders. In dealing with the genus *Palaemon*, Miss Rathbun unhappily relies on Latreille's 'Considerations générales' of 1810, a book with a long-winded title too troublesome to quote, a book crowded with definitions that don't define, and ending with a list of types that are not described.

A Year-Book of Agriculture

The Year-Book of the U.S. Department of Agriculture for 1896 has just reached us, and set us wondering why we have not a Department of Agriculture manned by scientific experts, which might issue each year for the benefit of farmers and others interested in the subject as much useful matter as is contained in the six hundred odd pages of the Transatlantic publication. The history of the year-book is this. It is the successor of the Agricultural Report which, in its original form, was made up almost wholly of business reports for the use of Congress. When, however, it began to circulate more freely among farmers, papers on agriculture, and discussions on the results of scientific investigations were introduced, and it gradually became more and more a popular report, business and executive matter being reduced to the smallest possible proportion, till finally it was decided (in 1895) to issue it in two parts—viz. (1) an executive and business report, (2) a volume of papers "specially suited to interest and instruct the farmers of the country," and to include also "a general report of the operations of the Department for their information." This second part is the Year-Book, and the one now before us is the third of the series. It is published in an edition of 500,000 copies for free distributing, and is therefore, as the assistant secretary remarks in the preface, "in many respects unique." Following the report of the Secretary, which occupies nearly fifty pages and is eminently suggestive, are thirty papers (filling 500 pages) contributed by nearly as many scientific expert members of the staff. The 164 figures and six plates are a useful addition. Thus the value of a paper on some common poisonous plants is much enhanced by very passable pictures of the poison ivy (*Rhusradicans*), water hemlock (*Cicuta maculata*), death cup (*Amanita phalloides*), and others. The same applies to some remarks by Mr Herbert Webber on the influence of environment on plant varieties. Enumeration of some of the titles will give an idea of the wide scope of the Year-Book:—extermination of noxious animals by bounties—potash and its function in agriculture—the country slaughter-house as a factor in the spread of disease—the blue jay and its food—migration of weeds—diseases of shade and ornamental trees—care of dairy utensils—and finally one by M. E. Tisseraud, Councillor of State and Director of Agriculture of France, entitled, An Ideal Department of Agriculture and Industries, which is reprinted from a report of a House of Commons committee on the establishment of a Department of Agriculture and Industries for Ireland! An appendix of one hundred pages includes an account of the organisation of the Department of Agriculture, copious statistics of crops, exports, imports, and numerous items of useful information.

An Ideal Agriculture Department

We make no apology for remarks on Agriculture, which is only practical Natural Science on a large scale, pursued for the best possible object—the benefit of the race. In the paper to which we have just referred M. Tisseraud raises certain points which are worthy of emphasis. The agriculture of Europe, like an old and leaking ship, tossed and buffeted on a sea of breakers, needs, to save it from foundering, to be steered by abler hands and navigated by pilots who will join to a thorough practical training a profound and extensive knowledge—scientific knowledge. Hence the need for a Ministry of Agriculture; not a sort of Providence on which all may lean, and which by a series of miracles can supply remedies for all evils, nor a central authority which shall absorb all services and assume the functions of private individuals and voluntary associations, but an authority which shall tend to awaken the spirit of initiative and independence, and to stimulate and develop it among the agriculturists themselves. M. Tisseraud refers to the methods of control, in France, where by co-operation with local societies and associations, the encouragement of private agricultural schools, and the establishment, with the assistance of skilful farmers, of from 3000 to 4000 'champs de demonstration' each year, the State is able to accomplish an enormous amount of good with a very little expenditure.

Another task of a ministry of agriculture is to induce the most eminent scientists to occupy themselves with agricultural questions. It "must not be niggardly in its encouragements and subsidies to such men, for their discoveries will repay, with large interest, the expenditure which may have been incurred to enable them to carry on their researches." By this means there will be prepared for agriculture "an élite of men to direct it in the way of progress and of the application of science." The Minister, in the nature of things as at present constituted, must be a politician, but not so the men at the head of the different divisions of the department, who should unite to a great experience of administration profound technical knowledge and an incontestable authority in the questions with which they have to deal, and whose office should be of a permanent character. Such a director must have the choosing of the staff placed under his orders. He must be their master, and be able to reward those who show merit and zeal, and to remove or punish those who cannot or do not properly discharge their duties. "Let the director himself be absolutely responsible for the good working of his department, and let him be replaced if he proves inefficient." Finally, M. Tisseraud suggests the appointment of scientific counsellors selected from the most distinguished agriculturists and men of science, and technical committees of professional men, specialists, and practical experts, from whom the Minister may obtain "trustworthy advice and indispensable light for rightly seeing, and judging, and forming in full security the decisions which concern the department over which he presides."

Catalogue of Fibre Plants

Another excellent specimen of the work of the United States Department of Agriculture comes to hand almost while writing the above. It is a descriptive list of useful fibre plants of the world by C. R. Dodge, special agent in charge of fibre investigations. More than a thousand kinds are enumerated under the botanical names of the plants producing them, while common and native names are also included in the alphabetical arrangement. The book is a large octavo of 360 pages; there are more than one hundred figures in the text, and thirteen excellent photographic plates showing the habits of the various plants. Much information is given about the more important kinds, including the structure and properties of the actual fibre, the source and method of cultivation of the plant, the preparation of the fibre, and references to the literature. The great number of native names possessed by some species is evidence of their long use. New Zealand flax (*Phormium tenax*), a liliaceous plant, has nearly sixty native names. Captain Cook, who first brought it to the notice of Europeans, found it in common use among the aboriginal New Zealanders; he speaks of it as "a grass plant like flags, the nature of flax or hemp, but superior in quality to either, of which the natives make clothing, lines, etc." Mr Dodge gives in a useful introduction a general account of the history, chemistry, and structure of fibres, and suggests also a classification. His system of arrangement serves to show what very various parts of the plant are used for the purpose; for it includes not only true fibres derived from the wood or bast tissue respectively, but whole stems, roots or leaves, or split and shredded leaves, as well as external hair-like growths, such as cotton, and also a class of "pseudo-fibres." The last comprises certain mosses, like *Sphagnum*, used for packing, seaweed wrought into lines or cordage, and the mycelium of certain fungi. The book will be a welcome addition to the libraries of individuals and institutions interested in economic botany. To enhance the value of future editions, the author requests notes or further information on any fibre plants, and especially photographs of foreign species.

Wing Neuration in the Lepidoptera

Mr A. Radcliffe Grote has been insisting both in German (*Ill. Wochenschr. f. Entomologie*, band II., no. 28), and in English (*Entom. Record*, vol. ix., no. 10) on the advantage of employing photography in the illustration of the wing-nervures of moths and butterflies, as by this means only can absolute accuracy be ensured. British entomologists will be specially interested by Mr Grote's severe strictures on Mr Meyrick's descriptions and drawings of wing-neuration in his revision of the Geometridae (*Trans. Ent. Soc.*, 1892) and his recent "Handbook of British Lepidoptera." Comparison of Mr Meyrick's figures with the photographs have led Mr Grote to the conclusion that in the former "the distances, relative direction, and at times the point of origin are frequently all wrong. Worse than this, Mr Meyrick supplies nervures which have no existence . . . and omits nervures . . . which are distinct in nature." We await with interest Mr Meyrick's reply to this criticism.

I

The Problems of the British Fauna

IN the current volume of *Natural Science* (pp. 223-4) appeared a short editorial comment on my friend Dr R. F. Scharff's paper on the Origin of the European Fauna.[1] The Editor has misunderstood Dr Scharff's views in several particulars, and has nevertheless expressed the fear that his "speculations will prejudice the use of zoological distribution in geological investigations." As the problems raised are of great interest to all naturalists, a further examination of the subject may perhaps be allowed. It is somewhat unfortunate that the present writer approaches the question from the same standpoint as that of Dr Scharff—zoological geography. But it is to be hoped that some of the special students of our Pliocene and Pleistocene deposits will, in due course, favour us with their criticism.

The problems suggested by the fauna and flora of the British Islands appeal in a marked degree to naturalists who live in Ireland, especially if those naturalists happen to be English immigrants to the sister isle. A botanist or a zoologist who has grown up in the south of England, and has then transferred himself to Ireland, is struck by the absence of many of his familiar wild friends, and the presence of many forms of life hitherto unknown to him as British species. The peculiarities of the Irish flora, such as the occurrence of Pyrenean saxifrages and Mediterranean heaths in western Ireland, have long been familiar to naturalists, and are discussed in the classical memoir of Forbes.[2] It may be well, however, to recall a few of the corresponding facts regarding the fauna. The student of vertebrates notices the absence, for example, from Ireland of the Common Hare (*Lepus europaeus*), the Voles, the Mole, the Weasel, the Polecat, the Nightingale, and all reptiles except the Viviparous Lizard. The entomologist misses such conspicuous insects as the Stag-beetle (*Lucanus cervus*), the great water beetle *Hydrophilus piceus*, and the Large tortoiseshell butterfly (*Vanessa polychloros*). These are representatives of a group of animals to which the present writer has applied the term "Teutonic fauna,"[3] while Dr Scharff, in his recent

[1] *Proc. R. Irish Acad.* (3), vol. iv., pp. 427-514. An excellent summary appeared in *Nature* of October 28th, 1897.
[2] *Mem. Geol. Survey Gt. Britain*, vol. i., 1846.
[3] *Museums Assocn. Report*, 1894.

paper, designates the mammals among them as distinctively 'Eastern' or 'Siberian.' The absence of these Siberian mammals from Ireland may perhaps be regarded as the central fact on which his views concerning the British fauna are founded. Most of the animals of this group die out in Great Britain as one travels north or west. It must be specially noted, however, that the mammals range over the greater part of the island. The Common Hare extends from Cornwall to the shores of the Pentland Firth; the Weasel and the Viper range far north into Scotland. But most of the corresponding invertebrates are not found north of the Trent or west of the Severn.

The naturalist in Ireland is compensated for the loss of this eastern fauna by the presence of two most interesting and distinct sets of animals, almost unrepresented in the south-east of England. It has been mentioned that the Common Hare is absent from Ireland, but the Varying Hare (*Lepus variabilis*) occurs all over the country, from north to south, both on the hills and in the plain. This is a typically arctic and alpine animal, with a complete circumpolar range, confined in Great Britain to the Highlands of Scotland. Quite a number of insects, which in Great Britain are to be found only in the north range to the extreme south of Ireland, such as the marsh ringlet butterfly (*Coenonympha typhon*) and the ground-beetles *Carabus clathratus* and *C. glabratus*. But perhaps the most striking example of this northern fauna is the ground-beetle *Pelophila borealis*, which has been found in most of the northern and western counties of Ireland, from Antrim to Kerry in the far south-west. This beetle is, so far, unknown on the mainland of Great Britain, but it occurs in the Orkneys; on the continent it is an inhabitant of high northern latitudes. Together with this arctic and alpine group may be mentioned the three species of North American fresh-water sponges, *Ephydatia crateriformis*, *Heteromeyenia Ryderi*, and *Tubella pennsylvanica*, which Dr Hanitsch[1] has lately described from lakes in western Ireland. These are comparable to the few North American plants which grow wild in the same districts. One or two of the plants have Scotch stations; but both plants and sponges are unknown on the continent of Europe.

The second characteristic group of the Irish fauna—like the peculiar plants of the western counties, the Arbutus, London Pride, and St Dabeoc's Heath—shows striking affinity with the life of south-western Europe and the Mediterranean region. Forbes, in his memoir already referred to, expressed the opinion that no fauna corresponding to this Hibernian flora exists in the British Isles. Everyone, however, agreed in assigning to this type the Portuguese slug, *Geomalacus maculosus*, when it was discovered spread over a small area in counties Cork and Kerry. Recently a number of

[1] *Irish Nat.*, vol. iv., 1895, pp. 122-131.

animals have been recognised which undoubtedly show similar faunistic relationships. Some of these are new discoveries, and are apparently confined to Ireland, such as the millipede *Polydesmus gallicus*,[1] the earthworms *Allolobophora veneta* and *A. georgii*,[2] and the weevil *Otiorrhynchus auropunctatus*.[3] Others have long been known as British animals, and their occurrence in the west of Great Britain as well as in Ireland has probably caused their faunistic import to be overlooked. Such are the snail *Helix pisana*, the wood-louse *Platyarthrus hoffmanseggii*, the ground beetle *Eurynebria complanata*, and the weevil *Mesites tardyi*. Some of these animals are found both in the east and west of Ireland, others only on the east coast. In Great Britain they occur mostly in the south-west, but the last-named is an example of a section which ranges northward into western Scotland. Abroad all are characteristic of southern and south-western Europe, while several are found in the Azores, Madeira, and Canary Islands. It is most important to take the foreign range into consideration when assigning animals to a distributional type. Just as the Common Hare is spread far to the north in Great Britain, as compared with many other members of the 'Siberian' fauna, so a number of animals belonging to the South-western fauna range farther to the east than the more typical species of the group. For instance, the slugs of the genus *Testacella* must be referred to the South-western section, when we consider the general range of the genus, though some species occur in our eastern counties. And Dr Scharff would add to this fauna many of our widely distributed species—the Bullfinch among birds, for example.

He believes moreover that this South-western fauna merges gradually into a 'South-central' fauna, including the Badger among mammals, and *Helix virgata*, *H. acuta*, and *H. nemoralis* among snails. And to these combined southern groups he is inclined to ascribe the bulk of the Irish animals, even those with a wide range both in Ireland and in Great Britain; except a few species that may have invaded the country since it became an island—such as the white butterflies, *Pieris brassicae* and *P. rapae*, and the 'painted lady,' *Pyrameis cardui*, or that have been apparently introduced by man—such as the rats and the house mouse.

A few remarks are necessary regarding the relative ages which are to be ascribed to these different sections of the British fauna. The South-western group, the most typical members of which are found in the most remote parts of the country, with ranges often discontinuous, are evidently the oldest. The 'Siberian' animals, which

[1] R. I. Pocock, *Irish Nat.*, vol. ii., pp. 309-312. [2] H. Friend, *id.*, vol. v., pp. 69-73.
[3] G. H. Carpenter, *id.*, vol. iv., pp. 213-218.

are confined as a rule to eastern and south-eastern England, and none of which have been able to reach Ireland, are clearly the newest. The Northern animals must therefore come between these two in regard to the time when they entered our area. It will be remembered that Forbes, when discussing the distributional groups of British plants, regarded the most western ('Hibernian') flora as the oldest, the 'Germanic' flora as the newest, and the arctic and alpine flora as of intermediate age. Forbes, however, considered the plants of general British distribution to have entered the country subsequently to the arctic and alpine species. And as he observed that there is a gradual transition from the most typical 'Germanic' to the most widely-spread 'British' type, he regarded all the immigrants since the Northern flora—that is to say the 'British,' 'English' and 'Germanic' types of Watson,[1] as belonging to one great central European flora, some of whose members have spread much more widely in our islands than have others. Forbes, moreover, separated two small groups of plants, one typical of Cornwall and Devon ('Norman' flora), the other characteristic of the chalk districts of south-eastern England ('Kentish' flora), which he believed to be entirely distinct from the recent Germanic flora. To these small sections he ascribed an age between that of the South-western and that of the Northern flora.

Dr Scharff's estimate of the relative ages of the sections of the British fauna differs from Forbes' view of the ages of the corresponding sections of the flora in one important particular. While Forbes placed the bulk of our widespread plants later than the arctic and alpine species, Dr Scharff considers that—at least as regards the species found in Ireland—the vast majority of the animals are of southern origin, and not more recent than the arctic and alpine species. As mentioned above, he believes that there is a gradual transition from animals of the most typical 'Hibernian' type, such as *Geomalacus maculosus*, to such widespread animals of his 'South-central' group as the Badger and the Fox.

The question of the exact geological period during which each section of the fauna entered the British area, and by what route the animals reached our territory, must now be considered. With regard to the flora, Forbes believed that the Hibernian plants lived on a now sunken Atlantis in Miocene times, and reached their present Irish and Iberian stations from the west before the Ice Age. The Cornish and Norman floras were supposed to have come into the country from the south-west or south—of course across the dry area of the Channel—also before the Ice Age. The arctic and alpine plants, Forbes naturally thought to be the relics of the Glacial Period itself. And he believed the rest of the British flora—the

[1] "Cybele Britannica," London, 1870.

'British,' 'English' and 'Germanic' types—to have entered the country from the east and south-east across the dry area of the North Sea and the Straits of Dover during the subsequent period when the British territory had emerged from the Glacial sea, England being united to the Continent, and Ireland to Great Britain.

At the time when Forbes wrote, the glacial deposits were believed to have been laid down on the bed of a sea covered with floating ice. The subsequent adoption by the majority of geologists of the theory that the Boulder Clay represents the ground moraine of vast sheets of land ice has led most recent writers on the British fauna and flora to regard most if not the whole of the living things in our area as post-glacial immigrants. Whatever animals and plants lived in these islands during Pliocene times are presumed by Professor James Geikie, and those who share his views, to have been exterminated by the terrible rigour of the glacial conditions during the Pleistocene age. And the general view at present is that it was not until the climate improved in later Pleistocene times that the country again became the abode of animal and vegetable life. On this theory it would seem certain that the arctic and alpine species were the first to establish themselves in our area.

Now, the results to which Dr Scharff's studies have led him are in startling opposition to the current opinion just mentioned. He believes that, with the exception of the 'Siberian' section, the whole of the British fauna entered the country in Pliocene or the earliest Pleistocene times. With regard specially to the Irish fauna, he considers that all the animals which now inhabit Ireland must have passed into that island in the Pliocene, or, at latest, about the opening of the Pleistocene period, there being, in his view, no evidence of any land-connection between England and Ireland after that date. It is hardly necessary to recall the fact that the absence of so many British animals and plants from Ireland has led naturalists without exception to regard that country as an older island than Great Britain, whatever geological age they may ascribe to the fauna and flora.

It is specially the study of the past and present distribution of the British mammals that has led Dr Scharff to his results. The 'Siberian' mammals which are found—living or extinct—in Great Britain, but not in Ireland, furnish, as has been said, the key to his argument. Remains of these mammals, preserved in the continental Pleistocene deposits, enable the course of their migration from east to west to be traced in considerable detail. They lived in Siberia in Pliocene times, but in Europe their remains are not found except in beds later than the Lower Boulder Clay, which Dr Scharff suggests was laid down in the northern part of a sea connecting the

Caspian and Aral with the White Sea and the Baltic, thus forming an effectual barrier to the westward course of the mammals. The existence of such a sea is supported by the presence of arctic forms of life in the Caspian, and the occurrence of the Caspian mollusc *Dreyssensia polymorpha* in the Lower Boulder Clay of Germany. As this central European sea was replaced in part by a land surface, the way was opened for the Siberian mammals to pass on into western Europe. Now we are confronted with the startling fact that the British deposit in which these mammals first appear is the Forest Bed, usually considered the newest member of the Pliocene series. Are we to suppose, Dr Scharff asks, that the animals made their way into England by Asia Minor, Greece and Southern Europe, and so reached our shores before Central Europe was open to them? That part of the older southern fauna—the 'South-Central' section—travelled into Western Europe by this route from Siberia during Pliocene times he does believe. But, he argues, it is impossible that the true 'Siberian' animals could have passed that way, seeing that their remains are entirely absent from South European, as well as from Irish, Scottish and Scandinavian, deposits. He is therefore driven to the conclusion that the Forest Bed and other British deposits usually classed as Newer Pliocene must be considered as rather later than the Lower Continental Boulder Clay, and reckoned to be of Pleistocene age. In support of this correlation he also brings forward the presence of arctic shells in the newer crags.[1]

Having thus fixed the period when these Siberian mammals appeared in England, Dr Scharff believes that he has obtained the latest possible date for the 'last link' of the land-connection between England and Ireland. For if the way into Ireland remained open long after these mammals reached English territory, what can have prevented their onward course to the western island? The wide range of the mammals as compared with the restricted range of the invertebrates of the same faunistic section has been dwelt upon in the opening part of this paper. It is certain that the vast number of widespread invertebrates that inhabit Ireland as well as Great Britain must have passed over the Irish Sea when it was a lake and river valley, or crossed the later northern isthmus which joined northern Ireland to south-western Scotland. But as the Siberian mammals were kept out of Scotland by the Pleistocene sea, this northern isthmus may be left out of reckoning as far as they are concerned. If the slowly-moving army of spiders, beetles, snails

[1] The reader is referred to Dr Scharff's paper for the numerous references supporting these positions. It will be seen that the editorial statements of Dr Scharff's views (*supra*, p. 224, "that the lower continental boulder clay is Pliocene . . . that the Siberian mammals migrated into Western Europe to the south of this sea") convey the exact reverse of the opinions really advocated by the author.

and slugs did not invade Ireland until after the Siberian mammals were in England, why were the latter unable to reach Ireland as well? Such, briefly, is Dr Scharff's argument for the pre-Glacial immigration of the Irish fauna.

Turning to the Northern section of the fauna, Dr Scharff argues that it must have entered Scotland by a land-connection from Scandinavia, and so passed southwards into Ireland. This land-connection he believes, in common with most geologists, to have been continued northwards to Spitzbergen, and westward to Greenland and North America. Thus a way was open for animals with a circumpolar range to wander southwards, while North American forms were able to invade Western Europe. The continuous coast-line to the north of the Atlantic, shutting off that ocean from the Arctic Sea, must have ensured a mild climate to its waters and shores. The vast majority of geologists would, of course, regard this land-connection and the migrations which passed over it as post-Glacial. Dr Scharff, necessarily considering the northern fauna older than the Siberian, believes, on the contrary, that its entry into our area must be put back to the time when the ice-laden sea of the Lower Boulder Clay covered Central Europe and the newer crags were being laid down in eastern England. The land-connection between Scandinavia and Ireland he considers, however, to have persisted into late Pleistocene times.

It has already been mentioned that the bulk of the Irish fauna is supposed by Dr Scharff to have come from South-western and South-central Europe, and that the more western section is regarded by him (as the corresponding section of the flora was regarded by Forbes) as the oldest section of the whole British fauna. Dr Scharff does not share Forbes' view of an extensive Atlantic continent; he believes that a western continental coast-line, including, of course, a tract to the west of the present British and Irish area, meets all the requirements of the facts. Across the valleys which occupied the present beds of the English and St George's Channels the animals of these southern migrations passed into Great Britain and Ireland; according to Dr Scharff, through the Pliocene and up to the earliest Pleistocene period. But the land-connection between Great Britain and Ireland broke down in the south sooner than in the north, so that the arctic migration could go on after the southern migrations had been cut off. Some of the animals of the 'South-central' migration are traced by Dr Scharff back to Siberia, where he believes they originated. He points out, moreover, that the same species can sometimes be proved to have taken part both in the 'South-central' and in the (later) true 'Siberian' migration. In such cases, however, a distinct race of the species usually characterises each migration. For example, the Irish race of the Red Deer is the small-antlered form which can be traced, by its remains in south European beds, from Western Asia into Greece, and "along the borders of the Mediterranean, at the time when Corsica and Sardinia were still connected with Sicily and Greece on the one hand and with Tunis on the other." In this way it is suggested that animals from western Asia and south-eastern Europe found their way to the western edge of the continent, while the central European plain was still covered by sea.

If Dr Scharff's views as to the geological periods during which the British fauna entered the country be accepted, it follows that the vast majority of our animal population must have survived the rigours of the Ice Age; as regards Ireland, the whole fauna (except comparatively modern immigrants) must have lived in the area from a time before the deposition of the British Lower Boulder Clay. It will be remembered that Forbes, who believed the distinctive South-western flora to be pre-glacial, suggested that the plants survived in a sunken land to the south-west. Dr Scharff, however, rejects the idea of such an asylum for the fauna on the ground that the south-western corner of Ireland is remarkably poor in species, many forms of life, common throughout the rest of the island, being absent from the peninsulas of counties Cork and Kerry; for example, the Helices of the sub-genus *Xerophila*. He insists that portions at least of the present Irish area must have been able to support the present animal population throughout the Pleistocene period.

Those geologists who adopt the extreme view of the glaciation of Ireland, advocated by the Rev. M. H. Close, and accepted by Professor Hull,[1] will naturally reject Dr Scharff's conclusions with decision, if not with derision. For, according to the opinion of this school, an ice-sheet of great depth covered the whole country. It is needless to say that Dr Scharff rejects with equal decision the existence of such an ice-sheet. In the closing section of his paper, he expresses his agreement with those geologists who believe that the Boulder Clay was formed in an ice-laden sea, and not as the ground-moraine of vast glaciers. Of course, this view requires the submergence of much of the country. But, recalling the opinion of several geologists that the western margin of the British area stood higher in relation to the eastern during the Glacial Period than now, Dr Scharff reconstructs the physical geography of our islands during that time of greatest submergence, which left shell-bearing gravels on the Dublin mountains and Moel Tryfaen. According to his map, the Scottish highlands, the Hebrides, and northern, western, and southern Ireland formed a peninsula still continuous with Scandinavia; the Scottish lowlands and northern England were an

[1] "The Physical Geology and Geography of Ireland," London, 1878.

archipelago; central and southern Wales an island; while the south and midlands of England were joined to France by an isthmus. The sea covered nearly the whole of eastern England, and stretched across north Wales, and over eastern and central Ireland. Zoological evidence for this transgression of the northern sea over eastern Ireland is found in the distribution of the arctic marine crustacean *Mysis relicta*, which forms part of the 'relic fauna' not only of the Swedish lakes, but also of Lough Neagh. The sea separating southern and central England from Scotland, as well as from Ireland, checked the northern as well as the western progress of the 'Siberian' mammals. None of these animals are found fossil in Scottish Pleistocene deposits, though the recession of the glacial sea has in recent times opened a way to the north, of which the surviving species have availed themselves. But meanwhile the isthmus between Scotland and Ireland had become broken through.

Having thus put forward a summary of Dr Scharff's views as to the ages and paths of migration of the various sections of the British fauna, I venture, with some diffidence, to offer a few observations and suggestions. I entirely agree with Dr Scharff in considering the South-western as the oldest section of our fauna, and I have no doubt that it came into our area long before the Glacial Period. The North American plants and animals seem to me to be more ancient than Dr Scharff is inclined to admit. He classes them with the general Northern fauna, but I believe that their very restricted and discontinuous ranges along the extreme western margin of Europe mark them as decidedly older than those northern animals and plants which have a general circumpolar distribution.

Study of the distribution of British insects shows that there is a Southern fauna[1] distinct from the South-western, in that its members occur generally in southern Britain, as well as in Ireland and western Britain, and have a wide continental range. It is clearly newer than the South-western fauna, yet the fact that it is confined in Ireland to the south and west suggests that it is of considerable geological age. Along the west coast of Ireland the insects of this group often range some distance to the north, and their general British distribution around the west and south of our islands renders it likely that they held the country west and south of the area where the Glacial deposits were being formed, and have, since the retrocession of the agent which produced those deposits, been unable to spread far eastwards in Ireland or northwards in Great Britain. This fauna may safely be regarded as comparable to Forbes' Norman and Kentish floras, and older than the Arctic fauna. As yet, however, I am not prepared to accept so great an antiquity for the bulk of

[1] Examples of this group are the ground-beetles, *Carabus cancellatus* and *Panagaeus crux-major*, the butterflies *Gonepteryx rhamni*, and *Leptida sinapis*, and the moths *Zeuzera pyrina* and *Stauropus fagi*.

the widespread Irish animals as Dr Scharff claims by referring them to his 'South-central' group.

I quite agree with Dr Scharff in rejecting the theory that the whole of our fauna is post-Glacial, since that theory would require us to regard the Arctic animals as the oldest, whereas the distributional facts require us to consider the South-western section the oldest. But it seems to me that we are equally bound to consider the animals of the Northern fauna—restricted as they are to the hill regions and the west—as more ancient than the widespread species which form the dominant element in our fauna to-day. I am quite prepared to believe that many of these widespread species inhabited the southern part of our area throughout Pleistocene times, but it seems unlikely that they extended their range far to the north or west until the glacial conditions had passed away. Dr Scharff apparently believes that, the glacial deposits being due to a marine submergence, sufficiently extensive land tracts must have been left to enable the whole fauna to survive. But even many geologists who reject the theory that the Boulder Clay is a ground moraine, consider that the polished and scratched rock-surfaces beneath that deposit are evidences of a former extension of land-ice.

In the opening paragraphs of his paper, Dr Scharff makes the suggestive remark that the study of the fauna of a single island is the best starting-point for the study of a continental fauna. Hence he takes Ireland as the key to the greater problem of Europe. It seems likely that considerable light would be thrown on the special British problem by one of the smaller British islands, and I believe that in the Isle of Man we have evidence of a post-Glacial land-connection between Ireland and western England. Professor Carvill Lewis[1] and Mr Percy F. Kendall[2] found traces of glaciation up to the summit of the highest hills in the island, the former remarking that the whole shape of Snaefell is that of a 'roche moutonnée.' Whether we believe with these geologists that the 'Irish Sea glacier' passed over the summit of Snaefell, or prefer to consider the high-level drifts, boulders, and striated rock-surfaces as evidences of an ice-laden sea, it seems equally certain that the present inhabitants of Man must have reached that isle since the climax of the Glacial Period.

Now the fauna of the Isle of Man resembles on the whole that of Ireland, western England, and Wales. Its cliffs form the most northern station for certain species of moths, such as *Dianthoecia luteago* var. *barrettii*, *D. caesia* and *D. capsophila*, some of which are scattered along the western British and the eastern and southern Irish coasts as far as Land's End and Dingle Bay. If the Isle of Man could not have supported any fauna during the height of

[1] "Glacial Geology of Great Britain and Ireland," p . 357-9. [2] *Op. cit.*, pp. 433-4.

the Glacial Period, we are forced to the conclusion that its shores must, since then, have formed part of the northern coasts of a gulf opening to the south, down St George's Channel. As the climatic conditions improved, I believe that many animals of the old South-western fauna—such as *Helix pisana*, *Eurynebria complanata*, *Otiorrhynchus auropunctatus*—which had doubtless lived to the south of England and Ireland from Pliocene times, were able to make their way northwards along the shores of this ever-widening gulf to their present stations on the eastern Irish and western British shores. North of this gulf, I believe that the vast majority of our present widespread species passed from north-western England into Ireland, where they have spread from east to west. The difficulty raised by Dr Scharff that the 'Siberian' mammals were in England, and should have passed over to Ireland with the rest, is doubtless serious. But these mammals were kept out of Scotland until recent times, and they may well have been kept out of north-western England by an arm of the sea until the Irish land-connection had broken down. One of them, the English Hare, inhabits the Isle of Man, showing that the barrier which confined them to the east had been removed in time for that one species to spread so far, though not as far as to Ireland. The fact that the other mammals of the group—such as the Voles and the Mole—are absent from the Isle of Man proves that the Hare must have made the most of her chance to spread north-westward.

While, then, I find myself in almost complete agreement with Dr Scharff with regard to the older sections of our fauna, I think that those widespread species which survived the Glacial Period must have been confined to the more southern parts of our area, and have only subsequently spread northwards and westwards to Scotland and Ireland. Doubtless the speculations of the extreme glacial school regarding the total extinction of all life in our countries in Pleistocene times need revision in the light of the past and present distribution of species. At the same time there seems enough agreement among those who have specially studied the drift deposits to warn students of animal distribution that the conditions over much of the British Islands must have been unfavourable to the presence of a rich flora and fauna.

But in any case it seems to us a necessity to believe that a considerable proportion of the British flora and fauna did survive the Glacial Period in our area, or in the now submerged tracts adjacent thereto. Readers of *Natural Science* will doubtless recall Mr G. W. Bulman's paper,[1] in which a plea was entered for the pre-Glacial age of our animals and plants on the ground that no geological evidence of an elevation subsequent to the Ice Age could

[1] Vol. iii., pp. 261-6.

be brought forward. Neither of the rival glacial theories requires belief in the annihilation of all living things in our area. On the land-ice hypothesis there must have been a now submerged tract bordering on the Atlantic, and stretching beyond the present south coast of Ireland, which the late Professor Carvill Lewis recognised as an unglaciated area. A similar elevation to the west of our present British islands is believed to have accompanied the submergence by which the other school of geologists explain the Pleistocene deposits. And it is generally agreed that the south and, in part at least, the midland areas of England were free from glacial conditions. When we remember how distinctly temperate and even sub-tropical forms of life can be found to-day close to areas of glaciation, it must be admitted that there is no impossibility in the suggestion that the ancestors of the older plants and animals which we now see around us witnessed in our territory the coming and passing away of the age of ice.

GEO. H. CARPENTER.

II

The Provincial Museum

WHAT may fitly be called the provincial museum question has of late rapidly assumed considerable importance. The need for these institutions or the enthusiasm of their staff is not called in question; as a matter of fact, it is in a great measure to the energy of provincial curators, as manifested at meetings of the Museums' Association, that much of the present awakening is due. The evils from which these museums suffer are acknowledged to be a general lack of means, undermanning, and a partially or wholly untrained staff. The widespread attention which the subject receives even from the lay mind is evidence of a knowledge that better things are possible, and that a satisfactory settlement will tend to the public good and educational progress. The museum question has hitherto been attacked in two ways. The late Dr Goode in America, and Sir W. H. Flower in this country, have laboured alike to educate the public mind to the value and necessity of these institutions as factors in education, and to direct and stimulate museum workers. The second form of attack has been made by Professor Petrie (Brit. Assoc., Liverpool), and more recently by the editorial comments of *Natural Science* (Vol. xi., No. 66, Aug. 1897). It may be defined as the suggestion of remedial measures. The addresses and papers of Sir W. H. Flower and Dr Goode have undoubtedly done much to pave the way for better progress, but before the remedial measures which are now advocated can be made effective or adequate to the needs of provincial museums, it will be necessary to consider fully their present position as a whole.

Even a brief consideration of the provincial museum reveals much that is anomalous and unsatisfactory. Hardly any two can be said to work upon a common plan, whilst most develop and exist rather as the sport of circumstances than as the outcome of definite purpose and design. We much doubt if one can be pointed out which has an income at all equal to its needs, or which is able to develop and maintain its various sections according to their true value and proportion.

We find also that Government recognition is accorded to provincial museums in a vague and half-hearted manner. The Libraries and Museums Act can be put into operation if the people of a borough demand it, but the operation of the act is dependent upon the action of the very people who are most needing instruction by museum agency. Not infrequently the act is put into operation to gain a library, and a museum of ill-assorted material grows up with it, by a system of accretion rather than assimilation. Indeed, it is this accretionary growth for want of a defined plan which is the bane of provincial museums, and one which the curators themselves are at present powerless to prevent. Further, museums are recognised by the Educational and Science and Art Departments in that attendance at a museum by school children and science students is, under certain conditions, allowed to count as class attendance, and specimens are loaned to them from the Art collection of South Kensington.

It has followed from the operation of causes such as those indicated that a type of museum has arisen throughout the country which justifies the charge of ill-conceived, lacking in proportion, wanting in utility and inadequately supported and staffed. If we bear the present conditions in mind, and consider the proposed remedies, it is manifest at once that the utmost difficulty would be experienced in the attempt to create a federal staff of scientific experts on the lines which have been advocated by Professor Petrie. The museums are so unequal, their sections vary so strangely, both in ratio to one another and in their value, that the attempt to send to a series of museums a number of specialists, devoting equal amounts of time to each, would result in confusion and failure.

The first plan suggested in the editorial comment of *Natural Science* of August last is practically that of Professor Petrie, with the added proviso that the specialists should be resident, one at each museum, whilst the same round of visits was maintained. Other considerations apart, municipal jealousies would effectually kill the scheme.

The second plan is delightful by reason of its naïve character rather than its practicability. The visits of specialists from the Government museums are red-letter days to the provincial curator, valuable alike for the rare good fellowship meted out to the humbler brother in science, for the gratuitous work done, and the remarkable stimulus and enthusiasm imparted; but that the arrangement and scientific work of provincial museums should be left to the staff of Government museums, increased in numbers for the purpose, is a plan hardly likely to commend itself to the provincial curator or his governors, and even less likely to Parliament, which would need to be approached to decide the question.

There is much in each plan that is admirable, but after all they are in the nature of makeshifts rather than a solution of the question. What seems to be needed is a thorough grapple with the whole question of museum development, and (to borrow a sentence from *Natural Science*) of the means of "co-ordination, investigation, and effective utilisation of all our obscured scientific and artistic material," together with the creation of means by which those aspiring to take up curatorial work may receive a thorough training.

A somewhat rough-and-ready classification of provincial museums seems to indicate naturally a string of suggestions which might possibly be elaborated into a workable and effective scheme. At present provincial museums may, we think, be placed in three or four classes as follows:—

I. Museums of University Colleges, and of large cities possessing large collections and a trained staff.

II. Museums of important manufacturing centres, or county towns, with usually fair collections and a self-educated staff.

III. Museums of small towns, and of scientific societies which are managed by honorary curators, or once arranged and afterwards left unaltered in the hands of a care-taker.

IV. Museums under the Libraries and Museums Act which are managed by the librarian in addition to his own work, outside help being obtained from time to time. Some of these museums rank in importance with those of Class II.

That a relative importance of provincial museums exists such as that implied in this classification will, we think, be admitted, and upon this assumption the following suggestions are formulated as a means of bringing about that co-ordination and improvement of museum development which is so much desired.

Suggested Plan

I. Creation of an annual museums' grant by Government in aid of provincial museums of the first class, the sum allotted to each being determined by considerations similar to those which guide the application of the present University Colleges' Grant.

Upon the strength of such a grant the Government could charge each museum with a definite scope of work and the attainment and retention of a certain standard of excellence.

For example, a museum might be called upon as a condition of receiving the grant to have special aims such as the following:—

(*a*) The specific task of investigating and demonstrating by collections the natural history, &c., of a defined geographical area of which the museum would be the centre.

(*b*) The formation and continued progress of a good general collection suited to the needs of the area served by the museum.

(*c*) The attainment of a certain standard of excellence and completeness.

(*d*) An adequate staff.

Means could be devised by which certain of the museums in classes II. and IV. could be raised to the standard of class I., so that, speaking generally, the country would become parcelled out into areas, in each of which a museum was maintained, in constant good order, and ministering in a special as well as a general manner to the needs of the population.

II. Remaining museums might be subsidised through the agency of County Councils, upon certificates of efficiency and progress received annually from an accredited visitor, who might be an official of the Government or of one of the first-class museums.

Great help would be rendered to the curators of small museums if, as a condition of the subsidy, a member of the staff of the first-class museum had a seat and vote upon the committee. Such a post might well be honorary, and would be analogous to that which obtains in the case of certain Grammar Schools, a governor serving upon the board by appointment from one of the larger Colleges or Universities.

III. The small museums might be grouped around the larger according to locality and function, and by means of a nominal contribution to the funds of the larger have a claim for help and direction.

The advantages, if a scheme of this sort be applied to the whole country, might be dealt with at considerable length, but it will be less likely to confuse if we narrate briefly what we consider to be the chief.

Museums' aims, management, and development would become organised throughout the country, and lead at the outset to the abolition of a needless overlapping where several museums exist in a large town or city under different management.

More museums of the first class would be created, and the curators of county museums, and students generally, would find help nearer at hand than at present. The conditions of existence of the large museums would require that the systematic study of the fauna, flora, &c., of large districts be prosecuted with diligence and the salient features of each being demonstrated in the museum collections. The whole would necessarily become of prime importance to the public, a permanent memorial of the scientific value of the area served, and of great usefulness to the specialist and the nation.

The small museums might have a similar charge, though, of course, in a smaller and more localised degree, whilst being also required to maintain a good type-collection suited to the wants of the population.

Museums would be brought into direct communication with each other, and the transference of specimens to centres where they would be of most use would be immensely facilitated, while the small museums would naturally become agents of supply of material obtainable in their districts. The whereabouts of material for the specialist would be better known, and be more readily available.

The linking up and increased usefulness of provincial museums would provide a healthy stimulus to local scientific societies, would result in increased and more thorough field work, and do much to aid that federation of remote scientific workers which is so desirable. Moreover, it must not be forgotten that such a chain of museums offers the best means whereby the collections of the humblest and most distant worker might be conveyed to the one best fitted to deal with them.

The plan we advocate is crude, but designedly so, for elaboration provokes criticism of detail rather than of principles, and it is the latter which are all-important to determine in connection with the present phase of the provincial museum question.

HERBERT BOLTON.

III

Cell or Corpuscle?

ONE of the youngest and most vigorous among the sciences is that which has been named Cytology.

Its strength, in all probability, is due to the fact that it has sprung from a broad foundation, and that it still rests, not upon one, but many pillars of support. The botanist, the zoologist, the physiologist, divergent from one another as their daily walks unfortunately are, yet agree to join hands over the common basis of their sciences —the organic cell.

With such splendid results already gathered into its encompass and with the hope and promise of such a brilliant future before it, it is all the more to be regretted that the language of Cytology is not only in contradiction to common sense, but such that it must assuredly lead to endless perplexities.

The word cell, the very watchword of the science, is one that in this mouth means one thing, in that quite another.

Dealing with the lower plants, the botanist will speak of the 'swarmspores' as cells, whilst when he turns to the phanerogams he will apply the same term to the elements of cork or sclerenchyma, notwithstanding the fact that the former consist of protoplasm and nucleus without a cell wall, whilst the latter are composed of cell walls without either protoplasm or nucleus.

Such inexactitude as this, such a want of definite expression, must hang as a burden around the neck of the science, impeding its progress at every turn.

More especially is it from the side of the student of vegetable life that this confusion is felt, the zoologist, although he uses the word cell in a sense which stands in flat contradiction to its everyday meaning, yet attaches a significance to it which is clear and precise in his own mind.

However desirable, therefore, it may be for a more common sense terminology to be introduced into zoology as well as botany, it is not absolutely essential for scientific advance. When it is remembered also that any change of nomenclature in this respect must involve with difficulties the immense mass of zoological literature which has gathered together since 1839, it should make us pause before we suggest giving up the word cell, in the zoological sense, in favour of energid or biophor.

It seems to me that there are only two possible ways of reform open to us, and both are accompanied with grave difficulties.

We may introduce that change from the word cell to that of energid (Sachs) or biophor (Hansen), which, it has already been pointed out, will bring countless troubles with it in respect to the old and classical literature; or we may effect an alteration in nomenclature within the science of botany itself. Retaining the word cell to express protoplasm and nucleus, we may bring in a new name for the cell wall minus its living contents. Here also great confusion would result to the older writings, but the confusion would be within a more limited sphere. As we have already mentioned, it is in the province of the vegetable kingdom that the difficulties with regard to the meaning of the term 'cell' have arisen, and in reading the older memoirs we are frequently brought to a pause to enquire whether it is cell-wall or living cell-contents that are being referred to under the name of cell. The perplexities accompanying a substitute for the word cell in its significance of cell-membrane would, therefore, not be so heavily felt as those associated with a new word for the protoplasmic contents. The changes involved would be heavy within their sphere (it is not simply the word cell but the compound expressions which have been formed from it which would have to give place to the new order of things), but still this range would be a fairly limited one. It would be only the single science of botany, already perplexed with difficulties of meaning and not the additional provinces of zoology and animal (and human) physiology, which are clear in their use of the word, which would suffer from the innovation. What substitute might be employed for the word cell in its application to the membrane and the cavity included by it, is not easy to see—perhaps the word 'vesicula' is not altogether inappropriate, but if changes on these lines should ever prove applicable, it will then be time enough to look about for a new term.

The first alternative, which was mentioned above, is the one which hitherto has alone been dealt with by biologists.

Sachs, in his two notes in Flora (1892, 1895), has proposed to call the nucleus, together with the protoplasm governed by it at any time, an energid. If such an energid be included within a membrane it is to be spoken of as a cell. The distinguishing characteristic of an energid is the living element (protoplasm and nucleus), whilst that of a cell is the membrane.

From this point of view, therefore, the swarmspore of *Ulothrix* is an energid, whilst immediately that it forms a wall around itself it reaches the further dignity of a cell: the elements of cork tissue are also cells. In plants of the type of *Vaucheria* the protoplasm is studded with nuclei, and the whole mass is enclosed within a single cell-membrane. Each nucleus may be conceived as exerting its influence over a limited area of protoplasm, and ruler and ruled may together be mentally mapped off as a unit. The body of *Vaucheria* can therefore be described as consisting of a number of units—the energids—enclosed within a single cell wall.

Quite lately another small work dealing with these matters has been written by Dr Adolph Hansen, Professor of Botany at the University of Giessen.[1]

That part of the little book which deals with the 'Geschichte' is most carefully and judiciously written, and it at the same time has a life and vigour in its sentences that fire our enthusiasm for the subject. When we turn to the latter pages of the pamphlet in which the 'Kritik' is embodied, it awakens very mixed feelings within us. It sets us thinking, which is a good thing, but nowhere does it bring conviction with it. Where the views are most definitely stated we feel the greatest doubt, and where the arguments should be the most irresistible we are the least convinced.

Glancing at what is written on pp. 50-58, we see that the zoological definition, if I may so term it, of a cell is taken as the starting-point of the argument. The zoologist (*e.g.* Professor Oscar Hertwig) defines a cell as a little mass of protoplasm that includes a nucleus within its substance.

From this point of view Hansen maintains that the general assertion 'that plants are composed of cells' is untenable, since the larger mass of them is built up of cell-walls; moreover, whilst a naked swarmspore may be correctly named a cell, it ceases to be such immediately that it forms a wall around its surface.

In considering these statements we must first ask whether it is really a general assertion that 'plants are composed of cells.' I do not think so. I myself, and all whom I have ever heard, have always, both in animal and plant histology, stated that the plant or the animal, as the case may be, consists of cells and the products of cells. Our author meets this qualification in part, perhaps, by saying that those who assert that plants consist of cells (in the zoological sense) approach the difficulties in the above cases by regarding the membrane as 'secondary' or 'unessential,' and in that case he goes on to argue how are we to look upon *Caulerpa*, whose whole form and existence is determined by the cell-wall?

Secondary and unessential the cell-wall certainly is, however, when we compare it with the protoplasm and nucleus.

The latter determine whether a structure is living or dead; the former merely influences the manner of life.

Caulerpa as a genus is undoubtedly dependent on the presence of a membrane, but *Caulerpa* as a living thing is due to the co-

[1] "Zur Geschichte und Kritik des Zellenbegriffes in der Botanik," von Dr Adolph Hansen. Giessen: J. Ricker, 1897.

existence of protoplasm and nuclei. Hansen cannot maintain that the fibres and inorganic parts of a bone are not 'secondary' in importance when placed side by side with bone corpuscles, and yet the whole form and existence, nay, the whole use of the bone, depends upon their presence.

Another, and perhaps even more analogous case, is furnished by the elastic fibres of certain ligaments (*Ligamentum nuchae*): the whole existence of these bands is justified by the elastic fibres they contain, and yet when valued against the protoplasmic corpuscles upon which their origin depends, these fibres must be relegated to a 'secondary' place.

The white connective tissue fibres, the elastic fibres, and the vegetable membranes are all of enormous importance in the phylogenetic development of the organism; without them neither animals nor plants would ever have been able to rise above the state of mere flabby masses of protoplasm of limited size, but that which stands higher than race development, the existence of life is interwoven with the protoplasm and its nucleus. It is only compared with this high standard that we dare speak of the membrane of plants as 'unessential' or 'secondary.' When Hansen, however, a few lines further on, indicates that those who use these two words when speaking of the cell-wall do so alternatively with the expression of 'no importance,' he altogether misunderstands their position.

The next few pages are occupied with a criticism, more or less destructive, of Sachs' views on energids; this is followed by a proposed improvement. The word energid shall be dropped; in such cases as the *Siphoneae* the separation of energids is artificial, and therefore to be avoided; in its place the whole contents of a single membrane, or the whole mass of a membraneless organism, shall be named a 'biophor,' whether this be uni- or multi-nuclear. When a 'biophor' is enclosed by a wall it becomes a cell. "The cell," he adds, "consists always of a biophor and a membrane." When, therefore, a few lines further, he says that with the adoption of this nomenclature there is no reason why the elements of wood or cork should not be named cells—cells that have lost their biophors—certain inconsistencies of statement become apparent. Dr Hansen himself points out that the name biophor has already been used by Weismann in quite another sense. I cannot think that it would be wise therefore to employ it in this new relation, as even supposing Weissmann's biophors do not prove all that was hoped of them, they certainly will take a permanent historical value, and fresh troubles will appear on the horizon in consequence.

Although the last eight pages of Hansen's pamphlet seem to me to be open to criticism, the fifty foregoing pages can only give pleasure and satisfaction to those who read them.

The historical account of the cell theory is one of the best that has been written for a very long time, and the frequent apt quotations from and references to the original memoirs breathe a spirit of life into the story which one only seldom finds in these retrospective writings.

Before leaving the subject of reform by change in the name of 'protoplasm plus nucleus' it may be mentioned that if a new term is to be given to these parts, a more justifiable one than energid is that of 'corpuscle.' It is a name which is already used in this sense by the zoologist: he speaks of the white blood corpuscle, the bone corpuscle, etc., and its introduction into botany would be far from insurmountable. To speak of a swarmspore as a 'corpuscle' would be both common sense and simple. The membrane which this 'corpuscle' manufactures might be termed a 'cell,' and we should be speaking both logically and intelligibly when we spoke of the 'corpuscle' which lies within the 'cell' when we dealt with an element of the cambium, or if we spoke of an aggregate of corpuscles lying within the cell when we treated of *Vaucheria*. When the elements of cork came into view they would be cells pure and simple.

If a collective name for the living contents of a cell be required we might resort to the terminology which has already been employed by Professor Strasburger. A corpuscle, in the above sense, might be described as consisting of cytoplasm and nucleus, and the name protoplasm be applied to the cytoplasm or the nucleus, or both together, whether one nucleus or many, or none, were associated with the cytoplasm.

In what has been written above I only wish to throw out a few rough suggestions which may perhaps help the cytologist in a small degree as he gropes in the darkness for the right path.

It is either the wall of the cell or the living contents of the cell which must be re-named. If it be the former the difficulty will fall upon the shoulders of the botanist but leave the zoologist unharmed; if the latter, much depends upon the nature of the alteration.

If we radically change things by bringing into use a new name (energid, biophor) endless perplexities will undoubtedly arise, but if we resort to a word like that of 'corpuscle,' and employ it in the way indicated, the troubles may be smoothed over. It is a word that the animal histologist has already often used, and one that is not really difficult for the botanist to adopt; it is one that is common sense, and which likewise would allow the term cell to be brought into the same category.

I will leave matters here, however, for others to judge and to criticise. RUDOLF BEER.

IV

Fossil Apodidae

EVERY group of animals contains in itself its own record if we could but decipher it. The zoology of the future is bound under the fascination of this idea to devote itself to an ever closer comparative study for the express purpose of gaining insight into the lines of their past development. We have no royal roads, and the hopes which were held out to us a few years back that embryology would provide us with one have not been realised. It can only supply us with hints, the full meaning of which we must learn by other methods. Most hopeful of good result are those groups in which allied forms, recent and extinct, offer themselves in sufficient numbers for comparison. None can compare in this respect with the crustacea. There is an enormous wealth of known crustacean forms extending upwards from the very lowest fossiliferous strata and still swarming in every suitable part of the globe. The problem has long been to find a genealogical key to reduce this immense stream of organised life to some order of development.

The first and most striking feature noted was the fact that all but the most extreme forms are segmented, and the natural inference was that the common ancestor of the whole immense family must have been some less specialised segmented form. Of recent years the attention of zoologists in search of the most primitive form among existing crustacea has been concentrated upon the freshwater *Apus* which appears sporadically in rain pools all over the world. When the pools dry up the eggs remain in the mud ; indeed it is said to be a necessary condition of development that they should be so dried. How long they remain capable of development is not known. These facts are interesting because they suggest to us a way in which *Apus* may have survived, practically isolated from the struggle for existence, almost unchanged from the days when the crustacea first appeared on the planet. Those who claimed the primitive character of *Apus* were not disconcerted by the absence of fossil remains which could be definitely assigned to *Apus*. There was, of course, always the hope that such might be found, and further there was the striking fact to which they could appeal that one of the most prolific of early crustacean forms, the Trilobites, possessed many characters in common with *Apus*. The great Paleozoic family of the Trilobites are thus claimed as relations of the modern *Apus*. Now, however, that the claims of *Apus* to have been co-existent with and in some way closely related to the Trilobites are being reasserted on the basis of a new interpretation of the morphology of the former and of new facts as to the organisation of the latter—other mysterious relations are cropping up. With two of these—viz., *Protocaris* from the Lower Cambrian, and *Dipeltis* from the Lower Carboniferous, Mr Schuchert deals in the *Proceedings of the U.S. National Museum* (vol. xix.). Neither are quite new, the former having been figured and described by Walcott,[1] and the latter, from very imperfect material, by Packard. Mr Schuchert gives a new figure of *Protocaris* showing a little more detail, refigures the type specimens of *Dipeltis*, and is fortunate enough to be able to describe three new and almost perfect specimens of the same, one of which represents a new species.

Mr Schuchert has no hesitation in claiming both these as Apodidae, a welcome claim to any who, as above described, have interested themselves in placing *Apus* with the Trilobites at the root of the crustacean phylum. But inasmuch as Mr Schuchert only deals with these new claimants very generally, it has been suggested that a cross-examination of them from this special point of view would not be uninteresting.

Protocaris marshii

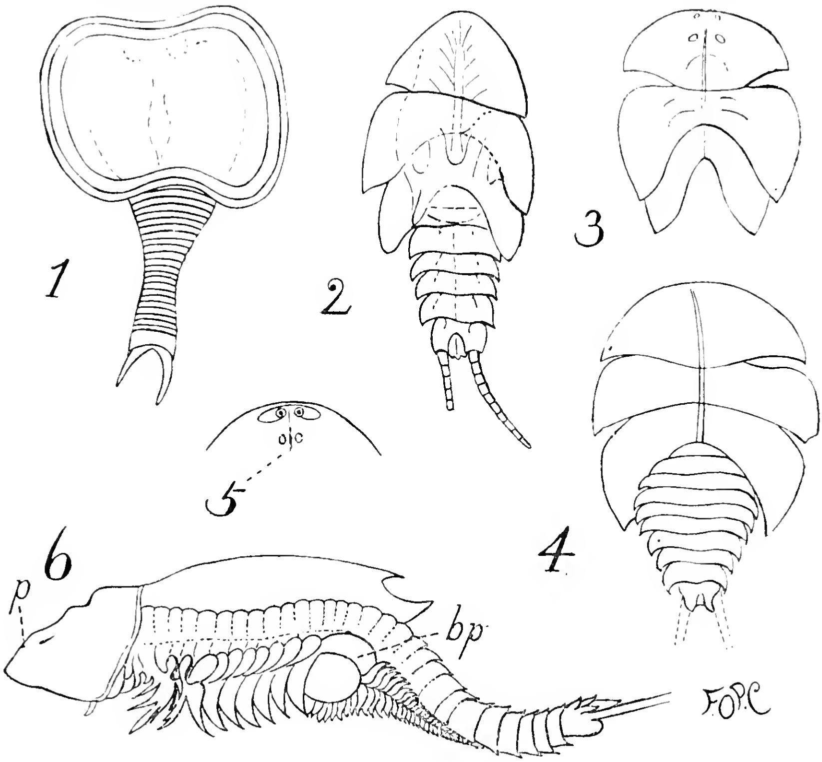
This fossil, only known in one specimen from the very oldest fossiliferous strata, speaks for itself (see fig. 1). Its large cephalic shield spreading backwards over the trunk segments, the extraordinary shortness of these segments in strong contrast with the wide anal segment with its pair of cercopoda, and lastly, which is a new detail added by Mr Schuchert, the pair of circular markings suggesting eyes near the anterior margin of the carapace—all proclaim its close affinity to *Apus*. Indeed, on first acquaintance with this fossil, I went so far as to suggest that it might with advantage be called *Apus marshi*.

Since studying Mr Schuchert's paper, however, I have been struck by two features which seem to me to have been generally overlooked, one of which is of prime importance. A comparison of the abdominal segmentation of *Protocaris* and *Apus* appears to show that the former retained a primitive condition which has been secondarily lost in the latter.

One of the principal arguments in favour of the great antiquity of *Apus* was found in the fact that a great and varying number of posterior segments are fixed in a rudimentary condition. The evidence for this was found in the progressively diminishing sizes of the

[1] *Bull. U.S. Geol. Survey*, 10, 1884, p. 50, pl. x. fig. 1; *Amer. Nat.*, 1885, p. 293; *Mem. Nat. Acad. Sci.*, iii. pt. 2, p. 145, pl. v.

limbs (see fig. 6), each pair being provided with a pair of abdominal ganglia. It was, of course, more or less of an assumption that these limbs and ganglia represented true segments, because to all appearance the true segments are marked off on the body as shown in the fig. 6. Further, authors were not wanting who definitely claimed that the apparent segmentation was the true one, and that the multiplication of the limbs, as many as four, five, and even six to one single segment, was due to some kind of secondary reduplication



1. *Protocaris marshi*, Walcott; 2. *Dipeltis carri*, Schuchert; 3. *Dipeltis diplodiscus*, Packard; 4-5. Another specimen of the same, showing the under surface and part of the upper with the eyes; 6. *Apus glacialis*, var. *spitzbergensis* from the side, with left half of the carapace cut away to show the whole trunk; *bp.* left limbs of the eleventh segment modified into a brood-pouch, *p.* pore leading into the water sacs over the eyes. [Figs. 1-5 after Schuchert.]

of organs on one and the same segment. The present writer, on the contrary, maintained that, as at the front end of the body each pair of limbs with its pair of ganglia corresponded with a true body segment, the whole series of the limbs should be taken as true segmental structures, and where, after the eleventh segment, marked by its brood pouch (*bp.*), the limbs begin to diminish in size, there being more than one to each external division of the body, these external divisions must be considered as secondary rings and not as true segments.

The correctness of this suggestion seems to me to be entirely borne out by the abdominal segmentation of *Protocaris* in which this secondary ringing has not taken place. If a body-ring were marked round the abdomen of *Apus* for each of the pairs of limbs, leaving out the most minute at the posterior end (*cf.* figs. 1 and 6), we should get a condition, at least for the limb-bearing portion of the abdomen, not unlike that shown in *Protocaris*, in which a multitude of very small segments (though not diminishing quite so clearly as they would in *Apus*) are in striking contrast with the anal segment.

It should, however, be noted by the way that this interpretation of the segmentation of *Apus* which receives such unexpected and welcome support from *Protocaris* was greatly complicated by the presence in *Apus* of a varying number of limbless segments in front of the anal segment. These still require explanation; at present I am inclined to look upon them as secondary reduplications of the anal segment.

This difficulty must not, however, be thought to stand in the way of my interpretation of the segmentation of *Apus*. That interpretation has already received abundance of support from the fact that the same fixation of rudimentary segments is found in the Trilobites. Inasmuch, however, as the relationship between *Apus* and the Trilobites is still a matter of discussion, it is especially welcome to obtain direct evidence from a fossil whose close affinity with *Apus* cannot be for a moment doubted.

The second point arises from the peculiar shape of the shield. Mr Schuchert describes it as subquadrangular, and quotes Clarke's suggestion that it has probably been subjected to some horizontal distortion in the shale. The longer I contemplate the figure of this shield the more convinced I am that it has simply been flattened out, and that in its original shape it was folded down at the sides of the body. Not only do the two anterior lateral projections of the shield suggest this, but the absence of the usual spikes at the postero-lateral corners of the shield are quite in accord. In *Apus* these spikes are turned up somewhat on to the back (fig. 6), and in the Trilobites they are spread out wide of the body in the horizontal plane. If the shield were folded down at the sides, these spikes would be a serious danger to the limbs and abdomen, and would be sooner or later dispensed with. Whether, again, the dotted lines running along the shield, shown in Mr Schuchert's figure (reproduced in fig. 1), lend any support to this view, I should not like to say, because we have no means of getting at their true meaning, but they certainly suggest to my mind a dorso-ventral flattening.

We have then, it seems to me, a simple and natural interpretation of the shape of the shield of *Protocaris* without assuming any other distortion than that due to flattening. Had there been any horizontal distortion, it would, one would expect, be more apparent in the abdomen than in the shield. I am therefore disposed to look upon *Protocaris* as an *Apus* in process of folding down laterally its whole carapace, a modification which, as I have shown in some detail elsewhere,[1] would lead on to the peculiar organisation of the ostracods. I lay stress upon the word 'whole,' because if only the free lateral flaps behind the head region are folded down, we should get a form which might lead on to the other bivalve-entomostraca, the Daphnidae and Estheridae. In making these suggestions I am again taking up my original position that *Apus* is the protonauplius of authors, and that from it or its young stages all the crustacea can be deduced. I may add indeed that nothing which has been said during the last five years has shaken me in that conviction, based originally upon my study of *Apus*. On the contrary, all the new facts which have come to light have tended without exception to confirm it. I refer mainly to the brilliant researches of Beecher on the limbs of the Trilobite, *Triarthrus*, and to these fossil Apodidae now under discussion.

The whole question, however, must of course be decided solely by the evidence; hence, I may remark in passing, it is somewhat surprising to find a zoologist declaring that he has no "sympathy (*sic*) with the peculiar phylogenetic speculations of Bernard." Antipathy against the views of a fellow-worker, however unscientific such an attitude of mind may be, is perhaps excusable, but it is not so excusable merely to refer readers to a semi-popular summary and not to the papers containing the detailed evidence for the hypothesis condemned. Readers could then judge for themselves whether these 'peculiar phylogenetic speculations' are speculations at all, and not rather necessary deductions from established facts.

Mr Schuchert's claim that 'eyes' can be faintly seen on the specimen will be noticed below.

Dipeltis

The claims of this fossil, of which only four specimens are known, to belong to the Apodidae, seem to me far more intricate than are those of *Protocaris*. It appears at first sight as if it might be a transition form between *Apus* and the Trilobites, and yet it only (so far as at present known) appears on the scene in the Lower Carboniferous, when the Trilobites were already beginning to pass away.

[1] "Apodidae," Section xv., p. 252.

We note, particularly at the outset, that Mr Schuchert evidently assumes that the head of *Dipeltis* corresponds to the head of *Apus* with its five segments. There is no reason to doubt the correctness of this assumption.

When analysing the differences between *Apus* and the Trilobites,[1] I came to the conclusion that the essential distinction between them was as follows:—In *Apus,* after the formation and fixation of the head region out of five fused segments, the cephalic shield grew backwards as a large free carapace over the trunk segments, which remained cylindrical, whereas in the Trilobites the head shield did not grow backwards freely over the body, but was repeated, by the familiar process of segmental repetition, on each of the trunk segments, giving rise on them to their pleural extensions. This curious difference in the mechanism of development resulted in two such apparently distinct forms as *Apus* and the Trilobites.

Dipeltis now comes on the scene. Its general appearance is that of *Apus,* but its repetition of the head shield is that of the Trilobite. The head shield and its two large segmental repetitions together appear as if they imitated the true free carapace of *Apus.* They only appear to cover the trunk segments, as the carapace of *Apus* covers its cylindrical trunk. Further, the trunk segments also show in their small pleurae a slight repetition of the head shield, or rather of the last of the two large repetitions of the head shield. These are unmistakable Trilobitic characters. On the other hand, the anal segment with its pair of cercopoda, the arrangement of the large eyes with anterior eyes or pores at the front of the head, the smooth round forehead without glabella or furrows, are unmistakably apodidan features. The animal looks remarkably like a cross between an *Apus* and a Trilobite! The only explanation I can suggest of this singular creature is that it resulted from a second attempt on the part of the main- or *Apus*-stem of the crustacean phylum to adopt the Trilobitic modification; that is, for a second time these primitive crustacea tried the segmental repetition of the early head shield instead of its free backward extension as a carapace. I would explain the *Apus*-like appearance of this second attempt as being due to the fact that it started from a true *Apus,* whereas the Trilobites owe their peculiar characters to the fact that they branched off before the *Apus* type was fixed—*i.e.* while the head region was still in process of formation—hence the glabella, the transverse furrows, and the uncertain position of the eyes on the head shield, with the three longitudinal regions running along the dorsal surface due to the segmental repetition of that head shield.

Turning, in conclusion, to the eyes in figure 3, these are shown very like those of *Apus* as to position and arrangement, while in front

[1] *Q. Journ. Geol. Soc.*, vols. li. and lii.

of these larger eyes are two spots. These are also found on another specimen, part of which is shown in figure 5, copied from Dr Schuchert's paper. In *Apus* in front of the paired eyes is a median pore (fig. 6, *p.*) leading into the water sac, in which these paired eyes are now sunk. In the base of the channel leading from this pore to the water spaces above the eyes a body called the median eye is suspended. The structure of this eye and its probable origin —as suggested by its structure—out of a pair of anterior eyes I have discussed elsewhere.[1] It is therefore of some interest to me to find an animal, with such claims to be a relative of *Apus*, having a pair of what appear to be eyes in front of the usual pair. It is true that this anterior pair persists in *Limulus*, but here again the relationship between *Apus* and *Limulus* is still matter of discussion. It is therefore once more pleasant to find what appears to be a direct confirmation of one's morphological deductions in an animal certainly related to *Apus*. What the peculiar ovals round these anterior 'eyes' are in fig. 5 it is impossible to say. It may be that here we see these eyes being drawn into the median line and below the surface.

In *Protocaris* I should regard the two eyes, suggested in figure 1, as corresponding with the paired eyes of *Apus*: perhaps owing to the bending of the carapace they are sunk deeper in the water sacs.

I quite agree then with Dr Schuchert in calling these early crustaceans fossil Apodidae. *Protocaris* I suggest is a modification of *Apus* in the direction of the bivalve Ostracods, *Dipeltis* as a second attempt of a true *Apus* to adopt the Trilobitic modification of repeating the head shield as pleurae along the trunk segments, while preserving for some reason or other the habit of *Apus*. HENRY M. BERNARD.

[1] "Apodidae," pp. 100-111.

V

Reproductive Divergence: A Rejoinder

IN the last month's number of *Natural Science* (p. 317) Dr Karl Jordan criticises a theory which I had briefly suggested in a previous number of the journal (p. 181), and which I had entitled "Reproductive Divergence: an Additional Factor in Evolution." I had there maintained that my theory differed essentially from Romanes' theory of Physiological Selection, for I endeavoured to show that if the less similar individuals in any species were at the same time less fertile *inter se* than the more similar, it would necessarily follow that in the course of succeeding generations these members would diverge more and more from each other, till eventually two or more new and mutually sterile species would be formed. I still hold, in spite of Dr Jordan's view to the contrary, that this theory is essentially different from Romanes', which maintains that if a portion of the members of a species happen to be sterile with all the other members, they will, in virtue of this physiological barrier, be enabled to vary independently of the parent stock, and so give rise to a new species.

I stated that my theory was made up of two parts, one of which was capable of mathematical demonstration, whilst the other could only be verified by experiment. Dr Jordan takes exception to the former, but accepts the latter, he holding that the "correlation between morphological characters and fertility of the specimens of a species cannot be denied." He also adduces an additional instance in support of the existence of this correlation.

The mathematical demonstration of the validity of the theory which I gave was, it would seem, rather too brief for its purpose. In excuse I must plead that my paper was intentionally of only a short and preliminary nature, as I thought a more extended discussion had better be deferred till I had more experimental evidence at my command. As, however, Dr Jordan appears to have entirely misunderstood my reasoning, he holding indeed that in the particular example I adduced to prove a divergence of character there would on the contrary be a convergence, I must now endeavour to explain the mathematical basis of the theory more fully. Thus of its validity I am convinced there can be no question. Its adequate demonstration depends only on skill in manipulation of figures, though this I am afraid I do not possess.

The fresh example I propose to adduce is founded on actual data, namely, the measurements of human stature in the case of parents and their offspring, which are given by Galton in his work on "Natural Inheritance" (p. 208). Here the relations between the stature of 205 parents of each sex and of their adult offspring are classified. The mid-parents I split up into three nearly equal groups—viz., those below 67·8 inches in stature, those from 67·8 to 69·2 inches, and those above 69·2 inches. These groups were found to give rise to more or less equal numbers of children. The numbers of children of each stature which were given birth to by each of these three groups of short, medium and tall mid-parents were then determined, partly by plotting out the various numbers and calculating from the smoothed curves, and partly by direct enumeration of the numbers. By then taking means between the numbers of tall children produced by tall parents with those of the short children produced by short ones, and also between the numbers of short children produced by tall parents with those of tall ones produced by short parents, roughly speaking the following mean values were arrived at :—

	Short.	Medium.	Tall children.
100 short parents give .	54	31	15
100 medium . . .	31	38	31
100 tall . . .	15	31	54
	100	100	100

From these figures it is seen that the percentage numbers of children are given, and not the actual numbers. This plan was adopted in order that the numbers of children might be kept the same as that of the parents. We see, therefore, that if 100 short, medium and tall parents of each sex be taken, the numbers of short, medium and tall children will still remain at 100, in spite of the fact that the children of each group produced by the different parents vary from 15 per cent. to 54 per cent.

In addition to the children produced by short, medium and tall parents, it is for our purpose necessary to know the numbers produced by intermarriages of short and medium, and of medium and tall parents. These numbers may be approximately obtained by taking means between the percentages for short and medium parents on the one hand and for medium and tall ones on the other. Thus :

	Short.	Medium.	Tall children.
100 short and medium parents give	42·5	34·5	23
100 medium and tall . . .	23	34·5	42·5

It is also necessary to assume, as Galton has shown his statistics warrant us in doing, that short and tall and also tall and short parents give rise to the same numbers of children in each group as do medium parents.

If we now take the case of 900 parents of either sex and divide them into three groups of 300 short, medium and tall individuals, it will be found that, as the result of their chance intermarrying, they will give rise to the following numbers of offspring of each sex:

293 short. 314 medium. 293 tall.

These numbers thus differ slightly from the 300 of each group which were started with, but it is not possible to arrange the numbers resulting from the various intermarryings so as to give quite identical figures. This, in fact, is not necessary, as it is only sought to compare two different cases, in one of which a correlation between fertility and stature is absent, and in the other of which it is present. This latter case we will now proceed to discuss.

Let it be granted that, in pursuance of this principle, 100 parents intermarrying with parents of similar stature now give rise to 120, and not 100 offspring of each sex. When on the other hand they marry with moderately taller or shorter individuals, let the number of offspring remain unchanged, and when tall or short parents marry with short or tall, let only 80 offspring instead of 100 arise. If now the same 900 parents of either sex intermarry according to the laws of chance, it will be found, on calculating out the numbers in accordance with the data given above, that the following numbers of offspring will be produced:

	Short.	Medium.	Tall.
	300·6	318·8	300·6
instead of	293	314	293.

That is to say, the numbers of short and tall individuals will have increased by 2·59 per cent., but those of the intermediate ones by only 1·52 per cent. There is thus a gain of about 1·07 per cent. in favour of the extreme individuals over the intermediate ones, or supposing the original 300 medium individuals had again been produced, there would now be 303·2 instead of 300 tall and short ones. This seems a very small amount in comparison with the fairly large differences of fertility we assumed to be present. It is, however, none the less genuine, in spite of its smallness, and it will of course become increasingly larger in succeeding generations. Thus supposing that in the next generation respectively 303·2, 300, and 303·2 short, medium and tall individuals of each sex intermarry, the short and tall offspring will be increased by slightly more than 1·07 per cent.; by in fact about 1·08 per cent. There will now, therefore, be 300 + 3·21 + 3·24 = 306·45 short and tall offspring, instead of 300. In succeeding generations the increase will gradually become more and more marked, and the divergence of the race into two distinct races more and more obvious. Again, I had supposed that the more widely any two individuals differed, the less on an average would be their mutual fertility. Hence this principle of reproductive divergence is a cumulative one, and the later stages of divergence will be very much more rapid than the initial ones.

Again, Dr Jordan objects to my statement that as the race diverges, shorter and longer individuals than those originally present will gradually be evolved. This seems to me to be so obvious as to scarcely need demonstration. Thus, let us suppose for the moment that our 900 individuals of each sex are split up into 450 short individuals and 450 tall ones. The members of each of these groups will deviate in either direction from the average size in the same proportion as the members of the original single group did. For instance, in the 928 adult offspring obtained in the above-mentioned anthropometric data, the mid-stature or median of the whole group was about 68·2 inches, and 10 per cent. of the group were below 64·5 inches in height. Supposing now the median of the new group of short individuals be 65·8 inches, it follows that 10 per cent. of this group will be below 62·2 inches in height. That is to say, individuals shorter than any of those originally present will have arisen. The tall individuals will, of course, deviate in a similar manner in the opposite direction.

This fresh attempt to demonstrate the correctness of the principle of reproductive divergence will not, I am afraid, appear much more easy to understand than the former one, but it at least has the merit of being roughly founded on actual data, so that fewer preliminary assumptions are necessary, and the result obtained is a more absolute one, and is moreover independent of the Law of Regression towards mediocrity.[1] To me it seems the principle is sufficiently obvious without the help of any mathematics at all, if it be looked at in the following manner. Let any number of individuals in a species be divided up into two groups—the larger ones and the smaller. Then if there be a correlation between size and fertility, it follows that those larger individuals which happen to breed with the smaller ones will give rise to fewer individuals of intermediate size than they would have done if there had been no such correlation. That is to say, the race will begin to diverge, and as this divergence is cumulative, it will ultimately split up into two or more new races. H. M. VERNON.

[1] "Natural Inheritance," p. 95.

VI

Professor Schiller on Darwinism and Design

I SUPPOSE that it must, on the whole, be reckoned as an encouraging sign of gradual advancement that, from time to time, articles dealing with physical science are allowed to appear in the monthly *Reviews*; for the appearance of such articles indicates that the editors of these strictly commercially-conducted *Reviews* can safely reckon upon a tolerable percentage of their readers being interested in physical science.

This apparent gradual spread of an interest in physical science is comforting; and it is good that the huge section of the public, who never by any chance read scientific books or scientific periodicals, should yet, through the medium of the *Reviews*, acquire some slight taste for physical science and some trifling knowledge of recent advances therein. From this standpoint, then, the practice of inserting scientific articles in the lay *Reviews* is much to be commended; but it is not to be denied that there are compensating disadvantages, and these are due to two factors.

In the first place, the editors of these *Reviews*, being usually—like most other "well-educated" Englishmen—utterly ignorant of physical science, are hopelessly unable to estimate for themselves the value of any scientific article submitted to them, and are thus entirely incapacitated from exercising any truly editorial functions with regard to such articles; and, in the second place, all these *Reviews* are conducted upon strictly commercial principles, being regarded primarily as money-making machines, and only secondarily as organs of education and enlightenment. Both these factors conspire to bring about one and the same result, viz., that the editors taboo all articles not signed by a big name—which is at once the guarantee of profit to their purses and of safety to their ignorance—and are naturally tempted to exclude controversial replies. Usually their assiduous worship of big names safeguards them from any fiasco; but now and then there creeps into a *Review* an article which betrays only too signally the fact that it was entirely unintelligible to the "editor," and that that omnipotent functionary was educationally disqualified from perceiving the extraordinary nature of the statements and arguments appearing in his *Review*.

To this category seems to belong the article on *Darwinism and Design*, contributed by Professor F. C. S. Schiller to the *Contemporary* for June last—an article that, though dealing with physical science, bears the most obvious traces of a metaphysician's handiwork. I certainly do not propose to criticise this article in detail; for such a criticism, to be at all adequate, might require an article a good deal longer than Professor Schiller's own: and, fortunately, any such detailed criticism were superfluous; firstly, because all readers who have any familiarity with biological science may perceive for themselves the errors into which Professor Schiller has fallen; and, secondly, because those readers of his article who are entirely ignorant of the subject would hardly be among the readers of *Natural Science*, nor would they perhaps be affected by my arguments even if they read them. I propose, therefore, merely to call attention to two or three notable points in *Darwinism and Design*.

The article, which opens with somewhat of a flourish of trumpets, and excites expectations that are by no means realised in the sequel, is directed to prove that 'Darwinism' has not necessarily excluded the possibility of a teleological conception of organic nature; but that, properly scrutinised, evolutionism rather strengthens the argument from 'Design' than otherwise.[1] Now Professor Schiller makes one or two initial omissions of a notable character. He intentionally confines himself to "living nature," thus putting aside altogether the awkward question as to whether the evolution of solar systems be, or be not, ascribable to Design; and he writes as though evolutionism and Darwinism were the same thing—as though to demonstrate an error in any one of Darwin's initial assumptions were to at once clear out of the way all biological objections to the teleological conception of the world. The former omission is highly significant of the philosophical value, or otherwise, of Professor Schiller's article; the latter oversight appears to me to vitiate his entire argument, and to render it little more than a beating of the air; and one is tempted to say that the article would have been topical in 1860, but is a generation out of date now.

Professor Schiller is good enough to tell us that the old-fashioned argument from design was rotten even before the advent of Darwinism; but he proposes to recast the argument in such fashion that 'Darwinism' shall be no obstacle, but rather indeed an assistance, to the teleologist. He tells us that "before the argument from design has any theological value, two things have to be shown: (1) that intelligence, *i.e.*, action directed to a purpose, has been at work; and (2) that the intelligence has not been that of any of the admitted existences."[2] The former part of this statement reads rather curiously; but it only means that, for the author's purpose, it is essential to prove that the adaptations in organic nature have not been brought about solely by a blind mechanical process, but

[1] Cf. p. 144.

[2] *Contemporary Review*, p. 868.

are due, at least partly, to intelligent effort on somebody's part. To make good this position, Professor Schiller has recourse to somewhat strained arguments. For instance, he suggests that "a complete denial of design in nature must deny the efficacy of all intelligence as such. . . . If that view were true, we should have to renounce all efforts to direct our fated and ill-fated course adown the stream of time" (p. 868). But this ingenuous attempt to make the anti-teleologist damn himself—on the plea that logically he is bound to deny the efficacy of all intelligence if he deny the argument from design—seems to me a mere trifling with words, and recalls the old saying that "words are wise men's counters, they do but reckon with them; but they are the money of" metaphysicians. Professor Schiller must know perfectly well that the "denial of design" is merely an elliptical phrase meaning the "denial that the adaptations in nature evidence the design of an over-ruling Divine intelligence"; and that this denial, neither logically nor historically, implies anything so absurd as the denial of "the efficacy of all intelligence"—such intelligence itself being regarded by consistent evolutionists as a notable adaptation to our environment. But this playing with words, this use of words in Humpty-Dumpty's portmanteau fashion, so that two very different meanings are packed into one word, is typical of Professor Schiller's article. Of this we have a glaring example in the following passage, which is, to a certain extent, the key to the author's position:—

"The ease with which the Darwinian argument dispenses with all intelligence as a factor in survival, excites suspicion. It is proving too much to show that adaptation might equally well—*i.e.*, as completely, if not as rapidly—have arisen in automata. For we know that we ourselves are not automata, and strive hard to adapt ourselves. In us at least, therefore, intelligent effort is a source of adaptation, and the same will surely be admitted in the case of the higher animals. . . . Intelligence therefore is a vera causa, as a source of adaptations, at least co-ordinate with natural selection; and this can be denied only if it is declared inefficacious everywhere—if all living beings, ourselves included, are declared to be automata. . . . If, however, intelligence is readmitted as a vera causa, there arises at least a possibility that other intelligence besides that of the known living beings may have been operative in the world's history" (p. 871; spaced type mine).

It is worth while to realise the fallacy involved in this passage; for, when once that is realised, there is not much left in our author's case. The whole fallacy lurks in that word adaptations. It is obvious that individual animals do functionally adapt themselves in many ways to their environment; and that, the greater their intelligence, and the better consequently their adaptations, the greater their chance of life. But, to Professor Schiller's argument, it is essential to show (1) that structural adaptations can be, and are, thus brought about; and (2) that such adaptations are inherited. This Professor Schiller does not even attempt to prove; but it is the crux of the entire argument. If Professor Schiller can prove that individual intelligence is capable of producing direct structural modifications — *i.e.*, modifications at which it has directly aimed—and if he can farther show that such modifications are inherited, he will certainly have made out the former part of his hypothesis, viz., that "intelligence, *i.e.*, action directed to a purpose, has been at work." This proof, however, he does not even attempt; but contents himself with asserting of 'adaptations' what is true, so far as we know, only of individual functional adaptations; and by thus using the word 'adaptations,' simply and without any expressed qualifications, he obtains a fictitious appearance of a demonstration; for the term 'adaptation,' when used in such arguments by evolutionists, is used in the sense of 'inherited structural adaptations'—as Professor Schiller ought to know perfectly well. It is simply the old metaphysical sophism of using one and the same word in two distinct senses, and crediting to the one sense of it what is readily granted to the other; and to perceive the fallacy involved in his use of the word 'adaptations' is to knock a pretty considerable hole in the bottom of Professor Schiller's argument. Can our author see no difference in kind between the functional adaptations of an individual man to a maritime life or a desert life and the structural adaptations of a whale and a camel to these respective modes of life? We all know that individual men, by varying their food and clothing, can thus intelligently adapt themselves to very different conditions of life; but how does this fact create any presumption that even the camel's stomach or the whale's form is the result of intelligent action directed to a purpose, and still less that the countless wonderful adaptations of which no man even is conscious until he studies anatomy, and over which even then he is powerless to exert any intelligent control, and which work best when he is unconscious —such adaptations, I mean, as the valves of the heart and countless others—are the outcome of intelligent action directed to a purpose? It does not indeed seem clear that even Lamarckian factors would avail Professor Schiller here, unless, possibly, the proverbial mouse could be shown to have acquired a neck a yard long by intelligently and purposefully directing its gaze to the unattainable cheese above it; for it were one thing to admit that the effects of use were inherited, but quite another to assert that any evolution has been due to the intelligent action of animals directed to the purpose of adapting themselves to their surroundings. Even were Mr Spencer's argument concerning the giraffe conceded by his opponents, I scarcely see how this would help Professor Schiller; for not even the most fervid Lamarckian ever dreamed that the giraffe, when straining at the leaves, was intelligently and purposefully directing the development of muscles and bones and the rearrangement of his internal and external anatomy generally. Thus the very modifications, on which the whole value of his Lamarckian neck rested, were due to blind mechanical action over which he exercised no intelligent control. Moreover, it does not appear that Professor Schiller had even cleared up his notions of 'adaptations' so far as to think of appealing to Lamarckism to support his contention that intelligent action is responsible for many adaptations; for in that section he does not even mention Lamarck, though soon afterwards he remarks in passing (p. 872) that "it is practically certain that some Lamarckian influences must affect both the number and the character of the variations"—the metaphysician, with typical assurance and hastiness, thus dogmatically deciding a question over which our leading biologists, who alone are competent to speak authoritatively, are hopelessly at variance.

I pass over the curious passage in which Professor Schiller cites the action of the 'general physical and chemical laws of nature' as barring variations in certain directions, and thus rendering impossible the indefinite variation on which Darwin founded his arguments (p. 872),[1] and I will not comment here upon his strange citation of Bateson's work on Discontinuous Variation—which he fondly supposes to constitute a stumbling-block to Natural Selection—for I have already replied to that argument in the columns of *Natural Science* (May, 1895); but we will pass at once to the concluding section of Professor Schiller's article. This is really suggestive and ingenious; and, had the author excised the first ten pages of his article and retained only the latter part, he would probably have stood higher in the opinion of biologists. The pith of this latter part of his argument may be stated in very few words.

Darwin assumed that organisms vary indefinitely in every direction, and that the evolution of species is due to the action of natural selection in seizing upon and fixing a few among these countless variations. Were this assumption a literal statement of fact, any possibility of interpreting the universe teleologically would be barred *ab initio;* but, if variation be not indefinite in every direction, but more frequent in one direction than in others, it may be purposive; and thus the ground is cleared for building up a new teleology. Now Darwin's assumption was not a statement of fact, but a methodological assumption, exactly analogous to the economic assumption of an ideal 'economic man,'

[1] The same sort of objection might be brought against the first law of motion, and in either case is obviated by the insertion of the words 'tends to—'

or to the mathematician's assumption of a single body in an otherwise void space. By arguing from such methodological assumptions, the 'laws' of motion and the 'laws' of economics were obtained; but all these 'laws' are applicable to concrete facts, only with modifications, and after re-introducing the qualifications that were methodologically omitted from the premises, and the methodological assumption must never be accepted as a statement of literal facts.

Now this is certainly a very interesting and ingenious thesis, and I do not remember previously to have seen the suggestion made that Darwin's assumption was thus purely methodological; but it does not seem likely that Professor Schiller's ingenuity will be of any service to the teleologist. It were necessary before any teleological argument can be founded that he should prove (1) that variations are not indefinite but definite; and (2) that such definite variation can be attributed to no mundane factors, but can be explained only by the assumption of supra-mundane purposive intelligence.

The former hypothesis—for the sake of the argument—we will grant to Professor Schiller, although he has not even attempted to prove it, except by his curious remarks about 'chemical and physical laws,' &c.:[1] but what is it worth to him? Nothing! for that 'ultra-Darwinian' Weismann has already contended that variation is definite in direction, and he has offered a purely mechanical explanation of such definiteness;[2] so that what was to be treasured up as the trump card of the teleologist has already been played on the other side. But is it not indeed significant that the author of this curiously belated article, seeking to turn the evolutionists' flank and to clear the field for the teleologists, should be unaware that our most prominent living evolutionist had already, by anticipation, outflanked his flanking movement more than a year ago? Thus the only really at all valuable part of Professor Schiller's article, the one part not invalidated by fallacious trifling with words, is yet invalidated by his ignorance of the science that he seeks to press into the service of teleology. What, in this year 1897, can be more hopelessly belated than the following remarks (p. 875): "It is clear then that, to explain the changes which have resulted in the existing forms of life, some variable factor has to be added to natural selection. And as to the nature of that factor, Darwinism *qua* Darwinism tells us nothing." Perhaps it is even clearer that, had Professor Schiller possessed any acquaintance with

[1] It is one thing to argue that Darwin did not prove variation to be indefinite, quite another to prove that it is n o t intrinsically indefinite; and the teleologist must prove the latter proposition, and prove it by a wide induction from multitudinous details and experiments, before he can even talk of teleology.

[2] See his "Germinal Selection."

Weismann's work—of the whole of which, and, indeed, even of Weismann's existence, he seems to be profoundly ignorant—this passage, and the whole of his article besides, would have remained unwritten. It does seem to me rather hard that metaphysicians who want to write about evolution will not take the trouble to find out what evolutionism connotes at the present day; but Professor Schiller's ignorance of recent advances in biology is quite on a par with the curious and fatal misconception of the processes and factors of evolution that prevails throughout his article; and the cue to his philosophical status is afforded by his very theological conclusion, from which we learn that, if the whole of his argument be valid, evolutionism rather helps than hinders the teleologist, since he is no longer compelled to attribute perfection, but only gradual perfectioning, to this very imperfect scheme of nature, nor to saddle an omnipotent deity with the responsibility for deliberately planning and designing all the cruelty and suffering prevalent throughout the world. In other words, he proposes, by the help of evolution, to save divine morality at the expense of divine power; his directing Intelligence being, not an omnipotent fiend, but only an unpractised though well-meaning bungler. I hope that the theologians will be duly grateful to their very candid friend.[1]

[1] The theory of an innate tendency to vary in a definite direction of advance was, of course, advocated long ago by Nägeli. In an article on "Evolution and Teleology" that appeared in the *New Science Review* of July, 1895, I pointed out how strongly the recent advanees in botany have told against this theory, and how in nearly every line of ascent evolution upwards has ended in a blind alley.

F. H. Perry Coste.

SOME NEW BOOKS

"Let us now Praise Famous Men!"

The Founders of Geology. By Sir Archibald Geikie. 8vo, pp. x + 298. London: Macmillan & Co., 1897. Price, 6s. net.

If, as Emerson has said, all history may be read in the lives of a few great men, this may be regarded as specially true of the history of science. The facts of science, no doubt, are accumulated by a multitude of workers; but ideas have their genesis in the brains of the leaders, and the growth of the ideas may best be studied as it took place originally in the minds of a few individuals. Hence Sir Archibald Geikie, when eager to dispel the lamentable ignorance of most of us concerning the historical development of geology, could have chosen no better means than this skilful and charming narration of the endeavours of the early pioneers.

The occasion of this work was the inauguration of the George Huntington Williams Lectureship at Johns Hopkins University, and one can imagine the delight with which American geologists listened to the polished periods and lucid exposition of Sir Archibald. Some of the perorations, indeed, are better adapted to the lecture-platform than the study, and an occasional weakness in the usually correct and forceful style suits the written less than the spoken word. Careful revision, for instance, would have eliminated such a sentence as this: "His father . . . died while the son was still very young, to whom he left a small landed property in Berwickshire." We test the author by his own high standard.

The geologists referred to in this book are—Leibnitz and Buffon (who, however, are not regarded as among the founders of geology, but as the last of the cosmogonists), Guettard, Desmarest, Pallas, De Saussure, Lehmann, Fuchsel, Werner, D'Aubuisson, Von Buch, Hutton, Sir J. Hall, Giraud-Soulavie, Cuvier, A. Brongniart, Omalius d'Halloy, Rev. J. Michell, William Smith, Murchison, Sedgwick, Logan, Agassiz, Nicol, Sorby, Lyell, and Darwin. Many others are mentioned incidentally and, in relation to these, the pioneers. For the expressed purpose of the book, this list is an excellent one. No doubt, every one that has read much in the early literature of geology will be ready with suggestions for its amendment. The fact is that a vast number of these old writers were not so ignorant or so foolish as we are too ready to suppose. Guettard is one whose claims have been strangely overlooked, and we are delighted to see this admirable appreciation of his many services to our science. But the list, it will be noticed, is almost entirely confined to French, German, and British geologists: Linnaeus and Wallerius are dismissed in a single line. Linnaeus, however, did more than arrange certain minerals in one of his kingdoms of nature: he studied the strata in which minerals and organised fossils occurred, travelling through Sweden and making careful geological observations, which he published in the accounts of his journeys to Gotland, to Oeland, and, above all, to Westrogothia and Scania. He drew a section of the strata composing Kinnekulle, and paralleled them with beds in other parts of Sweden, subsequently using his knowledge to interpret the structure of Scania. "Thus," said he, "the section of Kinnekulle serves as introduction to *Strata terrae* or the anatomy of the earth-crust, not only here in Westrogothia, but probably over the greater part of the world." He recognised that the strata had been deposited in the sea throughout many long-vanished periods, and attempted to classify them according to their relative age. Thus Linnaeus laid the foundations of the Wernerian system before Werner was born; and it was not long before his fellow-countryman Bergman erected upon that foundation the actual framework that Werner filled in. Other Scandinavians might have been mentioned, such as Gyllenhahl, with his truly remarkable palaeontological study of *Echinosphaera*, and Hermelin, with his geological maps of Southern Norway and Sweden. The most curious omission, however, considering the occasion of the lectures, is that of citizens of the United States. Featherstonhaugh and H. D. Rogers are just alluded to, but that remarkable and much-abused geological genius, Ebenezer Emmons, is not even named.

We have not mentioned these omissions for the sake of fault-finding, but as further evidence of the amount of good work done by many whom it is the fashion of the present day to disregard. We sympathise warmly, as we have already said, with Professor Miall's recent plea for a more historical method of teaching the natural sciences, and, as a help in that direction, no book is better adapted than the present. But those who imagine, if such there be, that they have passed beyond the student stage, would yet do well to dip now and again into the battered volumes that grow dusty on topmost shelves. There are many observations and many shrewd suggestions hidden in those old books, made perhaps too early in the day to have taken effect, but waiting to be applied by us now with our modern knowledge and methods. Rosinus, for instance, 178 years ago, described the course of the nerve-canals, "*foramina jure meritoque pro nervorum canalibus reputanda*," in the cup of the lily-encrinite, in language that Dr W. B. Carpenter (the modern discoverer of that nerve-system) could not have bettered. The fact is that the worth of a man's work does not necessarily depend on the number of his years or on the century in which he lives.

Some of the most valuable passages in this book are those in which Sir Archibald uses the weight of his experience to enforce the morals to be derived from the study of the older writers. In one place he quotes Fitton's review of the Wernerian school: "A Wernerian geognost is chiefly employed in placing the phenomena he observes in the situations which his master has assigned to them in his plan of the mineral kingdom. It is not so much to describe the strata as they are, and to compare them with rocks of the same character in other countries, as to decide whether they belong to this or that series of depositions, supposed once to have taken place over the whole earth, . . . to ascertain their place in an ideal world." Similar criticism might justly be applied to-day in various branches of science. Sir Henry Howorth's 'heretical' article in our November number did little more than translate the words of Fitton. Then are there not a good many hard-working zoologists and botanists who are perfectly satisfied if they can assign their specimens to certain pigeon-holes made for them in a rather hypothetical cupboard called a System? It is even a fact that many excellent old writers are ignored just because they could not, or would not, use a regular Linnaean terminology. Their works are neither read nor to be found in our scientific libraries. Into such obscurity even Hutton's great work, "The Theory of the Earth," may fall. It is nothing that we saw it characterised recently in the catalogue of a second-hand bookseller as "This extravagant theory which was defended by the celebrated Professor Playfair"; but we were indeed surprised to find no copy of it in the geological library at the Natural History Museum.

With reference to Playfair's defence, known as "Illustrations of the Huttonian Theory," Sir Archibald remarks: "For precision of statement and felicity of language, it has no superior in English scientific literature. To its early inspiration I owe a debt which I can never fully repay. Upon every young student of geology I would impress the advantage of reading and re-reading, and reading yet again, this consummate masterpiece. How different would geological literature be to-day if men had tried to think and write like Playfair!"

But it may be objected, How can we find time to read these old authors, much less to write like the best of them? We have to read the modern literature, and even a small part of that is overwhelming. We have so much to do that we cannot waste our energies on mere style, and we must rush out our results the easiest and quickest way we can, or we shall be anticipated. Sir Archibald's answer should be laid to heart, not merely by geologists old and young, but by all scientific workers. He finds his consolation in "the conviction, borne in upon us by ample and painful experience, that a very large mass of the geological writing of the present time is utterly worthless for any of the higher purposes of the science, and that it may quite safely and profitably, both as regards time and temper, be left unread. If geologists, and especially young geologists, could only be brought to realise that the addition of another paper to the swollen flood of our scientific literature involves a serious responsibility, that no man should publish what is not of real consequence, and that his statements when published should be as clear and condensed as he can make them, what a blessed change would come over the faces of their readers, and how greatly they would conduce to the real advance of the science which they wish to serve." There is not a dull page in "The Founders of Geology," but, were it only on account of this last paragraph, we should wish for it many readers in all parts of the world.

The Death of Rocks

A Treatise on Rocks, Rock-Weathering, and Soils. By G. P. Merrill. 8vo, pp. xx + 411, pls. xxv. New York: The Macmillan Company, 1897. Price, 17s.

What the unsatisfactory preservation of fossils is to a palaeontologist and surface drift to a stratigrapher, decomposition of rocks has long been to the petrologist. Each is regarded as an unmitigated nuisance, interfering with the observation of facts for which the enquirer is searching; and as the stratigrapher is expected to map solid rock through its drift covering, the petrological specialist, when consulted as to the character of a rock, is expected to say, not what it is, but what it may once have been.

Professor Merrill, however, has endeavoured, in the work under review, to make the processes and results of weathering as interesting and full of real importance as the American and English glacialists are making the drift. For it is the weathering of rocks which forms soil, the link between the dead earth-crust and the living plants and animals upon it. Geologists have studied rocks in all their various kinds, their origin, differentiation, and metamorphism, their birth, growth, and life; we have now to study their death, and the earliest parts of the process which culminates in their resurrection.

In order to make his book self-contained, Professor Merrill devotes several chapters to considering the chemical and mineralogical composition structures, mode of occurrence, and various types of rocks; this is a fairly useful summary, but we are inclined to doubt whether the geologist will need, or the lay-reader understand it. Many of the illustrations in this part are admirable, and the abundant analyses are particularly valuable for reference later on. To many of these no references are appended, and we may conclude that they are due to the author himself, who must have expended a great deal of time and labour on the analyses throughout the work.

Part III. is devoted to the weathering of rocks, each of the chemical and mechanical agencies being taken in turn. Several valuable instances of the effect of alternations of high and low temperature are given, and the effect of cold rain on highly heated surfaces is referred to. While stress is laid on the effect of hydration, the work of carbonated rain-water, and of the humic, ulmic, and crenic acids naturally comes in for a lion's share of consideration. The action of the first of these solvents upon many silicates can be detected within ten minutes, while forty-eight hours' digestion will obtain from some amphiboles, epidotes, felspars, etc., quantities of lime, magnesia, iron, alumina and silica, amounting to from 0·4 to 1 per cent. of the mass. Hornblende is more easily acted upon than felspar, and even magnesian silicates are attacked, so that serpentine cannot be considered a final product of decomposition. Increasing the pressure on the solvent has much more effect than prolonging the time of its action. Daubrée's experiments on attrition are referred to, and the work of plants, bacteria, termites, and marine animals on the sea-bed, is not neglected.

Special cases of weathering are next treated in detail and illustrated by full analyses. Mere bulk analyses of the fresh and weathered rock are misleading, as they do not show all that has actually occurred. It is necessary to ascertain which constituents are least liable to be leached out, and to recalculate the analyses on the assumption that they remain constant. Alumina and iron oxides are least liable to this, and the analyses of acid rocks are worked out on this assumption. An example will show the value of this method; the one chosen illustrates, in addition, that while only 30 per cent. of the original rock was soluble in hydrochloric acid and sodium carbonate, the proportion is now nearly 70 per cent. The instance is that of a gneiss from Virginia.

	FRESH GNEISS.		DECOMPOSED GNEISS.		CALCULATED AMOUNTS SAVED AND LOST.		
	Bulk analysis:	Soluble portion.	Bulk analysis.	Soluble portion.	Total loss.	Percentage of each constituent saved.	Percentage of each constituent lost.
S_1O_2 .	60·69	10·09	45·31	17·69	31·90	47·55	52·45
Al_2O_3 .	16·89	13·54 (Al₂O₃ + Fe₂O₃)	26·55	24·86	0·00	100·00	0·00
Fe_2O_3 .	9·06		12·88	11·80	1·30	85·65	14·35
CaO .	4·44	1·61	Tr.	0·06	4·44	0·00	100·00
MgO .	1·06	0·89	0·40	0·37	0·80	25·30	74·70
K_2O .	4·25	2·40	1·10	0·75	3·55	16·48	83·52
Na_2O .	2·82	1·10	0·22	0·25	2·68	4·97	95·03
P_2O_5 .	0·25	0·23	0·47	—	0·00[1]	100·	0·00[1]
Loss on ignition	0·62	0·62	13·75	13·40	0·00[1]	100·	0·00[1]
	100·08	30·17	99·98	69·18	44·67		

[1] Gain.

This table makes at once obvious the great loss in silica, lime, and alkalies, and the gain in water; while it further indicates from the increase in the soluble portion that the state of combination in many of the compounds has changed. Mechanical separation and microscopic examination of the decomposed material throws further light on the character of the decomposition and nature of the surviving minerals.

In basic rocks the loss of silica is rather less, and that of iron and magnesia more conspicuous, while in a French basalt the felspars were the first to yield, and the augite and olivine the last.

Under the heading "Physical Manifestations" are treated: Disintegration without decomposition, influences of crystalline and rock-structure, mineral composition (roughening and crusting), induration and colour changes. Original characters are sometimes lost entirely through weathering, basalts pass into apparent argillaceous deposits, and granites and gneisses with their veins and every structural detail well preserved, may become so soft that a stick can be thrust deeply into them. Chemical compounds become on the whole simplified by weathering, but oxidation and all chemical change, except hydration, seems to cease below the permanent water-level. More refractory and dense residues, like xenotime, monazite, tourmalines, rutiles and precious gems, have tended to accumulate in favoured spots, on account of the weathering and destruction of the rocks in which they were originally contained.

The rate of weathering is influenced by composition, texture, and position of rocks, and by humidity of climate. Weathering in humid climates differs in kind as well as degree from that effected in dry climates as shown in California by the author and by Judd in the Nile delta. Prehistoric implements and old surfaces covered unconformably by newer deposits may sometimes enable us to guess at the amount of work done in a given time, but the ascertaining of the actual rate of work is difficult, and the results not very satisfactory. Geikie's results, on this branch of the subject, not quoted here by the author, are important. A summary of the work of transporting agents concludes this portion of the work.

The fifth and last part of the book is devoted to the consideration of the regolith, a term used to include the soil which covers the solid rocks like a blanket (ῥῆγος), an incoherent mass of varying thickness composed of materials essentially the same as those which make up the rocks themselves, but in greatly varying conditions of mechanical aggregation and chemical combination. The materials of the regolith are either sedentary or transported, the first class being either residual like wacke, laterite, terra rossa, &c., or cumulose like peat. Residual deposits are generally unstratified and may possess characteristics which they have 'inherited' from hundreds or even thousands of feet of rock which have totally disappeared. Transported deposits are colluvial (scree and avalanche debris), alluvial and aeolian, or glacial. Alluvia vary in character and to a striking extent in fertility according to whether the river bearing them drains a dry or a damp country; in the former case the undecomposed silicates will be rich in plant food. Where saline deposits are absent, indeed, the soils of very dry countries are of an extremely fertile character and only need rain to be covered with luxuriant vegetation, and this is especially the case because percolating rain has not been present to concentrate the clay into a 'hard-pan' below the surface.

The averages of several hundred analyses of soils in dry and humid regions show that the soluble constituents of the soil in arid tracts amount to 30 and in wet tracts to 15 per cent., the greatest differences being in the quantity of lime, magnesia, and alkalies.

Richthofen's aeolian hypothesis is considered satisfactory when applied to the Chinese loess, but not to that of America, which latter the author supposes to be the result of streams draining from the ice of the glacial period.

An important organic agent in the formation of marine deposits in inlets is found in the eel-grass, which grows vigorously as soon as a little mud has formed on the bottom, and then by deadening all currents promotes very rapid deposition of fine salt.

In dealing with the 'rock-flour' brought down by glaciers, Professor Merrill alludes to the small amount of actual clay present. It has been proved by Mr E. Dickson that what there is of this material is ground up felspar and not kaolin.

To illustrate the wonderful degree of comminution reached in soils, it is pointed out that the total surface area of the grains in a cubic feet of soil amounts on an average to 50,000 square feet. On areas like this the operation of organic acids, plants, animals, and water must be enormous. Ants working in such soil effect as much change as the earthworms studied by Darwin, and Shaler's calculation that the former bring a layer of soil one-fifth of an inch thick to the surface every year finds a close parallel in the figures obtained by Darwin.

Professor Merrill's book is an admirable introduction to a complicated and difficult subject. It is packed with facts, not perhaps arranged in the best possible manner, and illuminated with a wonderful series of analyses which contain a fund of valuable information. The author is modest in his deductions, not eager to generalise, and only adopting or advocating a conclusion after full discussion. It is to be hoped that his work will lead to the more detailed study of the changes produced in rocks as they weather down and pass into their final, but by no means least useful, form. The illustrations are for the most part clear and well chosen, but it would be well to indicate the exact magnification of the diagrams of microscopic objects in the text.

The Volcanoes of North America

Volcanoes of North America: A Reading Lesson for Students of Geography and Geology. By Israel C. Russell, Professor of Geology, University of Michigan. 8vo. Pp. xiv+346, with 16 plates and 11 figs. in text. New York: The Macmillan Co.; London: Macmillan & Co., Ltd. 1897. Price, 16s. net.

The second title of this handsomely printed book must be regarded as that which expresses the intentions of its author. To the American reader, the work is an introduction to the study of volcanoes, comparable to those of Prof. Judd and others, which are already familiar to us in Europe. Only 170 pages, or half the book, deal with the volcanoes of North America; and it is to these that the professed geologist will most naturally turn. Considered, then, in its dual aspect, Prof. Russell's work must do much to stimulate curiosity and observation in the United States, by pointing out the absorbing interest of volcanic phenomena, and the extraordinary illustrations of the subject possessed by the North American continent.

Vesuvius, as seems inevitable, introduces the description of complex volcanic mountains; and the famous photographs of 1872 repeat themselves on the second plate. Nor will the beginner feel any irritation at perusing the graphic details provided by the younger Pliny. He is directed to Mr Lobley's book on Vesuvius for a history of the mountain; and we should have liked a reference also to the classic work of Phillips. Palmieri's experiences, and the report of the Krakatoa committee, are then drawn on; and these passages conclude with a remarkable quotation, from "one of our most profound students of volcanic phenomena" (p. 28), to the effect that the performances of Vesuvius "are mere Fourth of July fireworks in comparison with the Day of Judgment proceedings of Krakatoa." Prof. Russell's own good taste and style fortunately prevent his imitating this profundity, even where, as in Chapter VIII., he allows himself romantic possibilities.

Dutton and Dana furnish the account of Hawaii, and the Deccan trap and the Newark system of the Atlantic coast are quoted as examples of surface-flows of vast extent. In the latter instance, we may remind ourselves of the wide field of literature and 'solid geology' already traversed by Prof. Russell in the Eastern States.

The terms 'aa' and 'pahoehoe' seem to have gained vitality in geological literature (pp. 60-62), just as we might borrow with profit many of the names by which a Highlander or a Welshman designates the various forms of mountains. Prof. Russell refers the 'aa' type of lava-surface to imperfectly fluid streams, in opposition to what has been stated to occur upon the slopes of Vesuvius. The pahoehoe, on the contrary, flows easily and cools in thin sheets before it can break up, furnishing a smooth and often glassy surface.

The classification and description of igneous rocks professes only to be a sketch, and granite is selected as the deep-seated type, followed by four lavas. Augite or soda-augite, as well as hornblende and biotite, should, we think, have been mentioned among the common constituents of rhyolite (p. 122) and trachyte (p. 124). We do not quite gather the meaning of the description of andesites "as generally dark, and mostly fine-grained rocks, with a restricted amount of glassy base, but larger than in the trachytes."

On p. 127 we commence the study of North American volcanoes, and it is interestingly pointed out (p. 133) that the two ends of the great western line are highly active, while there is a "middle region of extinct or, perhaps, in part, dormant volcanoes, extending from central Mexico through the western part of the United States and far into Canada."

Considering the enormous area to be covered, no one geologist can be expected to have an intimate acquaintance with North American volcanoes; and hence the description of those in Central America and Mexico is necessarily a compilation from works already published. So little is known of this district, however, that we are glad to be put in touch with the amazing history of Izalco, the growth of which, from nothingness to a height of 1500 feet, was witnessed by the curé of Sonsonate during his quiet country life, or the catastrophic explosion of Conseguina in 1835, which seems to have been connected with earthquakes that carried ruin into Chile. Among recent accounts —still far too few—we have those of Prof. Heilprin's ascents in Mexico. There seems some contradiction, however, between p. 178, where we are promised mountaineering excitements on Ixtaccihuatl, and the mere comparison with Popocatepetl on p. 183.

A country that possesses the denuded necks near Mount Taylor (Pl. 6, fig. B), and the exquisitely preserved Ice Spring craters of Utah (Pl. 7), is truly a paradise for the student; but the distances from point to point must still remain obstacles to research. The cone near Lassen's Peak, described by Diller, is of admirable freshness, and may have been active in the present century. Hence there are further volcanic possibilities in store for observers within the limits of the United States.

On p. 234 we gain a conception of the huge chain of peaks, still awaiting detailed enquiry, that runs north from San Francisco into Washington. These seem to result from Tertiary eruptions, and are not unworthy companions of the Andes, which are more familiar to us, owing to the pre-eminence of certain of their summits. The photograph of Mount Rainier (Pl. 14) may be cited, among the beautiful series of illustrations that adorns Prof. Russell's book.

The account of the Columbia lava (p. 250) will interest students of fissure-eruptions. Prof. Russell clearly points out that the enormous area, 200,000 to 250,000 square miles, is not buried in one vast flow; "the lava sheets overlap and supplement one another so as to form a continuous and highly compound system." Individual flows have, however, been traced on cañon-walls for a score or more of miles.

When we reach Alaska, we welcome the photographs of peaks and islands on Plates 15 and 16, and feel more than ever grateful for the records that Prof. Russell has brought together in the American portion of his book. This portion concludes with an account of the great part played by volcanic dust in the soils of many of the western States. A reference to Prof. Judd's paper on the lavas of Krakatoa (*Geological Magazine*, 1888) would have excellently supplemented the important quotation from Diller on p. 293, to the effect that volcanic dust is richer in silica than its parent lava.

In the 'theoretical considerations' of chapter VII., we fancy that there is a certain amount of slaying of the slain. But this walk across the battlefield is in reality of service to learners, who are liable to regard all printed text-books as infallible. The suggestion on p. 314 that "volcanic activity increased with geological ages, and reached its maximum in Tertiary times," is confessedly based on the geological history of North America, and is, we fancy, not even well founded for that area. The amount of denudation that has laid bare the Archæan rocks of the north-east has probably wrought havoc along many old lines of volcanic activity.

Prof. Russell regards the water in lavas as collected during the passage of molten matter, moving under earth-pressure, from the lower into higher and waterlogged regions of the crust (p. 318); and the liquid matter may arise during local relief from pressure, as the product of rocks previously solid (p. 312). Hence he considers steam rather as a variable and unessential factor in determining a volcanic outburst.

The work is, as we have hinted, admirably produced by the publishers. The printers give us 'Roichthofer' for 'Richthofen' on p. 252, and, far more excusably, 'lavæ' and 'lava cases' for 'larvæ' and 'larva cases' on p. 209.

GRENVILLE A. J. COLE.

ANOTHER MEMOIR ON FUNAFUTI

THE ETHNOLOGY OF FUNAFUTI. By Charles Hedley. Australian Museum, Sydney. Memoir III., part 4, 1897.

THE pressing necessity of a systematic and immediate survey of the ethnology of the islands of the Pacific is again brought clearly before us by Mr Hedley's paper. It is only too manifest that the strictly native culture of Funafuti is rapidly dying out, that the older arts, customs, and appliances are changing apace under the influence of European missionaries and traders. In a few years' time the very recollection of the older culture will die out, and it will be impossible to obtain for our museums even models of the former appliances made with any accuracy at any rate. Hence every careful contribution to South Pacific ethnological literature must be welcomed. Mr Hedley's paper does not profess to be in any way an exhaustive monograph, but is, in the main, a descriptive list of the ethnological specimens and models collected by himself and others, and placed now in the Australian Museum at Sydney. Most of these are described clearly and in some detail, while their interest is increased by reference to the resemblances observable between the various native implements of Funafuti and those of other islands, as bearing upon their affinities and upon the probably complex origin of the general culture of the inhabitants of the island. Funafuti seems to have drawn its culture from various directions. Though in the main Polynesian, more particularly Samoan, in general character, there is evidence in the arts and appliances of affinities with Micronesian culture, while even Melanesian traces are not wanting. The curious shell-bladed cocoanut scraper, mounted upon a wooden, elbow-shaped stock, belongs to a type of tool which has been recognised in Matty Island, to the north of New Guinea, while Mr Hedley might have added that the same is also found in the Solomon Islands, and that it crops up again in Ceylon, with a metal blade substituted for the shell. In North India, too, a nearly allied implement is found with a knife-blade replacing the scraper. The details given regarding the various types and the manufacture of fish-hooks are of importance, and point to affinities with the fish-hooks, both of the eastern and the western Pacific groups. The canoes are described in detail. The various implements, toys, etc., are too numerous even to be mentioned here, but all are recorded with care. It is a pity that the term 'drum' is applied to the hollow trough-shaped wooden instrument of Funafuti. This belongs to a very widely distributed type of instruments, which belongs essentially to the gong series, and should on no account be confused with the drums, which are characterised by a sounding medium of tense membrane. The vague descriptions of some travellers constantly confuse the two perfectly distinct instruments, and ethnologists should studiously avoid falling into the same error. Mr Hedley is wrong in supposing that the 'ploughing' method of producing fire by friction is the only one employed in the Pacific Islands. The simple twirling drill has been described from New Zealand, the New Hebrides, and Carolines, and other instances might be mentioned. In this, as in many other instances, a specialist would, no doubt, have added greatly to the information given in the paper, but at the same time Mr Hedley's contribution should prove a useful one, and welcome to ethnologists. HENRY BALFOUR.

A CATALOGUE OF MAMMALS

CATALOGUS MAMMALIUM, TAM VIVENTIUM QUAM FOSSILIUM, a Dr E. L. Trouessart. Nova editio (prima completa). Fasciculus II., Carnivora, Pinnipedia, Rodentia I. (Protrogomorpha and Sciuromorpha), pp. 219-452; Fasciculus III., Rodentia II. (Myomorpha, Hystricomorpha, Lagomorpha), pp. 453-664. Berlin: R. Friedländer & Sohn, 1897. Price, 10 marks each fasciculus.

THE second and third portions of this admirable and most useful list fully bear out the promise of the first part, noticed in *Natural Science* for May. They contain, besides the Carnivora and Pinnipedia, which the author separates ordinally, the whole of the rodents, the most difficult and most numerous order of mammals, and will therefore be most welcome to every working mammalogist. The list seems throughout to be remarkably complete and up to date, and we have scarcely been able to find a single omission. The print and get-up are even better than in the first parts, and the misprints due to some of the specific names being printed with capitals are reduced to a minimum (though not to *nil*). Acting on a suggestion in our previous notice the original localities for the names considered to be synonyms have been printed opposite the latter, so that it can be seen at a glance what forms have been described from different parts of the main area.

En passant we may note two accidental mistakes—the first that a number of rodents from Mashonaland, described by Mr de Winton in 1896, have been wrongly credited to the present reviewer, and the second that by a confusion of two similar names, the marsupial *Thylamys carri*, of Trinidad, appears as a rodent in the genus *Tylomys*.

We may express a hope, in conclusion, that this invaluable list will not be allowed to come to an end without having a good index appended to it, at least of the genera names and their synonyms (with page, not number, references), as this will enormously increase the facility of its use, and will make it really worthy of its claim to be *Editio completa*. O. T.

Parasitic Flies

Revision of the Tachinidae of America North of Mexico. By D. W. Coquillett. Pp. 154. Washington. 1897. (U.S. Department of Agriculture, Division of Entomology. Technical Series, No. 7.)

Like most of the publications of the admirable Washington department, this work will be welcomed both by the systematic student and the practical farmer. The flies described are parasitic on various insects, usually on the caterpillars of moths, hence they play an important part in keeping the numbers of injurious species within bounds. The work contains tables of the parasites with their hosts and the hosts with their parasites, together with synoptical tables, and full descriptions of all the Nearctic genera of species. As most of the genera are identical with those of Europe, the monograph should be valuable to dipterologists generally. We notice with regret that there are no illustrations. In the study of so difficult a family, a few figures of structural details would be of great help to the beginner.

Further Literature Received

Das Kleine Botanische Practicum, E. Strasburger, ed. 3 : Gustav Fischer. Bau und Leben unserer Waldbäume, K. Büsgen : Gustav Fischer. The Span of Gestation, J. Beard : Gustav Fischer. Wild Traits in Tame Animals, L. Robinson : Blackwood. La Face de la Terre, French Trans. of Suess, Das Antlitz der Erde, by E. de Margerie, vol. i.: Colin, Paris. The Living Substance as such and as Organism, Mrs E. A. Andrews : Ginn, Boston. Cheltenham as a Holiday Resort, S. S. Buckman ; Norman, Sawyer & Co : Cheltenham.

On some European Slugs of genus *Arion*, and on two new species of *Parmarion*, W. E. Collinge : *Proc. Zool. Soc.* On Flow-Structure in an Igneous Dyke, and on Augite-diorites, by T. H. Holland : *Rec. Geol. Surv. India.* A Dictionary of the Call Notes of British Birds, C. L. Hett. Field Columbian Museum, Papuan Crania, G. A. Dorsey. Ratzel's History of Mankind, pts. 21, 22 : Macmillan. U.S. Dept. Agriculture, N. Amer. Fauna, No. 13. Concilium Bibliographicum, cards relating to *Nat. Sci.*

Amer. Geol., Nov.; Amer. Journ. Sci., Nov.; Amer. Nat., Nov.; Botan. Gaz., Oct.; Feuille des Jeunes Nat., Nov.; Irish Nat., Nov.; Journ. School Geogr., Oct.; Literary Digest, Oct. 16, 23, 30, Nov. 6 ; Naturae Novit., Oct.; Naturalist, Nov.; Nature, Oct. 21, 28, Nov. 4, 11, 18 ; Naturen, Oct.; Photogram, Nov.; Psychol. Rev., Nov.; Rivista Quindicinale di Psichologia, vol. i., fasc. 9, 11 (Sept., Oct.) ; Revue Scientifique, Oct. 16, 23, 30, Nov. 6, 13 ; Science, Oct. 8, 15, 22, 29, Nov. 5 ; Scientific American, Oct. 9, 16, 23, 30, Nov. 6 ; Scot. Geogr. Mag., Nov.; Scot. Med. and Surg. Journ., Nov.; Victorian Nat., Sept., Oct.; Westminster Rev., Nov.

OBITUARIES

CHARLES SMART ROY

BORN AT ARBROATH, 1854. DIED AT CAMBRIDGE, OCTOBER 4, 1897

THE death of the professor of pathology in the University of Cambridge is a serious loss to the scientific study of that subject. Roy received his training at St Andrews and Edinburgh. His earliest research work, that on the pleuro-pneumonia of cattle, was conducted at London, chiefly in the Brown Institution, of which Burdon-Sanderson was head. Proceeding to Berlin, he studied in the laboratories of Virchow and Du Bois-Reymond, and produced his paper "On the influences which modify the work of the heart," published in the *Journal of Physiology*. In 1879 Roy became assistant at the Physiological Institute of Strassburg University under Prof. Goltz, where he invented the sphygmotonometer and other instruments for measuring the changes in blood-vessels. Here also he invented the well-known ether freezing microtome. Passing next to Cohnheim's Institute at Leipzig, he invented the renal oncometer for the study of variations in blood-flow through the kidney. From here in 1880 he came to Cambridge as George Henry Lewes' student in physiology, and worked in the laboratory of Prof. Michael Foster. In 1882 Roy succeeded Dr Greenfield as Director of the Brown Institution, a post which he held for two and a half years, during which time he visited the Argentine Republic to investigate a disease raging among the cattle of Entre Rios. In 1884 he was elected a Fellow of the Royal Society and to the newly-founded chair of pathology at Cambridge. His chief work was on the mammalian heart, partly carried out in conjunction with Prof. Adami. In prosecuting this research he invented yet other ingenious instruments. Among his students at Cambridge may be mentioned the names of Kanthack, Hankin, Griffiths, Lorrain-Smith, Cobbett, Lloyd-Jones, Rolleston, and Wesbrook. Many of these came as J. Lucas Walker's students in pathology, an endowment which he himself was largely instrumental in securing. Prof. Roy's health had been failing for some time, and for the past year his work at Cambridge has been undertaken by Dr Kanthack, who has now succeeded him.

PETER BELLINGER BRODIE

BORN 1815. DIED NOVEMBER 1, 1897

THE veteran geologist, the Rev. P. B. Brodie, who had been a Fellow of the Geological Society of London for more than sixty years, passed away on the first day of last month. He was born in London in 1815, and early proceeded to Cambridge, where he came under the influence of Sedgwick, and was for some time volunteer assistant in the Woodwardian Museum. His first paper, on land and fresh water shells in association with mammalian bones in the gravels around Cambridge, was read before the Cambridge Philosophical Society so long ago as 1838, but he became best known by his numerous discoveries of fossil insects in the secondary rocks, and in 1845 he summarised all that was then known in reference to this subject in his small illustrated volume, "A History of the Fossil Insects in the Secondary Rocks of England," dedicated to his revered teacher Sedgwick. In the determination of the insects the author was assisted by the late Prof. J. O. Westwood. In 1838 Brodie entered the Church and became curate at Wylye, Wiltshire; in 1840 he removed to Steeple Claydon, Buckinghamshire; next year he was elected vicar of Down Hatherley, Gloucestershire; and in 1855 he became vicar of Rowington, near Warwick, where he died. In all these districts he accomplished much original work, and he contributed several papers to the Geological Society of London, besides others to the British Association and various Field Clubs. In recognition of the value of his labours the Geological Society awarded to him the Murchison Medal in 1887. He amassed a large collection of fossils, of which the more important specimens have been acquired by the British Museum, and he was indefatigable in his exertions to spread an interest in science among those by whom he was surrounded. In 1854 he was instrumental in founding the Warwickshire Naturalists' and Archaeologists' Field Club, and at the time of his death he was President both of this Club and of the Warwickshire Natural History and Archaeological Society. An excellent portrait of Mr Brodie appeared in the *Proc. Warwick Field Club* early this year, and it is reproduced, with an extended biographical notice, in the November number of the *Geological Magazine.*

ANDREW MATTHEWS

Born June 18, 1815. Died September 14, 1897

At the ripe age of eighty-two one of the most accomplished of our clerical naturalists has passed away. The Rev. Andrew Matthews, who had held the living of Gumley, Leicestershire, since 1853, was a close observer of Nature, and interested in botany and ornithology. His fame will rest, however, on his studies of the family of minute beetles known as the Trichopterygidae. He published a beautifully illustrated monograph of these insects in 1872, and we are glad to know that the MSS. and drawings of a second volume had been completed by him in recent years, and will probably be issued shortly. He also contributed the account of these beetles to the "Biologia Centrali-Americana." All collectors of coleoptera are familiar with the catalogue of the British species of the order which he compiled in conjunction with Canon Fowler in 1883. That entomologist, in an appreciative notice of Mr Matthews in the *Entom. Monthly Mag.* for November, claims that the deceased naturalist was worthy to rank with Gilbert White as an observer, while he far surpassed the famous parson of Selborne as a close student of minute structure.

LOUIS CALORI

BORN FEBRUARY 8, 1807. DIED NOVEMBER 1897

LOUIS CALORI, the doyen of the Italian anatomists, was born at San Pietro in Casala, Bologna. His father was a doctor of medicine in practice at San Pietro, and young Louis attended the University of Bologna, becoming M.D. in 1829. In 1830 he was elected assistant professor of anatomy at his University; in 1835 he obtained the chair of comparative anatomy; and in 1844 that of human anatomy. He was ten times President of the Bologna Academy, and that body published a bibliographical list of his works in the fifth volume of the fourth series of the *Actes*. In the same year, 1884, the Academy gave a fête in his honour. Calori's chief zoological work was done among the Reptilia, but his researches threw light on many other groups of the animal kingdom.

CAPTAIN EDWARD YERBURY WATSON, who was killed by a sniper on November 8 at Simla while with Sir Wm. Lockhart's camp, was well known as an authority on the Hesperidae. He had arranged the collection at the British Museum before returning to India. Captain Watson was acting as Deputy-Assistant Commissary-General, and had seen a good deal of service in Burmah. He was promoted to the captaincy in 1895.

DR OTTO VOLGER, whose death was announced at the end of October, was a well-known German educationalist. His chief claim to posterity was his worship of Goethe and his care of the Goethehaus, but he was an indefatigable writer in the teaching of natural history and geology, both in Switzerland and Germany, and has left many minor works on those subjects.

The deaths are also announced of:—

HENRY CALDERWOOD, professor of moral philosophy in the University of Edinburgh, on Nov. 19; LEOPOLD AUERBACH and RUDOLF HEIDENHAIN, professors of physiology in the University of Breslau; HJALMAR HEIBERG, professor of pathological anatomy in the University of Christiania; E. LE GROS, professor of physiology in the new University of Brussels, aged 36; EDMUND DRECHSEL, professor of pharmacology in the University of Berne; GIUSEPPE FISSORE, sometime professor of pathology in the University of Turin, aged 82; W. MARME, director of the pharmacological institute of Göttingen; ALEXANDER MILTON ROSS, author of several works on the fauna and flora of Canada, at Montreal, Oct. 27; Dr MIETSCHKE, the German entomologist and naturalist; MAX SINTENIS, the German entomologist; ISAAC N. TRAVIS, taxidermist at the American Museum of Natural History; Rev. SAMUEL HAUGHTON of Trinity College, Dublin; Dr M. F. HEDDLE, late professor of mineralogy in the University of St Andrews; GEORGE HARRY PIPER, geologist of Ledbury, Herefordshire; JOHN CALVERT, mining expert, aged 86; and WILLIAM SCOTT, director of Royal gardens and forests, Mauritius, aged 38.

NEWS

THE following appointments are announced :—Dr A. A. Kanthack to be professor of pathology, and J. Graham Kerr to be demonstrator of animal morphology, in the University of Cambridge ; Dr A. W. Sheen to be demonstrator of anatomy in the medical department of University College, Cardiff ; Dr Max von Frey, of Leipzig, to be professor of physiology in the High School of Zurich ; Dr W. Rothert, of Kazan, to be professor of botany and director of the physiological division of the botanical department of the University of Charkow ; Henry S. Pritchett, of Washington University, St Louis, to be superintendent of the U.S. Coast and Geodetic Survey ; William S. Carter to be professor of physiology in the University of Texas ; Dr Arthur Allin to be professor of psychology in Colorado University ; Dr Francis Kennedy to be demonstrator in experimental psychology in Princeton University.

SIR ARCHIBALD GEIKIE has been appointed Romanes Lecturer at Oxford for 1898.

DR HANS MOLISCH of Prague intends to spend the coming winter in botanical research at Buitenzorg, Java.

DR ARTHUR WILLEY has returned to Cambridge, where he proposes to investigate his collections during the winter.

THE tenth annual meeting of the Geological Society of America will be held in M'Gill University, Montreal, at the end of December.

LETTERS from Mr C. W. Andrews announce his safe arrival in Christmas Island, where he has already made some progress in collecting.

WE are pleased to learn from Sir Frederick M'Coy that our announcement of his retirement from his professorship in the University of Melbourne is premature.

'THE Louisiana Society of Naturalists' has been founded at New Orleans to investigate systematically the fauna and flora of the State of Louisiana, which has hitherto been much neglected.

THE new building for the Radcliffe Library at Oxford is to be towards the western end of the south front of the University Museum, that is to say, the end where the chemical laboratories are.

A LABORATORY for Experimental Psychology has, says the *Psychological Review*, been opened in the Illinois Eastern Hospital for the Insane, at Hospital, Ill., under the direction of Dr W. O. Krohn.

ACCORDING to *Science*, the University of the State of New York, following the plan it has adopted of lending to the schools libraries and pictures, offers to make loans of specimens of natural history from the State collections.

THE Annual Report of the Preston Scientific Society for 1896-97 records the accession of more than one hundred members during the year. Sections for technical scientific work have been formed, and we look forward to interesting results.

Dr Franz Steindachner superintends the zoological work on board the Austrian ship *Pola*, which this year continues the scientific exploration of the Red Sea, covering the ground between Jedda and Aden. Researches in physical oceanography will also be carried on.

Prof. Schauinsfeld, Director of the Bremen Museum, has returned with a large collection from a voyage of fourteen months in the Pacific. He spent some time on the small island of Laysan, and visited among other places the Sandwich Islands, Samoa, New Zealand, and Chatham Island.

The sum of £500 has recently been granted to the Manchester Museum at the Owens College for the purpose of installing the electric light. We understand that Mr Edward Holt's donation of £2500, lately announced, is to be devoted entirely to the rebuilding of the gymnasium of the College.

The Geological Circle of the Upper Holloway Centre of the London Society for the Extension of University Teaching will issue a strong programme for 1898. We expect great things from a Society with a name of such dimensions, and shall look for the programme with interest. Mr Rudler is the President.

The Imperial Russian Geographical Society is sending an expedition to Abyssinia, chiefly for the anthropological investigation of the country. The leader of the party is Mr Nicolas Dmitrieff, of Astrakhan, who has had some experience of the country as medical volunteer with the Italian army.

Messrs Schuchert and White, of the U.S. National Museum, have returned from Lieut. Peary's las Greenland expedition with a collection of Cretaceous and Miocene plants, as well as many Cretaceous invertebrates from the Noursoak peninsula. They seem to think the last word on these plants was said by Heer, which is hardly the view of European palaeobotanists.

The Jersey Natural Science Association is wisely including in its programme economic questions of general interest. At the October meeting Mr J. Hornell read an important paper on "The Possibilities of Fishery Improvement in Jersey, with Notes on the present state of Marine Pisciculture and Fishery Regulation." It appears in full in the *Jersey Weekly Press* of October 16.

One of the results of the terrible famine in India last year shows itself in the increase of deaths from wild animals. The greatest increase was in the North-West Provinces from wolves, and in the Sunderbunds from tigers, and was, no doubt, the result of a more vigorous search for food by the natives in the jungles. Snake-bites, however, decreased considerably, there being only 21,000 deaths in the year.

An Act was passed at the last meeting of the West Virginia Legislature, establishing a State Geological and Economic Survey in connection with the W. Virginia University, Morgantown. For its expenses a sum of 3000 dollars *per annum* has been appropriated. Dr Israel C. White is Superintendent, Prof. S. B. Brown is First Assistant Geologist and Curator of the Collections, and Professor J. L. Johnson is Assistant Geologist.

The Hull Scientific and Field Naturalists' Club opened its winter session by an Exhibition and Conversazione which ran for two nights—namely, November 10th and 11th, from 7.30 to 10 p.m. The whole of the exhibits were from the collections of members of the club, and testified to commendable activity in many right directions. The President of the club is Dr J. Hollingworth, and the Secretary is T. Sheppard, 78 Sherburn Street, Hull. The annual subscription is four shillings.

Science announces that the United States Geological Survey has practically completed the distribution of the Educational Series of Rocks, 175 suites of 156 specimens each having been sent out during the past summer to universities, colleges, and technical institutions in the United States. There remains a small number of incomplete sets, which will be placed in such smaller colleges as will make them most useful. The Educational Series were prepared by the Survey with much care, for the purpose of aiding students in acquiring a general and special knowledge of rocks and promoting the study of geology.

THE illustrations of geological sections as aids to the geologist who ventures into a museum are familiar to everyone who visits the museums at Jermyn Street, Cromwell Road, and many places on the Continent and in America; but there is one particular section—that built in the gardens of the Landwirthschaftliche Institut of the University of Halle, in honour of Dr Julius Kuehn—which we do not think has been brought to the notice of readers of this journal. It is built up of the rocks themselves, and represents a section through the mountainous district of north and middle Germany. This very striking representation of geology was described by Professor K. v. Isitsch as long ago as 1891. Besides forming an unique memorial to Dr Kuehn, it has considerable value for the teaching of geology.

WE learn from the *Shooting Times* that the Guildford Natural History Society have been considering the question of the preservation of Wolmer Forest, which is only fifteen miles from that town, and have decided to present a petition to the Commissioners of Woods and Forests, praying that Wolmer Forest may be reserved as a sanctuary for wild birds, in which they, their nests, and eggs may remain unmolested throughout the year; that it may not be let at any time for game preserving, or for any purpose inimical to bird life; and that it may remain in perpetuity as a national memorial to the greatest outdoor naturalist England has produced—Gilbert White of Selborne. Such a recognition, the society urge, would show that the admiration of Gilbert White in the nineteenth century was so practical as to be of value to the naturalist and the English-speaking race for all succeeding time. The society have no wish to attempt to interfere with the use of the forest by the War Office for the purposes of military manoeuvres.

AN editorial comment in the *American Naturalist* for October includes some complimentary remarks on the British Association, which will be read in this country with interest:—"We may be pardoned if we point out some features in which we think the British Association superior to our own. In the first place, the Presidential Addresses delivered before the British Association strike us as, on the whole, better than those with which our audiences are greeted. While now and then an American address will rise to as high a standard as anything that Great Britain can boast, theirs are on the average the more thoughtful and scholarly, while ours too often have a perfunctory air and lack in breadth of view. In personnel of those who attend, the British Association again has the advantage. In England it is the fashion to attend these annual meetings, and no one there has reached such a pinnacle of greatness that he can afford to ignore or neglect this national society. As a result, at their gatherings one can be reasonably certain of meeting most of those who are the leaders in English scientific thought. In America, on the other hand, the tendency is in the other direction. It would be an easy matter to give a considerable list of names of those prominent in American science whose faces are never seen at the association meetings."

CORRESPONDENCE

LACÉPÈDE'S TABLEAUX. . . . DES MAMMIFÈRES ET DES OISEAUX; 1799

LACÉPÈDE'S "Mémoire sur une nouvelle table méthodique des animaux à mamelles" was read before the Institute on 21 Prairial an 7 [9 June 1799]. It was published in the Mémoires de l'Institut, vol. iii., in 1801. Louis Agassiz quotes the table as Mém. de l'Instit., iii., 1797, an obviously incorrect date; but many others have quoted it as 1799 without giving any evidence as to the accuracy of the quotation. As great importance attaches to the proper date of this paper, it became necessary for me to investigate the matter carefully, but I could not discover any definite statement except that of Engelmann, who in his Bibl. Hist. Nat. 1846, p. 376, refers to it as "in-4. *Paris. an* vii. (1799). Plassan. (38 pag.)." Up to the present, however, I have completely failed to find this 4to tract; it cannot be a separate of the Mém. de l'Inst. paper because that was printed by Baudouin, and Engelmann gives Plassan as the printer of the tract; besides the pages of the Institute paper are 32 against 38 of the tract referred to by Engelmann. But I have found, quite by accident, in Didot's issue of Buffon's Hist. Nat., 18mo *Paris*, 76 vols., 1799-1806, in the xiv. vol. of Quadrupedes, this interesting tableau, and it is dated 1799, and it was printed by Plassan. The title of the two papers (for the birds are included) are as follows:—"Tableau des divisions, sous-divisions, ordres et genres des Mammifères, Par le C^en^ Lacepède; Avec l'indication de toutes les espèces décrites par Buffon, et leur distribution dans chacun des genres, par F. M. Daudin" (pp. 143-196).

"Tableau des sous-classes, divisions, sous-divisions, ordres et genres des Oiseaux, Par le C^en^ Lacepède; Avec l'indication de toutes les espèces décrites par Buffon, et leur distribution dans chacun des genres, par F. M. Daudin" (pp. 197-346). Both of these are referred to as one tract, "in-18. *Paris*, 1802. Plassan," by Engelmann, Bibl. Hist. Nat. 1846, p. 322.

It will be noted first—That the *Genera only* are by Lacépède; and secondly—that *all* the species are by F. M. Daudin.

As the period 1798-1801 is a critical one for nomenclature, I need do no more than point out the interest of this recovery; but my friend Oldfield Thomas hopes to prepare an analysis of the Mammalia. Ornithologists will find many interesting points to consider in nomenclature when comparing the above paper with Daudin's "Traité Elémentaire," which it undoubtedly precedes.

C. DAVIES SHERBORN,
INDEX ANIMALIUM.

17*th Nov*. 1897.

MR R. QUICK writes from the Horniman Museum, that an examination of Mr Harrison's collection of flints from the Plateau gravels of Kent has convinced him that these are truly primitive implements. He thus agrees with Sir Joseph Pretwich, and considers that Mr Cunnington's reasoning in our last number (pp. 327-333) is not cogent.

ERRATUM.—Page 321, line 25. *For* 'Siberian' *read* 'Silurian.'

NOTICE

TO CONTRIBUTORS.—All Communications to be addressed to the EDITOR of NATURAL SCIENCE, at 29 Bedford Street, London, W.C. Correspondence and Notes intended for any particular month should be sent in not later than the 10th of the preceding month.

TO THE TRADE.—NATURAL SCIENCE is published on the 25th of each month; all advertisements should be in the Publishers' hands not later than the 20th.

TO OUR SUBSCRIBERS AND OTHERS.—There are now published TEN VOLUMES OF NATURAL SCIENCE. Nos. 1, 8, 11, 12, 13, 20, 23, 24 being OUT OF PRINT, can only be supplied in the set of first Four Volumes. All other Nos. can still be supplied at ONE SHILLING each.

Price of Set of Vols. I., II., III., IV.	. . . . .	£2 10 0
,, ,, ,, V., VI., VII., VIII.	. . .	1 4 0
,, ,, ,, I.—VIII.	. . . . .	3 10 0

One Shilling each Number of any Bookseller.

Annual Subscription, payable in advance to J. M. DENT & Co., 29 Bedford Street, London, W.C., Thirteen Shillings ($3·50), post free.

CHANGE OF ADDRESS.

Messrs J. M. DENT & CO. beg to give notice that from January 1, 1898, the Publishing and Editorial Offices of *NATURAL SCIENCE* will be removed to their new premises, 29 Bedford Street, W.C.

INDEX

An Asterisk (*) indicates that a figure is given.