main herd; and, thirdly, the females, all of whom of two years old and upwards are eligible for breeding. The old males land on the islands first and take up stations along the shore; when the females arrive they are immediately seized by the males, each of whom, by force or blandishments, collects a number of females into a harem of, on an average, about eighteen individuals. During the summer all the females are fertilised, and owing to the peculiar bilobed structure of the uterus each female can give birth to a pup every year, the opposite sides being used in alternate years. Hence as the period of gestation is 355 days, it follows that, except for ten days in the summer, every female seal above two years old is always pregnant. Moreover, owing to the extraordinary jealousy of these animals, no mother-seal will give suck to any pup except its own. Hence if a mother be killed between the birth of her pup and the end of October or beginning of November, when the young are weaned her pup is inevitably starved to death. Thus every female seal killed between June and October, both months inclusive, means the death of three seals—a mother, a pup, and a foetus. At any other time of the year every female killed means the loss of two lives. At present there is a close time for seals in the Eastern Behring Sea for May, June and July. A healthy pup takes [illegible] days in which to starve to death, and it is, therefore, very significant that after the middle of August vast numbers of young pups are found on the Pribylov Islands starved to death. It is admitted by both sides that many, possibly a great majority, of the deaths of these pups are due to the killing of the mothers at sea. Hence the Americans say that the close time ought to be extended at least until the end of October, a decision which would practically mean the end of pelagic sealing.

It may be retorted that if the Canadians are to be debarred from killing fur-seals at sea the Americans ought to be prevented from killing them on shore. But the conditions are totally different. On shore only non-breeding males with perfect skins are killed. No females or breeding males are taken. But at sea no such selection is possible; the sex cannot be determined until the seal is killed. Many of the seals escape with fatal wounds, and as the females are less active than the males and are often hampered by the presence of their young, they are more easily captured. Hence the majority of the seals killed at sea are females. The economic value of the pelagic seal industry is now insignificant, and as it appears to be admitted by both sides that the herds of fur-seals are being greatly reduced in numbers by the excessive killing at sea of female seals and the consequent starvation of nearly 20 per cent. of the young, it is to be hoped that effective measures may be taken to prevent this inhuman and wasteful slaughter.

The Lepidosiren of South America

At the last meeting of the Cambridge Philosophical Society on December 6, Mr J. Graham Kerr gave a short account of his recent expedition to the interior of the Gran Chaco of Paraguay for the purpose of investigating the habits and development of *Lepidosiren paradoxa*. It will be remembered that Mr Kerr was aided by a grant from the Balfour fund, and was accompanied by Mr J. S. Budgett of Trinity College.

Lepidosiren occurs in considerable quantity in the swamps towards the centre of the Gran Chaco boreal. It is sluggish in habits, wriggling slowly about among the thick vegetation of the swamp. At short but very irregular intervals it visits the surface and takes a breath of air. Its food consists mainly of large Ampullarias and masses of confervoid algae. The young are to a greater extent vegetable feeders than are the adults. Lepidosiren makes a burrow in the ground at the bottom of the swamp and lines it with soft grass. In this the eggs are laid. The papillae on the hind limb of the male grow out into long filaments during the breeding season, and during life these are blood-red in colour. They appear to be ornamental structures.

The eggs are very large—about 7mm. in diameter. Coelomic eggs have a thick gelatinous coat: in fertilised and developing eggs this becomes thin and horny. Segmentation is during its later stages holoblastic and unequal. Gastrulation takes place in a manner which recalls that of Urodele Amphibia, and of Cyclostomes. Eventually a tadpole larva is hatched out. This develops large external gills and a very large sucker of the Amphibian type.

The external gills and sucker disappear about six weeks after hatching. At the same time the colour of the young Lepidosiren becomes much darker and the animals become much more lively in their habits. For the first ten to twelve weeks of their free existence the young do not eat but live on the yolk in the walls of their intestine.

A remarkable habit of Lepidosiren was observed, namely, that their normally very dark colour becomes nearly white during the night. The black chromatophores shrink up during the hours of darkness—large yellow chromatophores, which are also present, remaining expanded.

During the dry season the Lepidosiren retreats into the mud in which it remains breathing by means of an air-hole until the waters return and set it free.

Matters Molluscan

The fourth and last note by Mr Félix Bernard on the development and morphology of the hinge in bivalve Mollusca has just been received (*Bull. Soc. Géol. France*, sér. iii., tom. xxv., pp. 559-66, figs.). In the third note (vide *Nat. Sci.*, vol. x., p. 88), he dealt with the Anisomyaria, in the present one he deals with certain of the Heterodonta which had been passed by in his first note. The Unionidae, however, still remain to be studied. The author finds that the Heterodonta naturally fall into two groups which he provisionally, and without at present attributing any taxonomic value to them, calls the 'Lucinoid' and the 'Cyrenoid.' To the former he now refers the Cardiidae (cockles), Donacidae, Corbulidae, the Tellinacea, the Pholadidae, Trigoniidae, and Dreyssensiidae.

This decision as regards the last-named family is very important, in that it confirms the conclusions of competent anatomists that it was not, as always previously maintained, allied to the Mytilidae. The Chamacea and Rudistes prove a highly interesting group with respect to their development, for the hinge of the normal *Chama* is in its early stages Lucinoid; in the 'reversed' individuals, on the other hand, and in the Diceratidae, it is Cyrenoid, whilst in the Rudistes there is great diversity.

We still await with interest the yet promised monograph on the whole subject. Meantime, those who care for the study will find some very important theoretical suggestions in Mr Bernard's paper on *Conchylocardia* (*Journ. de Conchyl.*, July 1896 [1897], 1 pl., figs.).

These minute little bivalves, the largest of which only attains in its maximum diameter 1.3 mm., constitute a new type of pelecypod, and in their hinge present an early stage of the development of that in the Carditidae, Astartidae and Crassatellidae, especially the first-named. Not that they are to be regarded as an ancestral form; on the contrary, Mr Bernard considers the case to be one of neoteny (or paedogenesis). The genus would be derived from a normal type of *Cardita*, in which reproduction began early and which would thus be arrested in its growth. A similar condition of affairs is manifest in *Philobrya*, which genus forms the subject of another paper (*Journ. de Conchyl.*, Jan. 1897, 1 pl., figs.). *Hochstetteria*, if not generically identical with this, is at least very nearly allied indeed, and both present close affinities with the Aviculidae. Dall has stated very positively (so much so that we have quoted it as a fact—vol. xi., p. 341) that *Philobrya* passes through a glochidial stage; but it appears that this was an ingenious but unverified hypothesis based on an apparent similarity in shape between the young shells of this genus and of the Unionidae. Yet another paper by Mr Bernard (*Ann. Sci. Nat. Zool.*, sér. viii., tom. iv., pp. 221-52, 2 pls., figs.) describes the detailed anatomy of *Chlamydoconcha orcutti*, Dall, which might be described as a marine bivalve slug, for the two valves of the shell are reduced to two long narrow plates, and entirely bedded, opened out side by side, in folds of the mantle. All trace of an adductor muscle is wanting. Otherwise, in its internal anatomy, *Chlamydoconcha* closely resembles *Ephippodonta*, described by Martin F. Woodward, in *Proc. Malac. Soc.*, vol. i., pp. 20-26.

A Comprehensive Work on Moths

The British Museum (Natural History) has entered on a gigantic undertaking. Although publicity is especially desired, the authorities apparently have not considered it necessary to send their circular to the general scientific press. We accordingly reprint the circular issued by Sir William Flower from the *Entomologists' Monthly Magazine* for November:—

"Prospectus of a series of Volumes on the Lepidoptera Phalaenae of the Whole World, to be published by the Trustees of the British Museum.

"The Trustees of the British Museum having sanctioned the publication of a series of volumes on the Moths of the World, and entrusted Sir George Hampson with the commencement of the work, I beg to call your attention, as being interested in the classification of the Lepidoptera, to the following scheme, which has been approved of for the work. The chief want of entomologists working at this subject at the present time is a comparative analysis of the genera in each family, on the lines of the classification originated by Herrich-Schaffer and Ledorere for the European fauna, and amplified of late years by Snellen, Meyrick, J. B. Smith, E. L Ragonot and others, for various groups of the Palaearctic, Nearctic, Oriental and Australian faunas, and it is thought that the best way to supply this want is by the publication of such a work as the following:—

"(1) The size of the work to be large 8vo, similar to the Catalogue of Birds, Reptiles, &c., each volume to consist of about 500 pages, the exact number being regulated with a view to completing the classification of a family or sub-family; each volume to be complete in itself, with its own index.

"(2) The general arrangement of the work, and of each family and genus, to be from the more specialised to the most generalised forms, the arrangements being modified so as to make the system as natural as possible.

"(3) The work to contain synopses and descriptions of the families, genera and species of Moths, every described species, about which any exact information can be gained, being included, references only being given to those whose systematic position cannot be ascertained with tolerable exactitude, and no new species being described except such as are represented in the British National Collection.

"(4) No catalogue of specimens will be included, but a somewhat full and minute account of the range of the species, references to all 'types,' to collectors' names when known, and to series of specimens of which an account is given in published works.

"(5) The 'type' of each genus will be indicated by being placed after the reference to the genus, whether it be retained, treated as a sub-genus or synonym.

"(6) The references to the species will be confined to the original reference, to one for each synonym, with dates of publication, to the best figure of the species, to the best description and figure of the early stages, and to such faunistic works and catalogues as are of most general use and importance.

"(7) Local races will be treated as sub-species under sub-headings, with their own references and synonymy.

"(8) Each genus, and all the more prominent sections of genera, will be illustrated by process blocks in the text, showing the facies of a typical species of the genus or section, and all the more prominent details of structure on which the genus is founded.

"(9) An atlas of coloured 8vo plates will also be issued, giving half-figures of as many as possible of the species which have never before been satisfactorily figured, especially of 'types' in the British Museum. This will be issued in parts, as is convenient, and will be sold separately from the volumes; it will contain no letterpress except the explanation of the plates, giving the names of the species figured, reference to the pages of the volume in which the descriptions will be found, and the country where the species will be found.

"(10) The order of the families will, in the main, follow that adopted by Mr E. Meyrick in his recent work on British Lepidoptera, and, commencing with the Syntomidae, will work down through the Arctiadae and Agaristidae to the Noctuidae and Lymantriadae; then, beginning again with the Saturniadae and their allies, will work downwards by the Sphingidae to the Notodontidae, Geometridae and Uraniadae; then by the Lasiocampidae, Limacodidae, Cossidae, Psychidae and Castniadae to the Zygaenidae; then by the Drepanidae and Thyrididae to the Pyralidae, Sesiadae and Tineidae; and ending with the Hepialidae and Micropterygidae.

"It is obvious that complete success in carrying out the above scheme will depend on the willingness of entomologists and museums to send specimens of described species which are not available in London, and the authorities of the British Museum (Natural History) hope that as much help in this way, as possible, may be given when application is made for the loan of specimens, which, when examined and figured, will be carefully packed and returned, the Museum paying the carriage going and coming."

(Signed) "W. H. FLOWER, Director."

The chief points to be noted in the above scheme are the publication of both text and plates in octavo, a much more handy form than quarto; the promise to figure unfigured forms, a promise of inestimable value; and the inclusion of a proper index in each volume. We trust the bait held out to foreigners, the figuring of their unfigured species, will induce those who wish to further the science of entomology to heap coals of fire on an institution which cannot itself (and in our opinion quite properly cannot) allow types to leave its walls.

A PARASITE ON COCKROACH EGGS

MR E. BORDAGE, director of the Natural History Museum of the island of Réunion, has been able to throw some light on the life-history of the obscure hymenopterous genus *Evania* (*C. R. Acad. Sci.*, 1896). The insects of this genus were known to pass the larval stage parasitically at the expense of cockroaches. By breeding a specimen of *E. desjardinsii* from a cockroach's egg-capsule, Mr Bordage has confirmed the opinion that the evaniid grub feeds on the eggs, and not in the body of its host. He has observed a female *Evania* pursuing a female *Periplaneta americana* in the act of extruding her egg-capsule. As this *Evania* is presumably a Mascarene insect, while the cockroach is an introduced American species, it appears that the parasitic grub can be reared in any blattid egg-capsule which affords it enough food and room, and that each species of *Evania* is not confined to a particular cockroach as its host.

LEG-CASTING AMONG STICK-INSECTS

SOME interesting observations on the limb-casting of two species of Stick-insect (Phasmidae) are also due to Mr Bordage (*C. R. Acad. Sci.*, 1897). These insects occasionally threw off a leg that had been seized or bitten by an ant, or held or irritated by the experimenter. It was found that larvae parted with their limbs much more readily than adults. The insects have the power of replacing the lost leg, but, as might be expected, the newly-formed limb is shorter than its fellow which has not been thrown off. An interesting point is, that the foot of a regenerated leg has only four tarsal segments instead of the normal number, five. Mr Bordage mentions the observations of Messrs Bateson and Brindley on variation in the number of tarsal segments in cockroaches' feet. They regarded five as the primitive number of segments; Mr Bordage, on the other hand, believes that the appearance of a four-segmented foot in a species with normally five segments, must be considered as a case of atavism. We are inclined to think that the presence of the five-segmented foot in so many orders of insects raises a strong presumption that it is a primitive character, and that a reduction in the number of segments has been acquired in certain families; especially as the reduction is often most marked in insects which are most highly modified for aquatic life.

SWEDISH ARCTIC EXPEDITION

IN addition to the information which we were able to furnish in our last number, we now learn that Professor Nathorst has secured a vessel, namely, the *Antarctic*, in which, it may be remembered, Mr Borchgrevink accompanied Captain Kristensen and Mr Bull to the south polar regions in 1893-95. The measurement of an arc of the meridian on the east coast of Spitzbergen will, it is hoped, be undertaken by another expedition, specially equipped for that purpose, and will be the task of more than one year. The Swedish organiser of this work is the astronomer Professor Jaderin, and it is hoped to obtain the co-operation of Russia.

Apropos of Mr Borchgrevink, who is being financed by Sir George Newnes, we note that he has bought the s.s. *Pollux* for his forthcoming Antarctic expedition.

GERMAN DEEP SEA EXPEDITION

WE learn from the London *Daily Chronicle* that a parliamentary paper has been circulated among the members of the Reichstag with reference to an item of Mk.300,000 (£15,000) which will appear in the Estimates in support of a German deep sea expedition. This document passes in review the successful efforts made by other maritime nations in this direction, and notably by Great Britain, with the remarkable 'Challenger' expedition. Germany, it is pointed out, is the only European nation with any pretensions to maritime power which has taken little or no part in revealing the secrets of the deep seas, and it behoves her now to make amends for her past negligence, especially when the work to be done is still so vast and so varied. Accordingly, it is proposed that a suitable steamer shall be chartered and leave Germany next August. Her course would be first between the north of Scotland and the Shetland Islands, and then south to the Canaries and Cape Verde, along the West African coast to the Cape, and to the currents of the Antarctic. Special investigations will be made in that region where the cold currents of the Antarctic and the warm Indian currents meet. The expedition will then cruise through the Indian Ocean and the Red Sea, reaching home after an absence which is estimated at nine months.

We are able to add that the leader will be Professor Chun, director of the Zoological Institute of Breslau. The success of the expedition, if started, will therefore be assured.

The Cricket as Thermometer

THE *American Naturalist*, despite its change of editors, shows no falling off in vigour. The November number contains a novel observation by Mr A. E. Dolbear, and since his note is only a little one, we venture to steal it whole:—

"An individual cricket chirps with no great regularity when by himself and the chirping is intermittent, especially in the daytime. At night when great numbers are chirping the regularity is astonishing, for one may hear all the crickets in a field chirping synchronously, keeping time as if led by the wand of a conductor. When the numbers are so great, the resting spells of individuals are unnoticed, but when the latter recommence they not only assume the same rate but the same beat as the rest in that field. The crickets in an adjoining field will have the same rate, that is, will make the same number of chirps per minute, but with a different beat, as one may easily perceive by listening.

"The rate of chirp seems to be entirely determined by the temperature, and this to such a degree that one may easily compute the temperature when the number of chirps per minute is known.

"Thus at 60° F. the rate is 80 per minute.

"At 70° F. the rate is 120 a minute, a change of four chirps a minute for each change of one degree. Below a temperature of 50° the cricket has no energy to waste in music and there would be but 40 chirps per minute.

"One may express this relation between temperature and chirp rate thus:

"Let T stand for temperature, and N the rate per minute.

$$T = 50 + \frac{N - 40}{4}$$

"For example. What is the temperature when the concert of crickets is 100 per minute?

$$T = 50 + \frac{100 - 4}{4} = 65°."$$

This is an observation suggestive of further inquiry. First, we should like to know whether it applies to all kinds of chirping or stridulating insects. And incidentally we may ask—what is the species, or even the genus, referred to by Mr Dolbear? There are

are least six species, belonging to two genera, of what are ordinarily called crickets in North America. Those good people who ask us to use plain English names, instead of crackjaw Graeco-Latin compounds, would, we imagine, be hard put to it to decide what Mr Dolbear means by a cricket. The only sure thing is that he does not mean the 'cricket on the hearth,' which is what we usually understand by the cricket in England. Further, it would be of interest to learn whether this regular relation to temperature is not affected by moisture in the air, by altitude or barometric pressure, by locality generally, or by other varying conditions of existence. Until these questions are answered it is not likely that any species of cricket will prove a serious rival to Fahrenheit, Celsius, or Réaumur.

Dr William Francis and the "Annals"

The December number of our venerable contemporary, *The Annals and Magazine of Natural History*, completes volume xx., the last volume of the sixth series. This is the end of the 120th volume, and the 60th year of the *Annals'* existence, the first number having been published in January 1838 under the title "Annals of Natural History; or Magazine of Zoology, Botany, and Geology (being a continuation of the 'Magazine of Zoology and Botany,' and Sir W. J. Hooker's 'Botanical Companion'), conducted by Sir W. Jardine, Bart., P. J. Selby, Esq., Dr Johnston, Sir W. J. Hooker, Regius Professor of Botany, and Richard Taylor, F.L.S. London: Printed and published by R. & J. E. Taylor." The *Annals* is, therefore, the oldest magazine devoted to the natural sciences existing in this country, and we, as one of the youngest, offer to it our homage and congratulations on this its Diamond Jubilee.

Others who have assisted in the editorship of the *Annals* have been David Don (1840), C. C. Babington (1842), J. H. Balfour (1842), J. E. Gray (1858), A. Henfrey (1858), William Francis (1859), W. S. Dallas (1868), A. C. L. G. Günther (1875), W. Carruthers (1878). Although the name of Dr Francis does not appear on the title page until 1859, since when he has been the really responsible editor, we learn from the preface to the second series that he was intimately connected with the editorship from the beginning. We doubt if there is anyone who can show a longer record of unostentatious, unremitting work in the cause of science. It is therefore not surprising to learn from an announcement in this December number, that Dr Francis, to use his own language, avails himself of this occasion to pass the responsible editorship over to his son, who, for some years past, has largely assisted him in the management, and who is fortunate in retaining the co-operation of the two co-editors whose names appear on the title page. The good wishes of all working naturalists will follow Dr Francis into his retirement, and accompany his son on the future course of the *Annals.*

Bulletin of the Liverpool Museums

Dr H. O. Forbes, the director of the Derby and Mayer Museums at Liverpool, is certainly to be congratulated on the form in which his publication, entitled *Bulletin of the Liverpool Museums under the City Council*, makes its first appearance. As for the appearance itself, we need not repeat, after so short an interval, the remarks that we recently felt urged to make as to the multiplication of scientific periodicals. The object of the present publication is to make known the results of the investigations carried on in the laboratories attached to the museums, and to record the observations made on the animals living in the Aquarium. It is to be issued quarterly, at an approximate price of 8s. per annum. The price of the first number, stated on the wrapper (as we are glad to see) to have been issued on August 4, is 2s. 6d. It leads off with an account of the Derby and Mayer Museums, with portraits of their founders, the thirteenth Earl of Derby and Mr Joseph Mayer, also of Sir William Brown, who provided the building for the public library. The bulk of the number, eighteen pages, is made up of the first instalment of a "Catalogue of the Parrots (Psittaci) in the Derby Museum," by Henry O. Forbes and Herbert C. Robinson; this is illustrated by two coloured plates drawn by Mr J. Smit. We understand that this catalogue will eventually be issued as an independent volume. The "Notes from the Museums and the Aquarium," which conclude the number, contain: an account of a parasitic copepod, *Penella blainvillei*, attached to a flying fish, and itself covered with a colony of small cirripedes, *Conchoderma virgata*; the history of an ordinary stickleback living in salt water; a description of the successful feeding of young trout on powdered sheep's liver; notes on *Malapterurus electricus*; remarks on recent acquisitions by the Mayer Museum, viz., Cypriote antiquities, Neolithic flint implements from Egypt, West African masks. The last note of all being both short and interesting, we quote it in full as a sample of the rest.

'Medicine' at the Liverpool Museums

"It may not be without interest, from a folk-lore point of view, to place on record that, but a few days ago, an Irish lad suffering badly from scrofulous sores was brought to the Mayer Museum by his parents, who earnestly besought the authorities that they might be allowed to touch the child's neck with an Irish Stone Celt, exhibited in one of the cases. It was unavailing to try and persuade the deluded and superstitious couple that no possible good could follow such an application. As their faith in the efficacy of the Stone could not be shaken, and they were loth to go away without being allowed to try this, in their belief, unfailing remedy, opposition was finally, and not without some hesitation, withdrawn, and the ancient implement placed in their hands. After the operation the parents departed happy, grateful, and in the most perfect confidence that their child would be healed, and not without expressions of surprise that so great a boon had been conferred on them without the fee which they were prepared, and that very gladly, to pay."

Geology in Japan

A general account of the Geological Survey of Japan was given in *Natural Science* for February 1894 (vol. iv., pp. 105-111). We are glad to note that the cartographical labours of this survey are making good progress. A map of Japan proper, in five divisions, on a scale of 1 : 100,000 is already completed, and special maps on a scale of 1 : 200,000 have been published for about half the same area. These are accompanied by explanations, written in Japanese. Besides this, numerous detailed surveys on a larger scale are constantly being carried out to meet various official and private requests. Often these are in connection with earthquakes and eruptions, with which geological phenomena Japan is so favoured, and which often seriously affect work in the mining districts. An examination of the water-bearing strata in the Yokohama, Chiba, and Gumma prefectures has recently been concluded. Preliminary and special surveys have been made in the northern island, Hokkaidō. In connection with the war with China, a special geological and agronomical reconnaissance was made of the Liao-tung peninsula. As a result of that war the geological survey of the empire will soon be carried into Formosa. On this island a few fragmentary notes, stated by the Japanese authorities to be very imperfect, have been published by Gordon, Von Richthofen, Guppy, Kleinwächter, and others. Reports on the coal and gold found in the island have been issued by Sotokufu; and the geography of the Taiwan islands, written by Mr Ogawa, contains some references to their geology and agronomy. Lately, Dr Kotō, professor of geology at Tokyō University, and his assistant, Mr N. Yamasaki, have travelled round the main island.

There is in Japan no separate body like our Ordnance Survey, but the Geological Survey has to make its own topographical maps. All the work to which we have here alluded is carried on by a staff of six geologists, four topographers, and eight cartographers.

A New Manure

The above account does not exhaust the work of the Geological Survey of Japan, for it says nothing of the eight chemists and seven agronomists. The agronomical section treads on the heels of the geologists, studying the relations of the soils to the underlying rocks and investigating their capabilities.

In 1893 this section made a very important discovery, which it has since been carrying to a conclusion. A fruitless search had long been conducted for deposits of mineral fertilisers, but in that year a phosphate deposit was found in marine beds supposed to be of Miocene age, in Hyūga province in Kyūshū, extending over an area about 40 kilometers long by 6 wide. Within this area phosphatic marl occurs sporadically in nodular layers, chiefly along the sea-coast. The nodules contain some 20 per cent. of phosphoric acid, and a very small quantity of calcium carbonate, so that the extraction of the acid is rendered more easy. An attempt is being made to produce a super-phosphate by extracting the acid and pouring the liquor on the powdered phosphate. The process has not yet reached such perfection as to be a commercial success; but experiments with pot-plants have shown that the fertilising powers of the new phosphate are considerable. The origin of this peculiar deposit is not yet understood: it certainly is not derived from fossil vertebrates

One is always hearing about the depressed state of agriculture in Great Britain, and we do not suppose that the mere existence of a corps of agronomists would prove an all-sufficient remedy. But when we see other countries, Germany, Russia, America, Japan, and the rest, making exhaustive studies of their soils, and ever searching out new tracts capable of cultivation, or new substances of value to the farmer, then we ask—Where is our Agronomical Department?

Geology in Queensland

From the *Annual Progress Report* of the Queensland Geological Survey for 1895, just to hand, we learn that Mr Sydney B. J. Skertchly was appointed Assistant Geologist on 29th June 1895. Since then Mr Skertchly has been examining the Deep Lead of tin at Herberton, the tin and copper mines in the Watsonville, Montalbion, and Chillagoe districts, the gold fields of Gate River and Marceba, and the Coal Measures of Rockhampton. Mr Jack in his report mentions the occurrence of coal-seams at various localities near Brisbane, and regards the district as likely to prove payable. We have also received two reports by W. H. Rands on the Eidsvold Gold Field, with one on the Croydon Gold Field and one on the Horn Island (Torres Straits) Gold Field. Mr Jack reports on the Hodgkinson Gold Field and the Brovinia (or Brorinia) Gold Field. From a note by Mr Jack on traverses of the Bunya Bunya Range we extract the following sentences of more purely geological interest:—

"The road from Macalister to Jimbour lies over black-soil plains which the previous day's rain had made very heavy. The black soil is evidently derived from the detritus of the basaltic high grounds, which are first met with at Jimbour. It is probable, however, that the sandstone and shales of the Mesozoic Coal Measures immediately underlie the soil beneath the road. At Jimbour, near the edge of the basalt, a coal-seam is met with at the depth of 60 feet. . . . The basalt is disposed in several thick beds, which once formed continuous lavaform sheets. They dip to the south-west, or perhaps, more correctly speaking, have been spread out over a surface gently sloping in that direction. They might at first sight be supposed simply to overlie the Triasso-Jurassic Coal Measures, and therefore to be of Tertiary age; but as we occasionally find sedimentary strata of similar character to those of the Coal Measures interposed between two beds of the basalt, there can be little doubt that the latter are older than the outbursts of Tertiary lavas, and in fact contemporaneous with the Coal Measures. There are, however, strong grounds for suspicion that the basalts had suffered considerable denudation before the deposition of the sedimentary rocks west of Jimbour; that, in fact, there is an unconformity within the Trias-Jura area. A great thickness of the basalt beds is met with near the centre of the 'range,' whereas at Jimbour and the other western outposts of the denuded basalt only the lowest bed is seen. . . . It is evident from the sections of Ironpot, Boughyard, and Jumma Creeks that, as the base of the volcanic series (basalts) rests directly on the granite, the whole of the immense thickness of sedimentary beds lying between the Toowoomba basalts and the Brisbane tuffs is locally absent. In other words, there is what is called in geology an 'overlap.' The base of the Triasso-Jurassic series as seen at Brisbane is on a much lower horizon than the base of the same series as seen at Ironpot Creek. The lower beds of the series had to be built up so as to fill up existing depression in the surface, before the upper beds were deposited at such an altitude as to cover the higher portions of the old surface."

Disease in Lilies

From the U.S. Department of Agriculture (Division of Vegetable Physiology and Pathology, Bulletin 14) we have received a preliminary account by Mr Albert F. Woods of investigations into the cause of the spotting and distortion of the leaves and flowers of lilies, which have given much anxiety to American horticulturists. He believes the disease to be due to a combination of causes—a weak stock of bulbs, careless cultivation, and the attacks of mites, aphids and fungi. It is hoped that a rigorous selection of bulbs for propagation and a more scientific system of culture will check the progress of the disease. It is startling to read in a publication of this department that "the bulb mite (*Rhizoglyphus echinops*) is a white, sluggish insect with brown legs."

I

The Classification of the Day Butterflies

In classifying the day butterflies systematists have availed themselves of external characters, as a rule without subjecting these to a test, by way of ascertaining their comparative value. Now, each organ, or part of an insect, presents taxonomic character relatively stable and primary, and again relatively unstable, recurrent, or adaptive and secondary. It is a peculiarity of the secondary characters that they engage the attention, only in the end to disappoint our efforts to use them in establishing a system,—one which shall not constantly leave exceptions upon our hands to be accounted for otherwise than by the system itself.

There exists, also, the prevalent belief, that there is really a structurally more advanced leader to that host of butterflies which is always pressing forward out of the future to appear in the present and vanish into the past, and that the rank and file of the butterfly phalanx emulate this favoured one, or its type, in structural perfection. A direction in the specialisation of this or that organ—yes; an ultimate goal which all are alike striving to attain and in which some are merely outstripping the others—no. Rank must always remain a relative conception, and, to avoid a confusion of ideas, it is better to substitute for it, in natural history, the terms *specialisation*, as denoting advanced, and *generalisation*, as signifying retarded development of organic features.

We can illustrate what we mean by taking up one or another of the systems which have been put forward as an arrangement of the day butterflies, and no better one can present itself than that advocated by Mr Scudder in his book on the 'Butterflies of New England' and elsewhere. Here the Satyrids, or 'Meadow Butterflies,' are placed 'at the head' (to use the characteristic phrase of the Philadelphia Check List), while the 'Swallowtails' are inserted between the 'Blues' and the 'Skippers.' That, through this proceeding, Mr Scudder has been led into the mistake of separating two groups, which belong phylogenetically together, by the interpolation of a third, not at all nearly related to either, we expect to prove. The mistake would seem to have been committed by Mr Scudder through a want of discrimination between adaptive or secondary and primary characters, as well as by neglecting the lessons taught by a study of the neuration or veining of the wings. But first we will hear Mr Scudder:

"It is well known that as a general rule moths undergo their transformations to chrysalis within a cocoon, spun by the caterpillar, or in a cell moulded beneath the surface of the ground. The same is true of the lowest family (*i.e.* of Day Butterflies) Hesperidae, which usually make such a cocoon within a rolled-up leaf or cluster of leaves, and hence had given them by Boisduval the term Involuti. It was not noted by him, nor has it been, as far as we are aware, by any author, though figured by many, that within this cocoon they generally, perhaps always, spin a pair of shrouds, into the middle of one of which they plunge their cremaster, while by the other they support the middle of the body.

"Now, remove this outer cocoon and leave the shrouds, and one has, with only such changes as are absolutely required by the lack of the encircling cocoon, the character of the support of the chrysalis of the Papilionidae, *viz.*, a button of silk attached to the object from which the chrysalis hangs, and a loose girt around the middle of the body. In the Lycaenidae, we pass simply to a still closer attachment of these fastenings, so that the rounded chrysalis appears almost glued to the surface to which it is attached; and these two families, the Lycaenidae and the Papilionidae were classed by Boisduval under his Succincti. In the Nymphalidae, by the loss of the median girt the chrysalis hangs suspended by its hinder end, and forms the group termed by Boisduval Suspensi or Penduli, which he and his followers interpose between the Involuti and the Succincti. Yet we have here a regular progression from the cocoon of the moths to the almost total absence of the use of any silk of the quiescent period of life. Even the few exceptions to this rule seem to be entirely explainable as instances of reversion. Thus the only case among the higher butterflies where a cocoon properly speaking is made, is in the subfamilies most closely allied to the Hesperidae, among the group of Parnassidi and Anthocharidi; and again in exceedingly feeble instances where the necessities appear to be overwhelmingly great, among some of the higher Nymphalidae, which have lost even the last remnant of the cocoon of moths, *viz.*, in some of the Satyridae, which lack cremastral hooks and undergo their transformations ordinarily in the rudest form of a cell which they can construct above or at the surface of the ground, by the mere movements of the body and the spinning of one or two threads of silk."

I think, from the above extract, it is clear that Mr Scudder rests his classification upon considerations drawn from the peculiarities of the mode of spinning, *i.e.*, the preparations made by the active larva for the protection of the quiescent chrysalis. When we admit that the habit of spinning a 'shroud' within the cocoon by *Hesperia* may represent a stage preparatory to an abandonment of the making of a cocoon altogether, we make Mr Scudder a very ample admission. Yet upon this basis Mr Scudder proceeds, ignoring striking and important differences in structure, to place the Hesperiadae next to the Papilionidae, implying that the papilionid chrysalis is the immediate phylogenetic successor of the hesperid, and, consequently, that *Hesperia* represents the next ancestral form of *Papilio*. Granting that the hesperid cocoon represents an ancestral, generalised stage, one which may have led to the waist-tied chrysalis, there is no reason why the papilionid waist-tied chrysalis should be the successor, seeing that there are other Succincti to be considered. But, for Mr Scudder and his system, this is not an objection, because both demand that all waist-tied chrysalids should be thrown together in one category. Now, the hesperid cocoon may as well have preceded the lycaenid waist-tied chrysalis, and here there are other and good reasons for believing that this may have been the actual sequence. For the character which induced Boisduval to class the Papilionidae and Lycaenidae together as Succincti, is, when we compare it with other characters, seen to be secondary only, one of convergence in habit, one not in itself by any means synchronous, one, finally, evidently reached upon entirely differing phylogenetic lines. The structural gap between *Papilio* and *Lycaena*, is one of the widest among the day butterflies. The utmost we can grant to Mr Scudder is, then, this: that the mode of attachment in *Hesperia* may represent a stage by which the cocoon-making larva prepared itself to abandon the habit. To make more of the observation than this, is to trifle, to exaggerate. And, in itself, the view that the waist-tied chrysalis is a further and necessary stage between the cocoon and the suspended chrysalis seems erroneous, inasmuch as the former relapses sometimes into the cocoon, as in *Parnassius*. Elsewhere, in the Lepidoptera, the cocoon is apparently abandoned without giving birth either to the waist-tied or the suspended form. There exists, then, no such necessary series of changes as Mr Scudder's system should be able to fall back upon to give it stability.

Now, in *Zerynthia* or *Thais*, the girdle of the waist-tied chrysalis has slipped up to the head of the chrysalis, to the 'nosehorn,' as stated by Dr Chapman; so that if it went up any further, the pupa must fall over and, being then only fastened by the tail, would hang down and turn, so to speak, from a Succinctid into a Suspensid. But the girdle, in this case, although slipped over the head, would still be there, while useless as a support, and the general question would still remain of how the girdle had been gotten rid of to enable the chrysalis to pass from the type of the Succincti to that of the Suspensi. Just as Mr Scudder finds in *Hesperia*, making a 'shroud' within its cocoon, the beginning of the Succincti, so in *Thais*, with its girdle slipped up to the 'nosehorn,' we may find prefigured the beginning of the Suspensi. In each case we are furnished with a hint and no more of the possible *modus operandi* by which the cocoon and the girdle have become discarded. And if in the next more specialised butterfly than *Thais* we found a Suspensid, we might have Mr Scudder's ideal sequence actually realised. As a matter of fact *Parnassius*, which *is* this butterfly, walks back into the cocoon, and what becomes of the slipping girdle of *Thais* we, that is, I, cannot say. Perhaps these or similar considerations might have occurred to Mr Scudder; but, when one has so beautiful a theory in hand as is supplied by the 'shrouds' of the Involuti, one may be forgiven for not looking deeper into the matter. Yet it is one deserving of a little thought.

Long ago I called attention to the separate nature of the conditions of life attending the larva, pupa and imago, and drew the corollary that these different stages must be studied by themselves, and judged by their own standards. The caterpillar and moth of *Apatela* go their separate ways, having different cares to meet, and what in each is a relatively primary character must be distinguished from what is a relatively secondary. In one sense the larva dies when it changes to a chrysalis, and the chrysalis perishes when it discloses the butterfly. Both throw off their former mode of life and its dangers with the cast skin.[1]

It is probably, then, a fallacy to believe, with Mr Scudder, that there is 'a regular progression from the cocoon of the moths to the almost total absence of the use of silk,' and that the facts imply such a progression. Instances are not rare where the generalised forms spin little or no silk, and the specialised forms, on the same line, make large and complex cocoons. This envelope to the pupa is so clearly an adaptive character that in one, single, upon other characters quite homogeneous group, like the 'Emperor Moths,' it runs through the entire scale, from utter absence to a specialisation seldom attained throughout the Lepidoptera, the hanging cocoons of *Callosamia* and *Attacus*. Mr Scudder himself speaks of cases of exception to his theory, 'where the necessities appear overwhelmingly great.' So that a 'biological necessity' exists, at least sometimes. But the necessities are always great, being ultimately matters of life or death. In the 'Emperor Moths' it is the more generalised forms that spin no or little silk, and the more specialised that secrete

[1] In my original communication, *Mittheilungen aus d. Roemer Museum*, No. 8, p. 15, I have shown that the pupal specialisations given by Dr Chapman for the Pierinae do not run parallel with those offered by the neuration of the imago.

much silk and make complicated cocoons. Mr Scudder regards as a logical sequence the class of facts offered by *Hesperia*, making a 'shroud' within the cocoon, by *Pieris*, dispensing with the cocoon, by *Nymphalis*, hanging only by the tail and losing the 'shroud,' and finally by *Oeneis*, losing all attachment and transforming almost nakedly among the surroundings of its bleak home. But only on paper does this series figure as a scheme of progression. The isolated occurrences are brought artificially into connection by Mr Scudder as a basis for his system. The separate and recurring features of adaptation are not phylogenetically related. Nor do the specialisations of the butterfly keep pace with this imaginary series. *Pieris* has a more specialised neurational pattern than *Nymphalis*, and *Nymphalis* than *Oeneis*. As to the spinning, what evidence we have rather points to a reversing of Mr Scudder's sequence, since the more complicated the pupal envelope, the more specialised the butterfly appears often to be. The Succincti, as a phylogenetic group, has no reality behind it, because it is composed of butterflies specialised upon different primary patterns of wing structure. So that the elements out of which it is made up are in reality foreign to each other, and are only brought together upon a character which has been evidently separately evolved, the common origin of which is contradicted by the disparate structure of the 'Blues,' 'Whites,' and 'Swallowtails.'

Let us observe the way these schemes arise and are perpetuated. No sooner had Boisduval noted the differences in the manner of attaching the chrysalis, than he made a system out of the several modes, and invented Latin names for the resulting categories. M. le Dr Boisduval (alas! that I write in the past tense) was *bonhomme*, well satisfied with himself and the world, and, after launching his idea in its scientific dress, troubled himself not at all with its correspondence, or want of correspondence, with the entire facts it was designed to cover. It was as if the attractive and careless gaiety of French genius had presided over the fabrication of a system so manifestly *décolleté*. The net of Boisduval's classification is, however, sufficient to hold Mr Scudder, although the butterflies themselves may escape through its meshes. And Mr Scudder, surveying the Lepidoptera through its medium, supplies us plentifully with reasons why such a net should be the one thing needful to interpose between the butterflies and our understanding of them. It is true that Boisduval's system possesses both effectiveness and a certain ease which charms the mind, but when it is pieced out by argumentation, when it is used as a basis for further scheming, we come to see how insufficient it really is. Boisduval himself belonged to the old school and preferred *Papilio*. It was therefore rather an unkind use of this author's pupal system to forge thereout weapons against the ancient order. But I believe they will turn in Mr Scudder's hands.

To the French school, to which we owe so much which has made Entomology attractive, we are moreover indebted for that divorcement of the butterflies and moths which is pronounced by the terms Rhopalocera and Heterocera. Convenient as these terms may be to designate what is a rough and surface grouping, we must always be led to reject them as indicating, by means of categorical names, a structural difference greater than really exists. In coining these titles M. Boisduval stepped into the slippers of M. Dúmeril, and has since found imitators who have invented for us the additional names of Nitrocera or Grypocera. All this brilliant play at making systems, without weighing all the results, fascinates many, and among these also Mr Scudder, who remains among the Suspensi and Succincti, weaving in an additional original observation on the 'shrouds' of the Involuti, and believing for ever that upon these threads hangs the history of the butterflies. It is almost a pity that we are compelled to remember that systems and categories are but part of the machinery of science, and that when they over- or under-do their appointed work (which is here to designate the affinities of organisms) they should in reason be changed or even swept away.

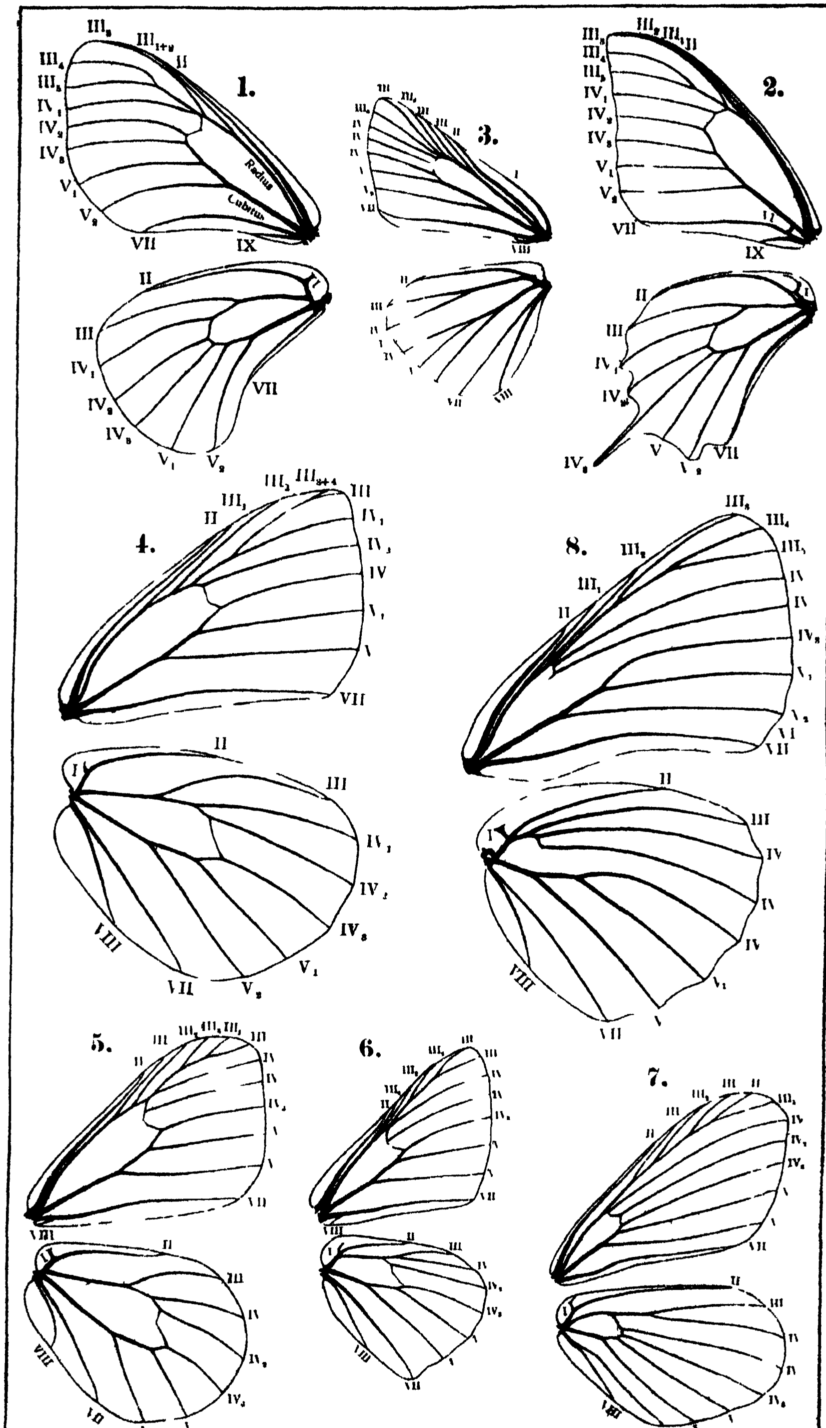
At what time we may imagine the primeval and colourless lepidopterous caterpillar to have left its hidden home in the stems and leaves of plants and emerge into the open, here, under fresh conditions, taking up the struggle for existence, and dodging the effects of its difficulties by adapting itself to them, thus finding in the conflict the way for the endless variation we find in the coloured larvae of to-day —at that time, also, did the primeval caterpillar bring with it, of necessity, the habit of spinning silk to provide for the safe repose of the chrysalis. This protection would naturally vary with the surroundings. Impeded by circumstance, subject to be thwarted and turned aside by different objects and new conditions blocking the fresh roads of travel, the habit of spinning would express itself unequally, the old fashion displayed would become modified, at times the original custom might even altogether fall away. And this is what we see—cocoons of all sizes and consistencies, to the Noctuid fastening together grains of sand with silky saliva. Even now the individual spinning larva, forcibly prevented from completing its web, or from attaching itself, will transform without protection, and trust to fate to live and become a butterfly. So that the mode of spinning is seen to be in itself secondary and auxiliary merely, and we can understand how identical or similar fashions might appear upon independent phylogenetic lines.[1] Now, however the fashion of

[1] Among the Bombycides, the Lachneidae spin a cocoon, the nearly related Crateronygidae pupate nakedly in the ground. My friend, Prof. Dr Pabst, writes from Chemnitz of a certain larva of the cocoon-making *Euthrix potatoria*, that it transformed nakedly upon the surface, without spinning a thread, yielding in due time a normal imago.

spinning might tend to crystallise and be sedulously followed, it would no less be liable to give way on occasion to change, to adapt itself to new circumstance, even to break more or less suddenly with custom, and this quite independent of the modifications of the butterfly. Undoubtedly, the mode of spinning has its story to tell, and we should be at the pains of reading it rightly, and use the information as a help to a phylogenetic classification; but, from its very nature, the character cannot be drawn out to the extravagant extent attempted by Mr Scudder. What, for example, are we to think of the Geometrid or the Plume which suspends its chrysalis after the fashion of a day butterfly? Mr Scudder can only answer: 'It is not a butterfly,' and dismiss the case. But we think that it is rather time to discard the use of Boisduval's terms of Suspensi and Succincti and Involuti as designating natural groups, and cease to rely upon the characters drawn from these assemblages as affording us a key to the phylogenetic classification of the butterflies.

Beyond this, we can show that Mr Scudder's estimate of *Oeneis* takes no account of the structure of this butterfly's wings. From these organs it appears clear that *Oeneis* is not a specialised form, one in any way remarkable above its fellows. The most specialised Satyrids I have yet examined in the European fauna appear to be *Pararge aegeria* and *Lasiommata megaera*. In these vein IV_3 of the hind wing has effected its complete junction with the cubitus, reproducing a character normal with the Nymphalidae proper. Now *Oeneis* has not this character at all; here, as in all Agapetinae, to which sub-family group it belongs, vein IV_3 springs still from the cross-vein. *Oeneis* is plainly allied to *Erebia*, even a little more generalised, as shown by its completely confined median cells, the space between the veins more equidistant, vein IV_3 a little further from V_1, the fusion of II and III hardly so extended. Among the members of its family we have in *Oeneis* by no means a 'high,' rather a comparatively 'low' butterfly; so that it seems absurd to place such a butterfly 'at the head' of the whole system, because it proceeds from a nearly silkless chrysalis. And we may conclude, with Hume, that when any opinion leads to absurdity it is certainly false.

In Mr Scudder's paper on the "Classification of Butterflies, with special reference to the position of the Equites or Swallowtails," Philadelphia, June 1877, we find a variety of reasons assigned to justify the deposition of *Papilio*. Those upon which Mr Scudder seems to lay most stress, we notice here: "But perhaps the most striking point of affinity between these two groups (*i.e.*, 'Swallowtails' and 'Skippers') lies in the possession, on the front tibiae, of the characteristic foliate epiphysis which is wanting in all other butterflies" (*l.c.* 77). Now, did this character occur only in these two groups, of all Lepidoptera, it might be conceded to be a mark of affinity, but since this is not the case it may either be continuous in the two groups from a remote ancestry, before either *Papilio* or *Hesperia* were thrown off, or it has been independently acquired. Since it is not a relatively primary character it affords no conclusive argument for the sequence adopted by Mr Scudder. Again: "The inner border of the hind wing is folded longitudinally just as it always is in the 'Skippers' and rarely in other butterflies." This is a secondary character and the same sort of argument would bring *Parnassius* and *Rothschildia* together, for both have the inner margin of the hind wings hollowed out, and here vein VIII has disappeared in both instances. In the Lepidoptera, quite generally, the inner border of the hind wing becomes variously specialised. "But again," says Mr Scudder, "the Swallowtails are universally conceded to be so closely allied to the Pierids, that they are invariably placed next them; consequently, if the Swallowtails are placed highest in the scale, the Pierids must go with them; nobody questions this; yet the Pierids possess not a single one of the characteristics by which a high rank is claimed for the Swallowtails. Commentary upon this is needless." Now, I believe myself that commentary upon this criticism of Mr Scudder's is 'needless.' The whole remark is based upon the existence of affinities between the 'Swallowtails' and the Pierids, which I decidedly question, and loses its point. The resemblances with the Pierids lie in the parallel secondary movements of the movable veins. The phylogeny of the two groups is most clearly distinct. *Papilio* is not so much a 'low' or 'high,' as a peculiar butterfly. The conclusion Mr Scudder comes to with regard to the *osmateria* of *Papilio* is, that because a similar character is developed in *Cerura*, which is a moth, it must indicate low rank in the butterfly. But this is a character of secondary larval adaptation, in which *Cerura* far outstrips *Papilio*, and by the same token shows the inequality of specialisation, and that this character in the larva, being secondary and adaptive, cannot directly affect the imago. It may seem extraordinary to Mr Scudder that a moth should be in any point 'ahead' of a butterfly, but such nevertheless is the fact. Mr Scudder's view of the case arises from the fundamental error that there is a linear succession in 'rank,' which, of course, precludes the idea that in any character a moth could outstrip a butterfly. Again, when we admit, with Mr Scudder, that the straight ventral surface of the abdomen in the pupa of the Suspensi is retained over from the Succincti, it does not necessarily indicate that the Suspensi outrank the Succincti, but it makes it probable that the brush-footed butterflies had their origin in the main stem of the six-footed butterflies after the waist-tied habit had been acquired. The Pierid Succincti have gone their own way after



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thus probably, at some point in their history, having given off the Nymphalid Suspensi, and to-day their wings show specialisation in both of two directions, while the brush-footed butterflies only show it in one. The plain palpable fact, that the wings of *Papilio* are laden with characters contradicting those of *Hesperia*, is ignored by Mr Scudder, who pursues after characters, either fancied or of less or misunderstood value, to assist his theory, which seems to him to demand that the two groups be brought together. Alone the position of the radial branches, notably that of III_4 and III_5, make it extremely improbable that *Hesperia* and *Papilio* should be at all nearly related. Reasons may be far-fetched and resemblances deceptive, but, when we come upon characters which forbid, we may be sure our schemes ought to be dropped. We cannot, with Mr Reuter, bridge a difficulty with a Latin name.

There exist likewise a number of published arrangements, beside Mr Scudder's, in which the 'Blues' are brought in next to the 'Whites,' and, as is oftenest the case, without any assigned reason. Not unlikely is this course, in some instances at least, due to a recognition of the fact that in both groups a diminution in the number of radial branches occurs. But this reduction takes place also in the Parnassiinae and in the Saturniades as well, and is clearly a recurrent, secondary movement, following a general direction in evolution. As such, in the absence of other grounds, it constitutes no sufficient basis for bringing the 'Whites' and 'Blues' together, and cannot be properly used for such a purpose in classification. But to review all the proposed arrangements of the day butterflies would give us a volume as bulky and difficult to manage as that recently issued by Mr Reuter. It will be sufficient if we have made it clear that the nature of the class of characters, used by recent systematists to establish their sequences, has frequently not been well understood.

Before giving a summary of the results at which I have arrived from the study of the neuration, it may be well to give the method employed. This consisted in preparing enlarged photographs of all the wing patterns to be studied. By placing these impressions side by side upon shelves where they could be shifted, comparison could be readily effected, and, in this way, the difficulty of examining each specimen by itself under a microscope and carrying the picture of what was to be noted in the mind, was obviated. Sketches and drawings are here of little use, while almost all the published drawings I have been able to compare are not sufficiently accurate to be relied upon for study.

And before enumerating the detailed observations upon which my conclusions are based, I may epitomise these latter as follows: The system of Bates, which places the brush-footed butterflies 'at the head,' in a linear arrangement, is not especially warranted by the neuration, since here the radius remains in a five-branched generalised state, none of the branches having submitted to extinction by absorption. The Parnassians and 'Swallowtails' possess exclusive neurational features which tie them together, and authorise their being regarded as a morphological group, equal in value to all the other butterflies. Hence they cannot be properly interpolated at any point among the latter. Nor can they be placed after the Hesperians, because the looped vein VIII on the primaries of the 'Skippers' is repeated in the Sphingides and other assemblages of the moths. Furthermore, the Parnassians constitute in the clearest way a specialisation of the Papilionidae, of which the steps can be followed, the Zerynthianae representing an intermediate stage between the Papilionidae and the Parnassiinae. Any sequence, either in letters or of an objective series of specimens, should commence with the Parnassi-Papilionidae. The 'Blues' are nearest related to the 'Skippers,' of which they form, in an analogous manner, a specialisation, although here the gap in other features outside of the wings is wider. The 'Whites' and the brush-footed butterflies are equally connected by a common pattern of neuration, however they may separate upon the structure of the feet.[1] I thus return, in the main, to the sequences of Linné in 1758 and of Fabricius in 1787.[2]

The drift of my discoveries and conclusions lies in the direction of the rehabilitation of *Papilio* and a correction of the estimate placed by Bates and his followers of the value of structural features in the diurnals. Mr Wallace and Mr W. H. Edwards, though their special arguments may be inconclusive, will have been justified after a long dispute. It might, indeed, have been thought sufficient to present *Papilio,* resplendent in colour, imposing in size, perfect in development, labelled in addition with a knightly name, to secure recognition. Anthropomorphic as are the actual terms of the presentation,[3] this is in so far valid, as the Parnassi-Papilionidae are structurally separable from all the other butterflies. But in this papilionid group it is not *Papilio* itself which is the most specialised, but its scion, *Parnassius apollo—cedant arma togatae !*

If we follow Mr Scudder, we must commence our sequence of

[1] Mr Bates' plea for *Heliconius*, in itself an otherwise generalised butterfly, but one in which the forelegs are specialised to an extreme, is a logical one under Mr Scudder's statement that "we must accept atrophy of these legs (!) as a conclusive mark of high organisation" (*l.c.* 79). Some moths (perhaps a fact not known to Mr Scudder, *e.g.*, *Pallachira bivittata*, Grote) have, however, also the front feet aborted. But since Mr Scudder passes *Heliconius* by, and prefers *Oeneis*, we are spared here the discussion.

[2] "Mantissa insectorum," Hafniae, Tom. ii. pp. 1-92.

[3] "The Malayan Papilionidae," etc., pp. 133-140, Am. Ed., 1871: "Owing to the complete and even development of every part of their organisation, these insects best represent the highest perfection to which the butterfly type has attained," etc.

the day butterflies with *Oeneis* and the 'Meadow Browns'; if Mr Bates, with *Heliconius*, classified incorrectly by Mr Reuter with the 'Wood Nymphs'; if Dr Skinner and the Philadelphia Check List, with *Danaus* and the Limnads. But, if anyone will follow me, then with *Parnassius* and the 'Mountain Butterflies, 'bringing in their train *Papilio* and the 'Swallowtails,' and restoring these to their own again.

Leaving all subjective ideas severely alone, we may have succeeded in showing that the reasons brought forward by Mr Scudder to sustain the pre-eminence of the brush-footed butterflies are quite hollow. It appears to me that the day butterflies are, considered as a whole, relatively specialised Lepidoptera, and that any sequence of the order should commence with or culminate in them. Whether we are now able to settle the difficult question of development in this order of insects, or whether we are not, it seems to be equally proper to commence our arrangements with the diurnals, and this from practical considerations, having in view all the exigencies of the case. This being so, and appearing to be the common-sense view of the matter, I need not enter here into the difficult question of the comparative specialisations and generalisations of the moths. I wish merely to record my opinion that the book, catalogue or collection will best subserve the interests of science, and will have the most success, which, reckoning with practical and not theoretical and disputable considerations, makes its beginning with day butterflies, whatever special sequence of their groups be chosen. But no shadow of a doubt remains upon my mind—we should commence the series of the day butterflies with the Parnassi-Papilionidae.

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(*To be continued next month.*)

EXPLANATION OF PLATE I.

The figures are obtained by photographic process.

III = Radial veins; IV = Median veins; V = Cubital veins.

FIG. 1. *Parnassius apollo*. Type of genus and family. Size 1/1. The specialisation, as compared with *Papilio*, is to be traced in that IV_1 of the median series has left cross-vein, and that the position of IV_2 is cubital on both wings. In the second direction that the radial branches have become reduced in number.

FIG. 2. *Papilio machaon*. Type of genus and family. Size 1/1. The character binding the group Parnassi-Papilionidae together is seen in the retained vein IX on internal margin of forewings, the hollowed out internal margin with vein VIII lost, and vein VII shortened, especially in *Parnassius*. The generalisation of *Papilio* is shown in the retained intercalary vein marked VI, the central position of IV_2 on primaries, the five-branched radius. On hindwings the separation of vein I must be noted, showing that this so-called 'praecostal spur' is really a longitudinal vein, which becomes basally absorbed by II in *Parnassius* and most other butterflies. It is partially distinct in *Limnas* and *Zerynthia*.

good example he will find hundreds of indifferent ones. The rough, badly-struck flakes used as scrapers and knives, as their edges testify, the rude axes and other tools, both Neolithic and Palaeolithic, are familiar objects to him; for the ancient savage, except on rare occasions, was in the habit of making his tools and weapons with the least possible labour. Hence these trimmed flints from the Plateau naturally appeal to him; he has seen similar work in more modern specimens, and of its human origin he has no doubt.

It is remarkable that, until the appearance of Mr Cunnington's paper in the November number of *Natural Science*, no serious attack had been made on Plateau man. Adverse criticism, it is true, had been evoked on the flints at the Geological Society and at the Anthropological Institute. They had also been dismissed with "Of no interest whatever." Consequently it is a matter of congratulation that we are at last in a position to examine carefully the various objections and criticisms which, though often urged in private, have never appeared in print till now. Since, however, the Pre-glacial age as well as the human origin of the flakings has been attacked, it is necessary to restate the case for Plateau Man.

Scattered over the North Downs, but always at a great elevation, comparatively speaking, are patches of flint gravel, in which one also occasionally finds Pliocene ironstone and the well-known Oldbury stone. The flints are stained a deep ochreous brown colour, and some of them are chipped on the edges, whilst very rarely a fairly well-shaped implement is found. That these flints were at one time in an iron pan there can be little doubt, as portions of a ferruginous deposit are often found incrusting both the chipped and the unchipped portions.

Objection was first made that, as all the finds were on the surface, it was impossible to say anything about their age. By means of a grant from the British Association, pits were sunk at the Parsonage Farm, Ash, whilst last year further excavations were made by Mr Harrison. In all these pits, at a depth of 8 feet from the surface, a bed of gravel, varying from 6 to 12 inches in thickness, was found. The gravel was cemented by iron, and was so hard that a pick was needed to break it up. The section from the first pit, which is typical of them all, was as follows:—Humus, 1 foot; stony loam, 2½ feet; grey loam, with a few scattered pebbles and small worked stones, 3½ feet; ochreous loam, 1 foot; gravel layer, with many worked flints, 6 to 12 inches. The underlying sandy loam, which was pierced for a further depth of 19 feet, is probably of early Tertiary age.

Since the publication of Mr Harrison's finds on the North Downs the chipped flints have been found by other workers over a large extent of country. They have been discovered at Friston, near Eastbourne, by Mr Hilton; at Stede Hill, near Lenham, by Mr Crawshay; at Blean Hill, Canterbury, Amersham, and Micheldever, Hants, by Rev. R. Ashington Bullen; near Salisbury, by Dr Blackmore; in South Essex, by Dr Frank Corner; and at West Wickham, Kent, by myself. With the exception of the Essex finds, the gravels in which they occur bear no relation to the existing river systems, whilst Dr Corner's specimens, though obtained from Thames gravels, are derived from older beds as their rolled and worn condition shows.

It cannot, however, be too strongly urged that photographs, however excellent as such, convey absolutely no idea of the appearance of these flints; and though the figures given in Sir Joseph Prestwich's papers, and in his last published work "On Some Controverted Questions of Geology," as also those given by Mr Abbott in *Natural Science* [4], are very good indeed, yet to obtain a true estimate of their character a careful examination of the actual specimens must be made.

In looking over a large series it will at once be noticed that the flakings differ very much in colour. On many of the flints from Branshatch the chipped edge is whitish or pale yellow; yet even on these specimens one may often find portions of the ferruginous deposit on the worked surface, so that probably the colour has been shown. Many of the examples, too, will be seen to be polished, and this Mr Cunnington attributes to the action of blown sand. But it is identical with that found on the flints from the Pliocene gravel of Dewlish, and considered by the Rev. O. Fisher to be probably "due to the long-continued percolation of water carrying fine silt with it"; and this view is, I think, the correct one. As to the scratches which occur but rarely, and which Mr Cunnington considers glacial, I think far too much stress has been laid on them. I cannot believe that they have been "caused by the movement of pointed flints across the flat surfaces of other flints under considerable pressure." The only effect of such conditions would be to smash all the points, the result which has occurred in all my attempts to scratch one flint with another. The scratches are perfectly clean and have certainly been caused by a mineral much harder than flint, and in all probability have been graved by the small quartz grains which occur in the iron pan. I should, however, like to point out that one implement, No. 534 of Mr Harrison's collection, is scratched on both sides, and this example has been accepted as of human manufacture by Sir John Evans. It is stained a deep ochreous colour, was found at West Yoke, and is now deposited in the Maidstone Museum, where I hope it will find a permanent home.

Mr Cunnington takes exception to the statement that, as a rule, the chips were struck off from one side of the flint only; but a careful examination of my own collection, numbering nearly a thousand examples, fully bears out this view. This indeed is perfectly obvious to anyone who has tried to chip flints. A very large number of the Plateau flints are similar to No. 1, *Natural Science*, Vol. XI., Plate IX., namely, with one surface flat, and the other convex and rough. In order to chip an example like this the blow must be administered to the flat side, and we find that such is the case. At least 90 per cent. of the flakes on these flints have been struck off from the flat side, and in the remaining 10 per cent. it will nearly always be found that owing to the shape of the flint it is easier to chip from the convex side. In fact they are chipped in exactly the same manner as a reasoning being would chip them to-day, If they had been flaked by natural causes we ought to expect that the flakes would be forced off from either side in the same proportions, and this, as I have just stated, is not the case. As Sir Joseph Prestwich has remarked, "the trimming, slight though it may be, is to be recognised by its being at angles or in places incompatible with river drift agencies, and such as could not have been produced by natural causes which tend to remove all projecting points."

Mr Cunnington has entered very minutely into the history of four of the Plateau flints, and lays particular stress on the fact that No. 1, Plate IX., exhibits surfaces of four different ages. But as two of these would be admitted by everyone as non-human, there only remain two sets of flakings. He further states that, as "the chippings were not all formed at one period, this fact is quite inconsistent with the theory that they were artificially shaped by man." Perhaps Mr Cunnington only wishes this remark to apply to these old flints, because it notoriously does not apply to Neolithic and Palaeolithic forms. It is very common to find Neolithic flakes which have been twice used, the original surface being white, and the newer work black. I have seen many Neolithic scrapers, especially from the Wealden area where flint is scarce, which have been made from Palaeolithic implements. Work of two and even three distinct ages is by no means uncommon among the River-drift examples. It certainly seems to me more reasonable to suppose that a flint was twice used by man than to imagine that it has been subjected at two different periods to the action of 'natural' causes operating in a manner which would be absolutely unique, and which cannot be paralleled anywhere on the globe at the present time.

Another objection is to the supposed uselessness of the shapes. But because we cannot in all instances understand their uses, it by no means follows that they were without use. An extremely common form in the River-drift is the well-known tongue-shaped implement, but for what purpose it was employed is still a matter of dispute, because at the present time no similar form is known to be in use by any savage people. With Neolithic specimens, what one authority calls a sacrificial knife (which by the way has generally a blunt edge), others tell us is a skin scraper. The fact is that little or no attention has been paid to savage wants and customs, and I am afraid that owing to the spread of civilisation it is now too late in most cases to obtain reliable information.

Mr Cunnington's statement that "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," is totally at variance with that of Sir Joseph Prestwich. "It is only in a very few instances that an implement has been entirely wrought out of a larger flint." As a matter of fact they do occur, but only rarely.

Another objection is the supposed abundance of these worked flints, and we are further told that "on the chalk plateau the stained flints are all more or less chipped." That there are large numbers of them in various collections is indeed true, but the vast majority have been obtained from the labourers, nearly all of whom are very keen hunters indeed. I have tramped many miles in search of them, and I should consider fifty examples found after six hours' search, as a very good day's work. There are patches of the gravel where one cannot find a single worked flint. If they occasionally occur in abundance at one spot, do not palaeoliths also occur in the same manner? I have found more Palaeolithic implements and flakes in one field as the result of four hours' search than I could carry away. I have never been so fortunate with Plateau specimens. Mr Cunnington states that "if the flints were worked, used, and then thrown down again, we should expect to find them widely scattered." Speaking for myself, I expect nothing of the kind, for it is well known to all geologists that the surface of the North Downs has suffered greatly by denudation in modern times, consequently the flints are not likely to be found on the wasted portions, but we do find them scattered over the surface of the unwasted portions. As to whether all the flints obtained from the pits show traces of human work, I am not in a position to state; but I must admit that I would reject a certain number, which, however, Mr Harrison accepts. The large bruised and battered specimens are to my mind the result of river action, but there are a large number which I unhesitatingly accept.

Then, Mr Cunnington urges that the hollows in the flints are chipped in the same manner as the edges, and points to this as a proof of the natural origin of the flakings. I have carefully gone through my own collection, and through a large part of Mr

Harrison's, and in no instance can I find a hollow the edges of which have been chipped. In some instances they were battered, but never chipped. We are further told that "the earliest chippings 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." This appears to me to be the opposite of what one ought to expect. The first chips would be small; they would increase in size as the temperature was lowered, until the maximum cold was reached, and they would decrease in size as the cold diminished.

Mr Cunnington, however, sees that it is of no use invoking the aid of river action to account for the flakings, so he postulates a frozen gravel, for which seemingly he invokes the remarkable property of softening flints. For we read that "pebbles in the gravel would be pressed against the upper edges of the flint slabs, and force off small flakes"; and then we are informed that "the chipping was due to pressure by some yielding material." The means, however, by which flint pebbles were converted into yielding material are not stated. Nor are we enlightened as to how a "yielding material" could exert sufficient pressure to chip so hard a substance as flint. Especially when it has to be borne in mind that the amount of force brought to bear in the form of simple pressure in order to effect such an object must be vastly greater than that which would be required in the form of a blow or tap. The action of this frozen gravel, moreover, we are asked to believe, was even yet more remarkable for having chipped the flints on one side; it turned them over and chipped them on the other, whilst it removed flakes from the thicker parts, and left the thin edges untouched. It made bulbs of percussion, and then by another pressure produced an *éraillure*. It chipped dozens of flints into exactly the same form; it carefully avoided breaking the pointed portions of these flints, however delicate they might be; and it even produced forms which experts have pronounced to be genuine implements. Lastly, this yielding material completely disappeared, leaving its work as the sole evidence of its existence, and no man has ever seen its like on this earth since!

There are thus two views between which to choose. The one held by the larger number of observers that these flints have been chipped by a reasoning being for a definite purpose, is proved by the fact that they admit of being grouped according to certain patterns. On the other hand there is the view advocated by Mr Cunnington, that they are the chance products of natural forces acting in a manner which cannot be matched at the present day. I for one certainly hold that the former is the more reasonable view. No one would think of challenging the statement of Sir John Evans "that the rolling and wearing of the edges were probably caused by natural agencies." It is the occurrence and manner of the flaking that is relied on as proof of the human origin of these implements. The old and oft repeated challenge by Sir Joseph Prestwich to the doubters to produce natural flints having the form of the Plateau specimens has not yet been responded to, and I venture to believe it never will be.

As to the age of the gravels in which these implements occur the bulk of the evidence is of such a technical character that it would be quite out of place in this paper, but should anyone wish to examine it in detail I would refer him to the papers by Sir Joseph Prestwich. His view that these gravels are of Pre-glacial age has received the support of practically all geologists who are well acquainted with the area. Mr Cunnington holds them to be frozen gravels and therefore glacial, and this view has the support of Mr Clement Reid.[1] Mr Reid arrived at this conclusion after a brief visit to the district, and he would further correlate them with the implement-bearing gravels of Sussex and the Thames Valley. The only sections of the gravel that we know of are from the pits at Parsonage Farm, Ash; and here it was six to twelve inches in thickness cemented by a ferruginous deposit. Anything more unlike a frozen gravel than this I do not know. An undoubted frozen gravel deposit does occur in the area of the North Downs, as was pointed out years ago by Charles Darwin. It is to be found filling up the bottoms of the chalk valleys, and sometimes attains a thickness of thirty feet. It consists of unrolled and subangular flints, Tertiary pebbles, and other material, and is without doubt the sweepings of the adjoining higher land. Bones of the musk ox, mammoth, and horse have been found in it, as well as land shells. Derived Plateau flints also occur, thus clearly showing that they are anterior in age to this deposit. They also occur as derivatives in both the Thames Valley and the Limpsfield gravels. From its situation the latter has always been held to be of greater antiquity than the former and this view is supported by the character of the contained flint implements. In fact the whole of the available evidence strongly supports the Pre-glacial age of these beds, and none is forthcoming to uphold the opposite view. Mr Reid suggests that the rude character of the implements arises from the fact that owing to toughening by weathering these flints could only be battered and not flaked, but this ingenious suggestion has been easily disproved by Mr Harrison, who has made from the Plateau flints very good copies of the River-drift types, one of which is before me as I write; and he informs me that there is no difficulty in so doing.

[1] Annual Report of the Geological Survey of the United Kingdom, 1897, p. 78.

and plants, and the landscape in the process of being actually moulded, as being in every respect a part of the geological record just as much as the Carboniferous beds that underlie the chair on which I am sitting at this moment. Things as they are at this moment represent the very last chapter in the history of the earth, and we can no more separate this chapter from the rest than we can tear a man from his shadow. It is with to-day that we must begin if we are to make any real progress in geological reasoning, and we must reverse the teaching of the orthodox museum-naturalist who raises a great fence between the dead past and the living present.

Again, another heresy, or rather a reversion from the orthodoxy of to-day to the orthodoxy of fifty or sixty years ago. Reasoning, we have been continually taught, is either deductive or inductive. To some of us deductive reasoning, which is the especial glory of the great German mind that fills our firmament just now with its appalling clouds, is a barren mother, or, if not barren, is a mother that produces monstrous offspring too often to give us any confidence in her results. To frame a splendid postulate, far-reaching, uncompromising, audacious, and then laboriously to make God's handiwork in Nature witness to the superior genius of Man, or, in other words, to bend and twist our facts in order to reconcile them with the *obiter dicta* of some Pope of Science, this is the epidemic from which we daily suffer more and more, and against which the natural instincts of a young and aggressive natural scientist as naturally revolt. We hate all kinds of *à priori* reasoning in a science like ours. It is not English, it is not rational; it is German, and it is metaphysical; and metaphysics, as some of us have found who have spent years in quest of its secrets, is like drinking Bass's beer out of an empty mug, or making love to the Venus of Milo. We abominate every kind of general postulate springing from the innate prejudices of superior men. We include in this the great fetish of the modern geologist, the doctrine of Uniformity.

The word 'uniformity' has done infinite harm in Geology from the confusion involved in its double meaning. In one sense every man of science is a 'uniformitarian.' Science is very largely the discovery or verification of law in the Universe, and the very first and most elementary of its axioms as confirmed by universal experience is the Uniformity of Nature's Laws. We none of us doubt that with the same causes the same effects will follow. We none of us doubt that the same quantity and quality of force acting upon the same materials will produce the same quantity and quality of result. In geology, however, the term 'uniformity' has acquired an entirely different connotation, which is as illogical and mischievous as it is scouted by the students of other sciences. Lyell, following the guidance of Hutton and of Playfair, converged a great deal of thought upon the various current methods in which the earth's surface is being disintegrated and renewed, and having calculated the cumulative effects of very small causes extending over very long periods of time, he was more or less fascinated by the result, and argued sometimes, as if he thought that the whole architecture of the universe had been fashioned in this gentle way. The diurnal effect of raindrops, of the grinding of the tide, of the results of frost and heat, of wind and water, when supplemented by unlimited drafts upon time, were deemed competent to shape the gaping hollows and the mantled crags which diversify the earth's surface. This was hinted at by Lyell in many places, although his keen eye and long experience made him qualify the conclusion by many exceptions. His scholars have had no such scruples. They have boldly and aggressively and continuously pressed the conclusion to its logical end. But they have gone further. They have iterated and reiterated the anti-Baconian aphorism that the forces of nature have always been in quality and quantity what they are now. The dictum was enunciated in all its naked absurdity by Ramsay when he presided over the geological section of the British Association, and this view has been since the inspiring philosophical creed of that most tyrannical of scientific tribunals, a Government Department—namely, the Geological Survey and its officers. Committed to preach a certain definite creed that has become the corporate geological faith of the Department, it has dominated shelves full of reports and manuals, which, from their official sanction, have almost the character of inspiration with most students. More especially has this been the case in America, where, as in this country, this bastard form of uniformity dominates official geology. Against it many of the older writers of the first rank protested vigorously, and against it I have preached myself (being only one of their scholars) all my life, and signs are multiplying that a revolution in men's opinions on the subject is not far off.

When we look at the face of a man pitted with small-pox, we refuse to believe that those hollows and scars are the normal results of human daily life instead of being the effects of a new disease which came upon mankind as a tremendous catastrophe in the last century. When we look at the moon through a telescope and examine its pitted face where the large rents and craters exceed all human experience, and when we, on the other hand, realise what peace and stillness and terrible quietude mark the face of that great cinder at this moment, we laugh at your uniformity as taught by modern geologists. It is as monstrous to suppose that any forces which have operated on the moon since human observation was turned upon it could have moulded that surface, as it is to suppose that the north wind hatches geese out of barnacles, which was a famous deductive conclusion of the science of the dark ages. We shoot a pellet of lead at a female pheasant. It enters and injures its ovary, and the result is that that female at the next moult virtually adopts the plumage of the male, and every cell secreting colouring matter and pattern in its skin, suddenly and with a tremendous impulse and jump, proceeds at the next moult to secrete an entirely different colour and pattern. We look through our telescopes at a star which, so far as we know, has been twinkling through the ages with one unbroken light, and it suddenly bursts out into flame, a glorified conflagration in which the fire must extend for tens of thousands of miles, a gigantic catastrophe unmeasurable by any index we can get hold of here, and you tell me that the course of nature is as monotonous as the conversation of a parrot or of the never-ending imbecilities that make up the small talk of society. The thing is monstrously ridiculous. A comet goes flying through space for hundreds of years along a very oblately defined orbit, and it maintains its form as a more or less globular mass of nebulous matter. Presently it approaches the sun, and for a short time only projects into space a tail hundreds of thousands of miles in length, with a velocity and under conditions which are stupendously marvellous. Is this not an excellent object-lesson in catastrophes?

Again, let us turn to another side of the case, namely, the uniformity in the rate of change or in the intensity of the force. Because a child grows two inches a year during its early years, an orthodox geologist ought to argue that it does so in old age as well. Because the surface soil in some places grows at a rate to be measured by an inch or two in a thousand years, therefore the Carboniferous beds must have taken millions of years to deposit. As if the famous tree trunks in the Joggins Mines and others we are familiar with in the north, many feet in length and standing upright on their roots, do not make it absolutely plain that sometimes the rate of accumulation must have been exceptionally rapid. Because under certain conditions biological changes of a permanent character take a long time to develop, therefore the myriad new forms which the gardener and the breeder of animals has crowded the world with in the last two centuries, must have taken millions of years to produce.

The fact is that instead of being, as the modern geologist would argue, an exception, catastrophe is an ever-present element in the world's history; and catastrophes, or what are the same thing, phenomena new, unique and unmatched by experience, are present everywhere in the history of the past, if we will look for them and not put on Lyellian blinkers. The whole course of geology presents us with formations and with problems which we cannot match in our present experience. Where, to take a very simple and obvious and recent case, can we in all that record match the boulder phenomena of the drift which extends over two continents, and does so independently of the contour of the country? No current phenomenon gives us even a hint as to how it was produced. Where, again, in human memory can we find among the phenomena of nature anything to compare to the great outbursts of basalt and trachyte which smother whole empires in India and South America with continuous tabular masses, and which have burst out of the ground by methods and processes apparently no longer active? Where can we match the manufacture of granite, the outflow of veins of quartz, the tossing of a huge mountain like the Rigi on end, the crumpling and reversing of the hardest crystalline rocks as if they were butter, such as we pass on our way to St Gothard, the breaking up of the chalk beds of East Anglia and of Denmark. These are mere samples of the effects of forces and of conditions which are no longer active, or rather have not been active within human memory. Potentially they may exist, no doubt, but they can no longer be seen at work. What is to be said for a President of the British Association, who was also President of the Geological Society, who in the face of such facts could deliberately lay it down that nature has always worked, not only by the same methods but with the same intensity that she does now? Uniformity, in the sense here defined, the idol of the modern geologist, underlying his teaching and the prime postulate of his philosophy, is as much a scholastic and ridiculous *à priori* prejudice as that 'all swans must be white,' that 'antipodes are impossible,' or that short-haired women and long-haired men must be exceptionally gifted.

Again, geology is a very complex science: it embodies crystallography, mineralogy, petrology, and biology. Some of its problems necessitate a training in mathematics, in mechanics, in physics, and in chemistry. They cannot be solved without such training; and yet is there any study on which so many uneducated men venture to write with the greatest assurance and impertinence? It is simply appalling to think how many men who have never been out of England lay down the most extravagant conclusions about phenomena that cannot be studied empirically at all in England. How many men whose training just enables them to map a country or to draw a section (assuredly a very elementary kind of land surveying, which few sharp boys at fifteen could not acquire), think that this entitles them to attribute the most astounding and purely imaginary qualities and properties to matter. Men who have never seen a glacier and never experimented upon ice in the laboratory, have published, and continue to publish, endless reams of unutterable rubbish about ice sheets and the movements and properties of ice, entirely based on imagination, while others have speculated and continue to speculate upon the stupendous problems involved in the moulding of the surface of the earth and its internal skeleton with no more knowledge of dynamics than can be gathered from the "Boys' Own Book." And it is sometimes thought strange that geology is treated as a pariah by the sister sciences in which knowledge, precision, and method are deemed essential qualities of fruitful investigation.

This is especially noteworthy among official geologists on both sides of the Atlantic. It is quite shocking to think how remarkably few of them know even the elementary things necessary to their proper equipment, if they are to go beyond the province of recording facts, and to venture into that of speculations involving training in mathematics and physics. Again, what is the use of writing on science in these days, unless we know what others have written upon it? What is the use of publishing observations which have been made, and which have been published over and over again? A man who does not master the literature of his subject, and ventures to write on it as if he was the first person to face the problem, is, in my view, a scientific criminal. He ought to be absolutely tabooed. He is doing everybody a very bad service. Nay, he is dishonest. He is seated in another man's chair. Yet how many official geologists in England, and especially in America, can read any language but their own? How many of them know the absolutely essential languages—Latin, German, and French? How many of them have had any training in mathematics, and physics, and chemistry? And yet we have shelves and shelves of books paid for out of the public purse, and dealing with tremendous problems, such as the so-called glacial period, and written by men who are as guileless of equipment for such a task as children in the nursery are. These books impose upon the great mass of men, because it is thought that every official is, in a sense, an inspired person, and every book with an official binding must contain the secretions of wisdom. I know it will be said that all this is unmitigated impudence. So it is; but I know of no methods but those of impudence which can penetrate the self-satisfied assurance of a dominant school of thought, in which mutual admiration is perpetual, in which discipline requires that every man should subscribe to the Thirty-Nine Articles prescribed by his chiefs, and in which the testing of premises is deemed as unnecessary as the reading up the literature of science before venturing to write upon it. Those who care for geology as an inductive science, and who were taught its lessons by those who were inductive geologists, are disheartened beyond measure by the condition of things now prevailing in England and America, and by the elementary trifling in which so many of the dominant school, who say 'cuckoo' to each other, indulge. There is no way of bursting our bonds but that of indulging in timely impertinence, and this can only be done by those who do not care a fig for conventional orthodoxy, and would rather be burnt with Savonarola than go on repeating worn-out shibboleths with Pope Leo or Pope Pius, with Pope Lyell or Pope Ramsay. I have now laid down some audacious impertinences. If they are false, my superstructure must be a mere building of sand, and I myself must be a more ridiculous person than even some of my orthodox friends have thought. If they be true, I may perhaps claim for my position, at least, a more lasting reputation than can be claimed for a very large part of the orthodox geology of recent years. Having laid these foundations, I shall, in the next chapter, try in some measure to justify my rashness, and grapple more concretely with the problem I set out to face, namely, to assimilate more closely than at present the terminology and methods of geology to the conclusions which accumulated experience seem to render necessary. HENRY. H. HOWORTH.

IV

Dipeltis, a Fossil Insect?

THE figures of the genus *Dipeltis* which accompany Mr Bernard's interesting article in the last number of *Natural Science* are very suggestive. According to Mr Schuchert, they represent fossil Apodidae. To Mr Bernard himself, who adopts Mr Schuchert's view, *Dipeltis* looks remarkably like a cross between an *Apus* and a trilobite; while the first thought that occurs to an entomologist is—how wonderfully like insects these ancient Apodidae must have been. But the entomologist need not stop at this suggestion. He may venture to go further and ask—What greater reason is there for regarding *Dipeltis* as a fossil crustacean than for considering it to be a fossil insect?

The structure of *Dipeltis*, so far as it can be made out from the figures and from Mr Schuchert's description, agrees well in all essential respects with that of an insect; the three large anterior segments are easily explained as the three divisions of the thorax; this is followed by an abdomen, consisting in one species of seven, in the other of ten segments, just about the number we should expect to find; and in one species at least the abdomen bears at the extremity a pair of jointed cerci, not unlike the cerci met with in many different insects.

Unless there be some good and sufficient reason, not to be found stated in Mr Schuchert's paper, it is hard to understand why all these insect-characteristics have been entirely left out of account in discussing the zoological position of the genus. Mr Bernard has personally told me that his chief ground of objection to considering *Dipeltis* as an insect is in the presence and position of the four eyes. That would be a fatal objection, I admit, if Mr Schuchert's interpretation were proved to be correct. But as matters now stand it has no force whatever. There is no necessity to go beyond the actual facts of the case. What has to be explained is the presence, not of eyes and ocelli, but of "two faintly preserved spots" and "two small shallow pits." Assuming that the so-called head-shield of *Dipeltis* is in reality the pronotum of an insect, it requires no effort of the imagination to account satisfactorily for the presence of these pits and spots. It is not at all exceptional to meet with pits and spots and tubercles on the pronotum of insects, nor would it, perhaps, be very difficult to adduce examples in which the pits and spots approximate closely in number and position to the "eyes" of *Dipeltis diplodiscus*. Dr Woodward has described and figured a blattid larva in the *Geological Magazine* for 1887 (decade iii. vol. iv., p. 433, pl. xii., fig. 1). In the figure of that fossil larva two spots are shown near the middle of the pronotum; and on looking through a series of existing Blattidae many species may be found in which either two spots, two pits, or two tubercles occur in a similar position. Four pits may sometimes be seen, though I have not met with any species in which one pair is placed close to the anterior margin.

But, after all, why should so much importance be attached to these eyes of *Dipeltis*, seeing that no trace of them appears in the figure of the second species.

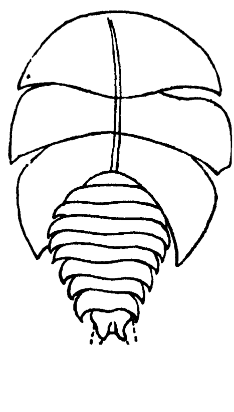

FIG. 1.

FIG. 2.

Outline figure of Carboniferous *Dipeltis* (1) compared with Photograph of Recent Larva of a Beetle (2), natural size.

So far, therefore, as Mr Bernard's objection is concerned, I think the claims of *Dipeltis* to be considered as an insect are fairly well established. And if an insect, the next question is what sort of an insect?

At first sight the figures of *Dipeltis* reminded me most of a remarkable coleopterous larva, a photograph of which I am enabled, through the kindness of my colleague, Mr Gepp, to offer for comparison (fig. 2). Nothing definite is yet known of the life history of this larva. A somewhat similar larva was figured by Westwood in his "Introduction to the Classification of Insects," and was considered by him to belong to the malacoderm series, and to the family Lycidae. In dead specimens the head is always retracted, and concealed, within the cavity of the prothorax.

That *Dipeltis* was not, however, the larva of a beetle may be inferred from the presence of the long segmented cerci in one of the species, though this fact in itself affords no absolute proof. Creatures of the cockroach kind were fairly abundant in Carboniferous times, and it is amongst those that the true relations of *Dipeltis* will probably have to be sought. The abdomen of *Dipeltis* is narrower in proportion to the thorax than is the case with existing cockroaches, but a similar condition may be seen in the figure of the fossil larva referred to above; and if this figure be compared with the figures of *Dipeltis* one cannot help being struck by the agreement in general shape, and in the number and relative size of the parts.

In the facts, apart from the speculations, which are disclosed in Mr Schuchert's account of *Dipeltis*, I find nothing that is inconsistent with the view I have ventured to put forward in this paper; while it is interesting to note that "these animal remains are found in [Lower Coal Measure] nodules associated with *Prestwichia*, *Eurypterus*, malacostracous, schizopod and decapod crustaceans, arachnids, insects, *Aviculopecten* and *Solenomya*, and numerous ferns."

Dr Packard, who established the genus *Dipeltis*, is a distinguished entomologist, and may very well be trusted to recognise a fossil insect when he sees one. But so far as I am aware he has not yet expressed an opinion upon the more perfect remains that have come to light since he first described the genus from a mere fragment of a specimen. I shall be anxious to know what that opinion is, and shall be surprised should it not support my own view of the present question. C. J. GAHAN.

[I may as well at once confess that, until my attention was called to them by my entomological friends, I was quite ignorant of the existence of insects, either adult or larval, showing the habit of *Apus* so nearly as does *Dipeltis*. I venture to submit that very few zoologists, who are not specialists in entomology and acquainted with these particular forms, would recognise anything specially Hexapodan in *Dipeltis*, which does not even show the ordinary insect segmentation with its antennae-bearing head. I do not, therefore, think that much blame attaches either to Mr Schuchert or myself that we failed to detect the insectiform characters of *Dipeltis*, hidden away as they are under the guise of *Apus*.

Leaving, however, this unimportant personal question, I think it may be easily gathered from my remarks on *Dipeltis*, in the paper referred to by Mr Gahan, that the Apodidae would not lose much by giving it up. Its claims to belong to that ancient family required a strong pair of pincers to bring it into line. This is, I think, the chief argument in favour of Mr Gahan's claim that *Dipeltis* belongs to the entomologists: it fits easily into their system, thanks to the existence of certain larval forms, but only with great difficulty into the Apodidan. But for this difficulty, which is, by the way, only revealed by careful analysis, we should have to wait for further evidence before coming to any definite decision as to its affinities.—H. M. B.]

V

Reproductive Divergence not a Factor in the Evolution of New Species

AS it is of fundamental importance to know whether a species can split up into two or more new species in one and the same locality without the aid of any kind of local separation, or whether isolation of some kind is a *conditio sine qua non*, I ask leave to reply to Mr Vernon's article in the last issue of *Natural Science*, in which he tries again to demonstrate mathematically the correctness of his theory of Reproductive Divergence. For the sake of argument and brevity I accept Mr Vernon's figures as being correct, as I did in *Natural Science*, Vol. XI., p. 317. Then we have as the result of the chance intermarrying of 900 parents under ordinary circumstances the following number of offspring (of each sex):

I.	*a.* Parents,	300	short,	300	medium,	300	tall	=	900.
	b. Offspring,	293	„	314	„	293	„	=	900.

If now the fertility of parents of the same size is assumed to be higher than that of parents of different size, the figures are according to Mr Vernon, as follows:

II.	*a.* Parents,	300	short,	300	medium,	300	tall	=	900.
	b. Offspring,	300·6	„	318·8	„	300·6	„	=	920.

By a comparison of the offspring II. *b.* with the offspring I. *b.*, which, however, have nothing to do with one another, the individuals under I. being physiologically different from those under II., Mr Vernon comes to the conclusion that "short and tall individuals have increased by 2·59 per cent., but the intermediate ones only by 1·52 per cent." It is obvious that the figures under II. *b.* can only be compared with those under I. *b.* after the total of II. *b.*, which amounts to 920, has been reduced to 900. But that is a mere oversight in the manipulation of the figures and of no great importance, though the figures will, after the reduction, be very different from what they are now. The grave error in Mr Vernon's interpretation of the figures lies in the sentence following the one quoted before: "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." I have to reply:

(1) That, as the individuals under II. *b.* are not the offspring of those under I. *b.*, but of the individuals II. *a.*, the number of tall and short offspring II. *b.* is smaller than the number of their tall and short parents under II. *a.* after the reduction to 900.

(2) That against every 300 medium individuals three are under II. *b.* only 282·9 short and talls ones.

(3) That Mr Vernon changes the 1·07 per cent., which, by the assumption of greater fertility of parents of the same size, the short and tall offspring II. *b.* gain more than the medium-sized offspring II. *b.* over the respective numbers of the offspring I. *b.*, into an excess of the number of small and tall offspring II. *b.*, over the number of medium offspring II. *b.*, and hence comes to the astonishing conclusion that the 900 parents II. *a.* produce 303·2 short and 303·2 tall offspring against 300 medium ones.

(4) That the before-mentioned gain of 1·07 per cent. on the part of the short and tall offspring II. *b.*, means nothing else than that the curve representing the offspring II. *b.* is not so convex as that representing the individuals I. *b.*

Apart from mathematical details it is evident from Mr Vernon's figures on p. 405, Vol. XI., that the degree of fertility of the three sets of parents of equal size can have no influence upon the proportional numbers of tall, medium, and short offspring, as long as the three sets are equally fertile. The difference in the numbers of the three sets of offspring is entirely dependent on the results of the intermarrying of parents of different size. From the lower figures on p. 405, Vol. XI., we see that there will always be more medium than tall and short offspring produced by these marriages, however much less productive these marriages may be than those of parents of the same size. The number of medium offspring produced by the intermarrying of short with tall parents has certainly decreased by the assumption of lesser fertility of such parents, but so have the numbers of short and tall offspring.

A consideration of another kind will perhaps more obviously show that there is an error concealed in Mr Vernon's mathematical demonstration. The above 920 offspring II. *b.* are produced by 300 short, 300 medium, and 300 tall parents; according to Mr Vernon's own calculations the set of 300 short and the set of 300 tall parents have 300 offspring each, while the 300 medium parents produce 320 offspring. The medium individuals are, therefore, according to Mr Vernon's figures, the more favoured as regards fertility and hence must necessarily gradually replace the tall and small ones, provided (as is assumed by Mr Vernon) that the three sets of individuals have no advantage over each other in respect to other factors.

But the best test of the fallaciousness of the hypothesis we shall have, when we accept Reproductive Divergence as a true factor, and apply this factor again to the two races into which the original species has developed by means of Reproductive Divergence. Let us then suppose that the 900 individuals are split up into a race of 450 short and a race of 450 tall individuals. The mean of the short race is "supposed" by Mr Vernon to be 65·8 inches—why it must be 65·8 inches I do not know. As "the members of this group deviate in e i t h e r d i r e c t i o n (spaced type mine) from the average size [65·8 inches] in the same proportion as the members of the original group did, it follows that 10 per cent. of this group will be below 62·2 inches in height." Certainly; but the average size being 65·8 inches there will also be 10 per cent. above 69·4 inches in height! The members of the tall race will, of course, deviate in a similar manner; the medium size of this race being 70·6 inches there will be 10 per cent. of the individuals below 67 inches. Where is the gap between the two races? Let us, however, suppose there be a gap between them and apply the principle of Reproductive Divergence to the two now separate races. It is self-evident that the short race will split up into a still shorter one and into a taller one, and that the tall race will develop into a less tall one and a more tall one, and so on. Will not the gap which we assumed to exist between the above two races be filled up now? Reproductive Divergence itself will mend the gap which it is claimed to tear into the evenly convex curve of a species.

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VI

The Structure and Habits of the Plesiosauria.[1]

SINCE the discovery of the Plesiosauria and the determination of their characters, which are so completely different from those of all the existing reptiles, palaeontologists engaged in studying them have often attempted to reconstruct them in their outward form and natural surroundings, or, so to speak, to bring them back to life. Conybeare, the describer of *Plesiosaurus dolichodeirus*, which was first found in the form of complete skeletons, quite rightly determined its position among the reptiles, its aquatic habits, and its character as a beast of prey. The diagrammatic sketch of the skeleton given by him also avoids an error which was afterwards repeated and continually exaggerated, since it represents the neck not bent like that of a swan, but only gently curved as a whole. Nevertheless, he states in the text, that he also ascribes to the neck great flexibility and the power of snatching prey, and recognises in the first character a compensation for the want of a large mouth, with powerful jaws and strong teeth, such as the Ichthyosauria possess. He represented the Plesiosaurus as a creature lying in wait for prey in the sea-weed in the shallower parts of the sea near the shore, the body under the water, the nostrils projecting from it for breathing. While a more sluggish mode of life was thus postulated, other workers ascribed to the Plesiosauria a quick restless movement on the surface of the sea, either in the neighbourhood of the coast between the rocks or in the open sea, rapidly darting the long flexible neck and the small head to obtain food. Restorations which illustrate this view, accepted, among others, by T. C. Winkler, Hutchinson, and Koken, are to be found in the works of the two first-mentioned authors, and are based on the assumption of an easily moveable swan-like neck, sharply separated from the barrel-shaped body. This idea was, indeed, carried to such an extreme that Plesiosaurus was compared to a snake drawn through the shell of a tortoise, giving rise also to the idea of a head standing out at an angle from the vertebral column of the neck like that of a bird.

From a possibly more careful examination of the separate parts

[1] Translation of the final chapter of a memoir on the Plesiosauria of the South German Lias Formation in *Abhandl. k. preuss. Akad. Wiss., Berlin*, 1895, pp. 75-80. Revised by the Author, 1897. The illustration (Plate III.) kindly lent by the Berlin Academy.

of the vertebral column of the neck in comparison with that of the trunk, I cannot regard as proved either the outer form just mentioned or the easy power of motion of the neck.

If the head had been fixed at an angle to the vertebral column, the occipital condyle must have been more or less sharply turned downwards, which is not the case, as will be seen from a glance at the figures of the skull of *Plesiosaurus rostratus* and *P. hawkinsi* given by R. Owen. Moreover, the skulls always lie in the same stratum as the vertebrae, and almost always in direct continuation of them. The neck-vertebrae increase very gradually in all dimensions from the head towards the trunk; the neural spines which become higher and higher backwards, as also the strong cervical ribs, indicate a well-developed muscular covering which was capable of moving the neck as a whole, but not its separate parts one on another, such as is rendered possible in the neck of the bird by the saddle-shaped articulations and the almost uniform size of the vertebrae from the first to the last. So highly developed a power of motion is rendered improbable by the almost flat articular ends of the neck-vertebrae of most species, as also by the high neural spines, and especially by the double-headed cervical ribs, which overlap each other like those of the crocodiles. The Plesiosauria could indeed move their neck powerfully and quickly in all directions, but more like an elastic rod as a whole, not in S-shaped bends like the neck of a bird. This conclusion, based on the structure of the neck-vertebrae, also receives important confirmation from the arrangement of the more or less completely preserved skeletons. The drawings of complete skeletons in the works of Conybeare, R. Owen, T. C. Winkler, and Sollas show that the large majority of individuals are preserved with the neck quite straight and lying in the long axis of the animal, or at most only slightly curved. In the latter case the curvature is met with only in the front half, while the hinder portion extends in a straight line. This is also the case in the skeletons in which the maximum curvature is attained, namely those of *Plesiosaurus macrocephalus* and *P. brachycephalus*, R. Owen, both with a remarkably large head and short neck; but even here the curvature of the front portion is very far from being a semicircle.

It is, moreover, to be remarked that the neck was not sharply separated from the body, but gradually passed into the trunk, because the vertebral centra are similar in size, and the neural arches correspond exactly in height and breadth with those of the last neck-vertebrae. This proves also that the long bands of muscle which extended along the upper part of the neck-vertebrae passed into the front of the trunk without an increase in size; and thus the hinder part of the neck-region must have been as thick as the front part of the trunk. No sharp line of demarcation could have been observed externally between the neck and the trunk.

In the representation of the trunk and the limbs given in the figures already mentioned, there is scarcely anything to alter; but the *Plesiosaurus guilelmi imperatoris* now described, permits an important addition to the end of the body. Sollas has already observed remains of the skin in *Plesiosaurus conybearei.* He found it lying upon a part of the dorsal vertebrae and their ribs, and extending as a narrow band over the ends of the neural arches; but the specimen investigated by him showed no trace of a tail fin. It is to be remarked that R. Owen inferred the original presence of a tail fin from the form of the caudal vertebrae, and this idea is now confirmed. It is determined from the parts of the skin preserved that the Plesiosauria, like the Ichthyosauria, possessed a vertical dermal fin at the end of the body. More nearly complete discoveries in the future will perhaps definitely determine its size and shape. The accompanying illustration of the Swabian species (Plate III.) represents it as it seems to me probable from the fragments at my disposal. If the parts go together, as I suppose, its length is approximately given; but whether the height is correct cannot yet be decided. To justify the rhombic form given to it, it may be pointed out that among all the reptiles and amphibia of the present day which possess a vertical expansion of skin at the end of the body, such as crocodiles and salamanders, it ends in a point, and is never split into two lobes like the ordinary tail fin of a fish. It follows from this that, if the vertebral column remains quite straight, the fin may thus be divided equally between the upper and lower side. It is different among the fishes and Ichthyosauria, in which the vertebral column is bent upwards or downwards at the end; and among the teleostean fishes it is not made of a single piece, but results from the fusion of the tail fin and the posterior anal fin. The outwardly homocercal caudal fin is also inwardly heterocercal, and its division into two lobes is in direct relation to this. Plesiosaurus, however, with its absolutely straight tail can thus have possessed only a pointed, undivided, dermal fin, and hence the restoration given here.

This restoration shows yet another difference from those of earlier authors. If the determination of the facts given above is correct we can no longer represent the Plesiosauria as creatures living on the surface of the sea like swimming birds, but they must have lived, like the Ichthyosauria, beneath the surface of the water. For such a mode of life a pointed anterior end of the body is important, and this is here brought about by the elongation of the neck and diminution of the head, while on the other hand the Ichthyosauria have entirely lost the neck, and therefore extended the

head in a long porpoise-like snout. The principal power of locomotion in the Plesiosauria resided in the limbs, as is indeed evident from their remarkable length in proportion to the trunk. The fore and hind extremities were also nearly of equal size. That the latter were as powerful for locomotion as the former is shown by the structure of the pelvis, which, with its great spreading pubis and ischia, afforded space for the attachment of large muscles, just as the coracoids in front provided for the fore limbs. Here also there is an important difference from the Ichthyosauria, whose pelvic elements are reduced to delicate rod-shaped bones, corresponding with the small hind limbs. Further, while the Ichthyosauria relied on the large downwardly heterocercal tail fin for a good part of their locomotion, as recent discoveries in Swabia have shown, the tail fin of the Plesiosauria was less suited for that purpose on account of its smaller expansion. The frequently enlarged neural arches of the last caudal vertebrae indicate that bands of muscle extended into the fin, so that it would admirably serve as a balancing and steering apparatus.

The accompanying drawing (Plate III.) represents two Plesiosauri as they may have appeared according to the conclusions here advanced in reference to their shape and habits. The one individual is swimming below the surface of the sea in search of prey, the other is rising to the surface to breathe. They are represented of about one-twentieth the natural size.

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SOME NEW BOOKS

Wild Traits in Tame Animals

Wild Traits in Tame Animals: being some familiar studies in Evolution. By Louis Robinson, M.D. 8vo. pp. xii. and 329, illustrated. Edinburgh and London: Blackwood & Sons, 1897. Price, 10s. 6d. nett.

Darwin made a great point of the insight afforded into the habits of their bold ancestors by many traits exhibited by our domestic animals, such as the scratching of tree-stems by cats to clean or sharpen their claws, the turning round of a dog before settling to slumber on the hearthrug, and the pawing of snow by horses. And it has been the object of the author of the work before us to hunt further instances of such ancestral traits, both as regards bodily and mental habits.

The author has a pleasant and attractive way of expressing himself, so that he is likely to draw a much larger circle of readers than many writers of popular science. And some of his inductions are well founded and of considerable interest. For instance, he tells us that from the quiet unaggressive way it takes its food, it may be inferred that a cat is naturally a solitary animal, whereas, on the other hand, a dog must be a gregarious creature, forced to get its food by snatching it as quickly as possible in order to prevent its being seized by a rival. And, although it is perfectly obvious, we do not recollect to have seen it expressed elsewhere that "recognition-marks," like the white under surface of the tails of fallow deer and rabbits are actually injurious to the individual, and advantageous only to the tribe as a whole.

If all the inductions and theories of the author were as well founded as the above, there would be little or no scope for destructive criticism. But there are some, which, to put it mildly, are more than doubtful. As an example may be cited the supposed mimicry of snakes by cats, both as regards coloration and hissing. This appears to be based on a certain resemblance, so far as coloration is concerned, between an ocelot when coiled up and a boa. And it is argued that in early times all cats were small and coloured somewhat like the ocelot. There are many objections to this startling generalisation, but it will suffice to say that the ocelot type is almost certainly not the original feline coloration. Again, as regards the ancestry of our European domestic cats, it is seriously argued that the tabby, grey, and tortoise-shell-sandy breeds are derived from as many unknown extinct species. This obviously implies a want of acquaintance with what constitutes a species, and the great colour variability inherent in domesticated animals. If such a theory was admitted, what would become of Darwin's conclusions as to the common ancestry of our domesticated pigeons? But the most astounding suggestion of all is that the tortoise-shell-sandy breed is derived from a wild species of which the females inhabited scrub, and the males open sandy districts. Is there any evidence of a wild animal of any kind in which the two sexes have such dissimilar habitats?

In his introduction, the author is somewhat severe as to the treatment accorded to the amateur naturalist by the specialist; and he seems to fear that the former class is in danger of extinction by the latter. If amateurs will indulge in wild theories, there is in truth some likelihood of their not receiving the attention they undoubtedly merit at the hands of their professional brethren. For both there is plenty of good work remaining to be done, the amateur, as a rule, being the one by whom our knowledge of animal habits is advanced. Before, however, he proceeds to theorise it is essential that he should have a fair grasp of the leading principles of zoology; and if that be attained, he has the chance of advancing some brilliant hypothesis, which, from over caution, the specialist would never dare to propound.

If the reader be sufficiently advanced as not to take for gospel all that he finds set before him, there is much worthy of his best attention in this latest contribution to a very interesting subject.

Verworn's Physiology

Allgemeine Physiologie. Ein Grundriss der Lehre vom Leben. By Max Verworn, a.o. Professor der Physiologie an der medicinischen Facultat der Universitat Jena. Second Edition, with 285 illustrations. 8vo, pp. viii. 606. Jena: Gustav Fischer, 1897. Price, 15 marks.

The first edition of this work has already been fully reviewed in these pages; the second, therefore, calls for no more than a brief notice of new features. The addition of seventeen new illustrations, and the incorporation of some new matter, have increased the bulk of the volume, but there is little material alteration in the text, except that one section of the first chapter has been rewritten and much shortened, and now bears the heading "Psychomonismus," instead of the original "Psychologische Methodik."

The figures are, on the whole, very much better executed than those in the first edition; some which were unimportant have been omitted, some others improved upon (see the figure of *Orbitolites* on p. 98), and many new ones added which are a distinct gain to the work. Among the latter, the full-page illustrations of *Rhizoplasma kaiseri* (p. 289) and of *Hylopus* (p. 333) are worthy of special mention. We note that the elaborate "Quadrille des centres," from Boveri, is now conspicuous by its absence. One feature in the new edition which we do not regard as an improvement is the use of the same figure to illustrate different points. These repetitions occur with irritating frequency; for instance, a very commonplace *Amoeba* appears no less than three times, on pages 88, 97 and 172; a dividing ovum (after Boveri) on pages 72 and 199; while the familiar *Paramoecium* and *Spirogyra* are reproduced twice respectively. We fail to see the necessity for this system of "carrying forward"; a reference to a previous page would have met the wants of a student who could not bear in mind for eight pages the details of structure of an amoeba, and lazier readers should not receive consideration.

Nemertine Worms

Traité de Zoologie. Publié sous la direction de Raphael Blanchard. Fasc. XI. Némertiens, par Louis Joubin, professeur à l'Université de Rennes. 8vo, pp. 59, and 53 figg. in text, 18 of them coloured. Paris: Rueff & Cie., 1897. Price, 2 fr.

Professor Joubin has given us a concise, well-arranged, and very readable account of the Nemertines. After giving a short sketch of their biology and general anatomy, he proceeds to give a comparative review of the different organs, or systems of organs, in turn, followed by a very clear account of the three types of development, and concluding with a few pages on the systematic arrangement of the group and its relation to other groups, especially to the Turbellaria. It is, however, to be regretted that, in his anatomical part, he employs the term "coelome" to designate what has no more morphological than it has etymological right to the name, and without even attempting to justify his use of it, or perceiving that it is necessary so to do. There are also a few apparent discrepancies in his descriptions, arising from the fact that, while he admits Burger's classification to be the more natural, and adopts it finally in the systematic part, he uses Hubrecht's names for the "sections" throughout the anatomical part, and does not seem always to remember that they do not connote the same thing as the names used by Burger. It would have been clearer, as well as more consistent, to have used throughout the same classification.

Most of the figures are diagrammatic, and it is, we think, unfortunate that so many of these are of the composite type. Not only are such figures misleading, since they represent structures side by side which do not so occur in nature (as, for instance, in the section of the epidermis on p. 11), but also it is very difficult to make them strictly accurate. Thus, by representing in one diagram the proboscis of a nemertine invaginated on one side and evaginated on the other, the impression is produced that evagination is due rather to the drawing back of the proboscis-sheath than to the pushing out of the walls of the proboscis itself, since this organ is no further forward in the one case than in the other, while its junction with its sheath is further back on the evaginated side. Again, one would gather from Fig. 18 the erroneous impression that the vascular system of *Carinella* ended posteriorly in the same way as that of *Carinoma*, also that all species of *Carinoma* had the two blindly-ending longitudinal vessels anteriorly that are there represented; and from Fig. 21, since nothing to the contrary is stated in the text, that the excretory system of *Amphiporus*, and of all species of the genus, communicates with the exterior by five pairs of ducts, whereas Oudemans, from whom, as we are told, the diagram is taken, not only expressly states that in some species there are but two ducts, but also that in the particular species to which the diagram refers the number is not even the same on the two sides of the body. Prof. Joubin, as it happens, represents on the right side what Oudemans made out to be the arrangement of the excretory system on the left.

It would also have been an advantage to the reader if the same reference letters had been used for the same structure in all the figures, and if the text and figures had harmonised better with one another. One does not know, for instance, whether one is to believe the text or the figure with regard to the position of the deep glands of the Schizonemertines (p. 12 and Fig. 7B), or with regard to the position of the nerve-cord in *Cephalothrix* (p. 29 and Fig. 7A); and it is also somewhat confusing to find that in the description of the characteristic features of the proboscis of the Unarmed nemertines (p. 16), the figure of a papilla in an Armed one is referred to, while it is nowhere mentioned that, in the latter group, papillae occur at all. Fortunately, however, a bibliographical index is given, so that it is not necessary to remain long in doubt as to the facts.

CYCLOPS

BULLETIN OF THE ILLINOIS STATE LABORATORY OF NATURAL HISTORY, URBANA, ILLINOIS. Volume v. Article ii. A contribution to a knowledge of North American Fresh-water Cyclopidae. By Ernest B. Forbes, B.S., 1897.

ANALES DEL MUSEO NACIONAL DE BUENOS AIRES. Tomo v., pp. 331-332. Sur quelques Entomostracé's d'eau douce des environs de Buenos Aires. Par Jules Richard, 1897 (Apareció el 8 de Junio).

FIFTEENTH ANNUAL REPORT OF THE FISHERY BOARD FOR SCOTLAND, Part iii. The Invertebrate Fauna of the inland waters of Scotland. Part vii. By Thomas Scott, F.L.S., Mem. Soc. Zool. de France. Plate ix. 1897.

MR ERNEST B. FORBES has utilised an academical essay to adjust the synonymy of the fresh-water Cyclopidae of North America. Unfortunately he has to admit that his revision of the group is still incomplete. He institutes two new subgenera. Some people like subgenera; others regard them as scientific misfortunes. In compensation Mr Forbes cancels several species too hastily introduced as new by American writers. These results have evidently not been won without much laborious and conscientious comparison of specimens. Whether all Mr Forbes' conclusions are valid it would be premature to decide. At the outset he lays stress on the fact that in the Cyclopidae, "of all the specific characters, the most valuable are those derivable from the *receptaculum seminis*." Then, on the ground of certain microscopic characters, he upholds the *Cyclops edax* of his instructor, Dr S. A. Forbes, as a distinct species from *Cyclops leuckarti* Claus, although he himself says "the structure of the *receptaculum seminis* is identical in the two species, so far as I can tell." But of *Cyclops edax* he makes *Cyclops annulatus* Wierzejski a synomyn. Rather opportunely, therefore, Dr Jules Richard has just published a figure of the *receptaculum seminis* in *Cyclops annulatus*, relying on it as a character distinguishing that species from the neighbouring *Cyclops leuckarti*. But if the *r. s.* in *C. edax* agrees with, and the *r. s.* in *C. annulatus* differs from, that in *C. leuckarti*, it would follow that *C. edax* and *C. annulatus*, cannot be identical, or else that the *r. s.* does not deserve to have much stress laid upon it.

The most notable feature separating *C. edax* from *C. leuckarti* is afforded by the hyaline plate of the last joint in the first antennae. In the latter species this has a characteristic notch, not present in the former. Mr Thomas Scott, who has within the last few weeks added *C. leuckarti* to the known fauna of Scotland, makes a reference to this notch, but speaks of it as a feature of adult specimens, though without definitely stating that it is not found in younger stages. The coincidence of three writers in different parts of the world independently busying themselves with the characters of this little species, of the length of one millimetre, must give an impression that in this age the fauna of the globe is being very carefully scrutinised.

A System of Bacteria

System der Bakterien: Handbuch der Morphologie, Entwickelungsgeschichte und Systematik der Bakterien. By Dr W. Migula. Band I., Allgemeiner Teil. 8vo, pp. viii + 368, with 6 plates. Jena: Gustav Fischer, 1897. Price, 12 marks.

Of the making of books on bacteriology there is no end, yet as very many of these books are scarcely worth the paper they are written on, any really solid contribution to bacteriological literature ought to meet with due appreciation. Dr Migula's first volume gives ample promise of a very useful work, and contains a general survey of the classification, morphology, and development of schizomycetes. It is much more than a text-book; it is full, not only of well-digested information, but of original facts and suggestive hypotheses. The first section is devoted to a historical review of the classification, which concludes with the author's own scheme, a later modification of that already published by him in Engler and Prantl's work. The second and most interesting section deals with morphology and life-history, and the chapters treating of controversial questions, such as spore-formation, pleomorphism, etc., are worth reading, not so much for their weight as evidence as for their suggestiveness. Argument is perhaps hardly Dr Migula's strong point, and we think he is too apt to put forward as facts statements which, while unsupported as they often are by the quotation of a single instance, we are bound to regard as assumptions. These very statements, however, often contain ideas well worth working out to their logical conclusion. The question of pleomorphism is more or less begged, but we gather that Dr Migula is not so strong an opponent of the theory as we were led to believe by his earlier writings. The pros and cons are at any rate retailed with admirable fairness. The third section is concerned with the discussion of chiefly physiological characters and various biological criteria of species.

On the whole, the author and his readers are to be congratulated on this first instalment of the work. The plates are very good, and the bibliography at the end of each chapter exhaustive and exact.

Another Work on Bacteria

Vorlesungen uber Bakterien. By Dr Alfred Fischer, a. o. Professor der Botanik in Leipzig. 8vo, pp. viii + 186, with 29 figs. Jena: Gustav Fischer, 1897. Price, 4 marks.

Dr Alfred Fischer's book does not need the apology he makes for its publication in his preface; a book on non-pathogenic bacteria, or, more strictly, one from which medical bacteriology is entirely excluded, would supply a real want, and the work in question might have answered this purpose if the subject had been given fuller treatment in a less popular form. It is very good as far as it goes, but, like Oliver Twist, we 'ask for more,' and it is to be hoped that in a second edition, which is sure to be called for, Dr Fischer will remedy this defect, and expand his modest volume into a work at once comprehensive and detailed enough to satisfy the demands of the researcher as well as of the student. The author writes very clearly, and is to be congratulated on the exactness of his terminology. As an example of his accuracy in description, he confines his use of the word 'Faden' to strictly filamentous forms, whereas nine out of ten writers habitually apply this term—or its English bacteriological equivalent 'filament'—

to such distinct structures as a true sheathed or unsheathed filament, a branched zoögloea, and a moniliform series of cocci. To make confusion worse confounded in this way is inexcusable, and we hope Dr Fischer's better example may find followers. The captious critic might perhaps suggest that, in using the word 'zusammengesetzteste' (p. 3), the author, without malice prepense, casts a slur on the German language which it does not deserve; it is certainly rich enough to supply him with a less barbarous superlative. The book deals almost exclusively with such bacteria as are concerned in metabolism, fermentation, nitrification, and various physical and industrial processes; it contains brief chapters on morphology, classification, distribution, habitat, and conditions of life, the general principles of nutrition and culture, respiration, and effects produced by chemical and physical agencies, with a more detailed account of the relation of micro-organisms to nitrogen and carbonic acid, and a short mention of bacterial diseases affecting plants and animals. The figures in the text are excellent; many are new, at any rate to text-books, which are too often illustrated only by miserable reproductions from the most antiquated sources. Altogether the book is one which may be read with both pleasure and profit in its present form, but which merits and might command a wider circle of readers.

BOTANY IN THE FIELD

OPEN-AIR STUDIES IN BOTANY: SKETCHES OF BRITISH WILD FLOWERS IN THEIR HOMES. By R. Lloyd Praeger. 8vo, pp. xiii + 266, with frontispiece, 6 plates and 68 illustrations in the text. London: C. Griffin & Co., 1897. Price, 7s. 6d.

"Come forth into the light of things,
Let nature be your teacher."

THE quotation from Wordsworth which appears in the title page of this book is an appropriate text for the matter contained in its eleven chapters. In a series of brightly written descriptive accounts of familiar scenes in our own islands, Mr Praeger contrives both to interest the reader and tell him a good deal worth knowing about plants. "We stand," he says, "in fancy, out in the open country, with the wild flowers at our feet, the hum of insects and the rustling of the wind in our ears, and the blue sky overhead, and we use these powers of observation that have been given to us. Thus only can we hope to comprehend the life of a plant, or of a plant-community, and appreciate the conditions under which each species lives, and the adaptations by which each is able to maintain its position in the plant-world, and fulfil its proper functions." It is not a text-book, and with students working merely for examination will not find favour; but to nature-lovers in general, and wild-plant lovers in particular, "Open Air Studies" will be a very welcome and a very healthy companion and guide. Being resident in Ireland, the author has taken most of his scenes "from the meadows and mountains, streams and seashores of that fair land," and the addition of place and date beneath the title of each chapter gives a circumstantial appearance, and adds a spice of interest to what follows. But as the spots selected are not exceptional in their vegetation, and familiar flowers not rarities are described, the majority of the scenes might be equally well located in similar places in any part of the British Isles. They are as follows:—A

Daisy-Starred Pasture, Under the Hawthorns, By the River, Along the Shingle, A Fragrant Hedgerow, A Connemara Bog, Where the Samphire Grows, A Flowery Meadow, Among the Corn—a Study in Weeds, In the Home of the Alpines, and a City Rubbish Heap. We get an idea of the characteristic vegetation of each place, and the individual plants are made the pegs on which to hang facts of elementary botany; and, on the whole, the science is not by any means so disjointed as might be imagined from this method of treatment. He who has visited these or similar scenes with Mr Praeger as his guide, will have got knowledge enough to enable him easily to acquire more, and thereby to satisfy a craving which we are quite sure he will feel.

Our notice would be incomplete without some reference to the illustrations. Those in the text, the work of Miss Rosamund Praeger, are useful, though by no means ambitious; but the plates which are reproductions of photographs, taken by Mr R. Welch, are very nice, the frontispiece, "When Daises Pied," the group of Reed-mace on the Boyne (Plate I.), and a Cavan Meadow (Plate VI.), showing Purple Loosestrife and Meadow-sweet, call for special mention, and so do his other four.

BRITISH WILD FLOWERS

FAMILIAR WILD FLOWERS. Figured and described by F. Edward Hulme, F.L.S. Cheap Edition. 5 vols. 8vo, with 40 coloured plates in each. London: Cassell & Co., 1897. Price, 3s. 6d. each volume.

WE are glad that Messrs Cassell have issued Mr Hulme's sketches and chats on familiar wild flowers in such form as will bring them within reach of many flower lovers. Our 'wayside weeds' are so beautiful, and their study is such a healthy, restful recreation, that any genuine effort to bring them nearer to us, and to help the would-be student, is always welcome. But we rarely find a teacher who can give us a pleasing and life-like picture as well as tell us, without being prosy or technical, something worth knowing about our plants. Mr Hulme is not a botanist; much of what he says is irrelevant gossip, and might well have been replaced by useful facts. But he does make us feel what pretty and interesting objects are to be found in the country lanes, the fields, and the woods. He is not by any means systematic. Beginning with the field convolvulus, we come next to the field rose, then the meadow crane's bill, then silverweed, apple, borage, and so on, higgledy-piggledy all through the five volumes. The only sop to the scientific is a brief summary at the beginning of each volume, while a short botanical diagnosis, such as we find in the "Student's Flora," is given for each plant contained therein. It would have been quite easy to arrange the two hundred odd species in some systematic order, and such arrangement would have pleased everybody. The book would then have been a useful companion to any of our smaller British floras, and thereby a great help to the more serious student, while even the most popular-minded person would, we think, prefer to find his roses or his buttercups associated in the same volume. We would also suggest that some of the plates are getting very worn; most of them, in fact, look best by candle light, but some, like that of the periwinkle in the first volume, had better been left out altogether.

As regards the text accompanying the plates, we see from it at once that the author has an eye for the general form and habit of the plant, which he describes in simple language, and tells where it grows and thrives best, and when we may find it in flower. We learn something too of its history, and what Gerarde, Ray, and many another old botanist (whose name and work is often unknown to the modern student, for whom botany was founded by Sachs) thought about it. The popular names afford scope for a good deal of speculation; in many cases where they originate from the economic or medicinal use to which the plant was put in olden times, or bear some relation to the shape of the flower or the leaves, the derivation becomes sufficiently obvious. But often the writer walks on very thin ice; and we are bound to admit that a popular name was vaguely applied to several plants whose appearance for instance coincided with some well-marked recurring event, such as the arrival of the cuckoo in the case of the numerous 'cuckoo flowers.' We might say more about this handy little series of volumes, but we have said enough to recommend them to those who want an inexpensive, popular and, within limits, useful account of British plants.

A French Text-book of Botany

Traité de Botanique. By L. Courchet, professeur d'histoire naturelle à l'école supérieure de pharmacie de Montpellier. 2 vols. 8vo, pp. viii + 1320, with 514 figs. Paris: Baillière, 1897. Price, 18 francs.

Prof. Courchet has added one more to the long list of text-books, and has produced a book no worse than many other treatises in botany, and better than some. According to his preface it embodies the course of lessons which, for seven years past, he has put before his students at Montpellier, and which he tells us has been attended with happy results. We hope, for the students' sake, that the course included well-arranged practical work; as an accompaniment to such the two volumes might be made useful, but taken alone would prove very tedious reading, for the attractiveness supplied by the illustrations, which are fairly numerous, is counterbalanced by repellent-looking tables which make one feel giddy and thankful that examination days are past. The most useful portion of the book is the first part on general botany—an introduction to the morphology and physiology of plants, with a chapter on the principles of classification. The whole of this occupies only 176 pages, and for the size of the book should have been much expanded. The least satisfactory portion is that dealing with physiology. The account of the modification of the various members is also extremely meagre. The second part, 'Special Botany,' is divided into two—(1) Embranchement des Cryptogams, and (2) Embranchement des Phanérogams. This demarcation of plants into two groups is not authorised by the present state of our knowledge and should be dropped. The definitions given of the sub-divisions are often inexact, as for instance in the case of Phanerogams (seed-plants would be a preferable term), which are defined as plants on which the highly differentiated sexual organs constitute a flower, and this, in spite of the fact that on p. 81 we are told that comparable associations of reproductive organs, to which it is difficult to refuse the name of flower, are found in other groups. The account of the Cryptogams occupies pp. 177-363, and all the rest of the book is given up to seed-plants. It is to be regretted that Prof. Courchet did not make himself acquainted with recent work in the Fungi, especially as regards the question of a sexual process; and a better and longer account should have been given of the Bryophyta. The arrangement of the seed-plants is practically that of Eicheer, but the treatment is not calculated to excite enthusiasm on the part of the student. It is full enough, in fact too full; the omission of the formidable-looking small print keys to the families, orders, and genera would have saved much space in the book and many groans on the student's part. A good feature is the description of a well-known plant in illustration of some of the natural orders; provided the plant is a typical one the student gets a concrete knowledge of the order of which it is a representative. Enough has, however, been said to give those interested a notion of the value of Prof. Courchet's hand-book, and also to suggest wherein improvements may be made should other editions be called for.

Faulty Geology

Failles et Géogénie d'après les Observations et Découvertes faites dans le Nivernais. By F. Lefort. [Publié par les soins de l'Institut Scientifique du Sacré-Cœur à Paray-le-Monial.] Pp. 259, with 6 pl. of sections, and one large map (1 m. 10 by 1 m. 20.) 4°. Paris: H. Le Soudier, 1897. Price, 20 fr.

"Geology is the study of the planet on which we live. Its special aim is the definition of the different mineral materials which compose the earth," begins M. Lefort. It would be discourteous to M. Lefort after this assurance of his belief to express any doubt as to whether this be his real idea of the special aim of geology. But if it be, then geology must have an extra special aim—the confusion of the evolutionists and the justification of Moses. M. Lefort's work is a bulky quarto, and it is illustrated by an enormous map of the Nivernais; but unfortunately neither of them can be regarded as a serious contribution to scientific progress; the map is cut up by a vast series of faults, which cross at regular angles, and make the map look like a mosaic of lozenge-shaped pieces of different colours. The faults are classified according to their exact orientation. It is more pretty than plausible. The Nivernais has been mapped by the Geological Survey of France, and most of the author's faults are omitted from the official maps. We must say that on comparison of the two, the latter appear by far the more plausible.

The text is not easy to summarise, but a series of quotations will illustrate the author's standard. There is a great deal of petrology in the volume, and its value can be estimated from the following: "The granites are more ancient than syenites and porphyries; after them come the trachytes and basalts, and finally the lavas of modern volcanoes." As an example of his palaeontology we may quote the following: "Les plus anciens crinoides sont supérieurs à leurs plus récents congénères; les échinides qui ont possédé 125 espèces n'en comptent pas aujourd'hui 50. Les poissons des premiers temps l'emportent sur ceux qui peuplent actuellement nos mers. Les labyrinthodontes, amphibiens qui vont apparaître tout d'un coup gigantesque pendant l'ére Triasique, ne sont pas moins supérieurs aux batraciens qu'ils ont devancés. On ne leur connait ni ancêtres ni descendants" (p. 16). The author also quotes with obvious approval an expression of surprise that a "célèbre professeur de géologie à l'Institut catholique de Paris" should have written a history of the world, "sans citer une seule circonstance où Dieu est intervenu." This mistake the author of the present work certainly manages to avoid. "Il y a," he tells us, "des cataclysmes recurrents," possibly to be attributed to the Evil One; and he continues, "chacune de ces révolutions a anéanti les êtres vivants. La création d'une faune nouvelle a été nécessaire chaque fois ensuite, pour repeupler la terre."

The view that man has descended from some lower vertebrate is dismissed very emphatically, with language which is clearer than the reasoning: "L'existence de reptiles vertébrés munis de vertébres extrêmement rudimentaires, pendant que les os de leurs membres ont des épiphyses, et que les os craniens sont très développés, donne un démenti formel à la croyance que l'homme puisse dériver d'un animal vertébré."

The view of the antiquity of man is even more summarily dismissed: "L'idée d'un homme qui aurait vécu à l'époque Tertiaire est une hypothèse gratuite, qu'aucun savant de bonne foi n'a pu étayer d'un seul fait positif." Man dates only from the Quaternary, which is one of the three periods of the "Contemporary Epoch," of which the total duration "ne dépasse certainement pas 80 siècles."

From the above extracts our readers can form their own estimate of the value of the book. Possibly its main object was to inform the authorities of the "Institut Catholique de Paris" that, should they want a new professor of geology, it is quite possible to get men who can write big books about geology, and who are orthodox enough for cardinals.

A Guide to Zermatt

The Valley of Zermatt and the Matterhorn: A Guide. By Edward Whymper. 8vo, pp. xiv+212, with 2 Maps, Panorama, and 77 Illustrations. London: J. Murray, 1897. Price, 3s.

It is not often that guide-books have any literary merit, or any value beyond the immediate needs of tourists, except for topographic reference. Mr Whymper's guide to Zermatt is an exception. The illustrations are all of artistic merit; the text is not only full of exactly the information that a tourist wants, but it is written in perfect English. The book is full of touches of subtle humour, and tells such tersely and thrilling stories that it is as racy and interesting as a novel. The first chapter narrates the history of Zermatt before climbing days, and quotes the evidence to show that the Theodole Pass into Italy was used in Roman times. The first English visitor went to Zermatt in 1800, while English mountaineering began there with the ascent of the Strahlhorn in 1854 by the three brothers Smyth. The succeeding four chapters tell the story of the first attempts upon the Matterhorn, of the first successful ascent and its tragic end, and of the subsequent history and accidents. Then follows the information as to the routes to Zermatt, an account of the present village and its resources, and of the principal excursions. Much valuable information as to guides and their tariffs, lists of peaks and passes, and tables for the conversion of metres into feet, are given in the appendix. The work is a model of what a guide-book should be.

THE GEOLOGY OF CHELTENHAM

CHELTENHAM AS A HOLIDAY RESORT. Part I. THE NEIGHBOURING HILL COUNTRY. By S. S. Buckman, F.G.S. 8vo, pp. vii. 100. Cheltenham, 1897.

THIS little work comprises a series of gossiping articles which were originally published in the *Cheltenham Examiner*, and which are descriptive of that charming region, the Cotteswold Hills. With this country Mr Buckman, and his father before him, have been associated for a long period, though not without interruption, for the parental home was transferred from Cheltenham to Cirencester, and finally into Dorset. Leaving this southern county, our present author has found successive homes at Andoversford, Stonehouse, and Charlton Kings, near Cheltenham, and he has not only made himself familiar with the scenery and geology of the breezy uplands and the picturesque combes, but has written some amusing sketches of the manners and customs of the rural inhabitants.

His present object is mainly to serve as a guide to those in search of the picturesque, although not overlooking the needs of those in search of scientific information. The work is therefore hardly open to scientific criticism, it is for the casual visitor rather than for the geological student—for the latter would find most of the detailed information he wants in Witchell's "Geology of Stroud" (1882), while Lycett's "Handbook to the Cotteswold Hills" (1857), though forty years old, will ever remain of great value and interest to the geologist.

The author's geological notes refer mainly to the Inferior Oolite, and he enumerates in his first section the several local subdivisions of the freestones and ragstones. He speaks of a "new school of geologists" who give names to the time during which the rocks were deposited—but surely Quenstedt and Oppel forty years and more ago recognised the need of chronological terms and used them, and no geologist has, so far as we are aware, disputed their value apart from and in addition to the local stratigraphical terms. The author would have done well to be a little more explicit in the use of such terms as "*Valdani* beds" and "*Striatus* beds," mentioned on the very first page, although he observes in his preface that "technical information has been purposely avoided." The term "*Valdani* beds" has rarely been used in this country. We may trust, however, that this little work will stimulate enquiry, as a great deal of useful information has been gathered together by the author on archaeology and topography as well as on his special subject of geology; and he notes here and there favoured spots suitable for "picnics," information which may be alike useful to the casual visitor as well as to the severer student of natural science.

A DESCRIPTION of the Great Auk (*Alca impennis*) in the Natural History Museum at Amiens, France, has been published by H. Duchaussoy (Piteux, Amiens, 1897).

SERIALS

BARTH, Leipzig, is now publisher of the *Zeitschrift für Psychologie und Physiologie der Sinnesorgane.*

Timehri, the well-known publication of the Royal Agricultural Society of British Guiana, is discontinued with the December number.

An interesting series of papers on the Geology of the Congo Basin is appearing in *Le Mouvement Géographique*. They are compiled by Professor Jules Cornet.

We hear that, with the retirement of Mr H. C. Mercer and Prof. H. C. Warren from the board of associate editors of the *American Naturalist*, anthropology and psychology will no longer occupy the pages of that journal.

Mr G W. Murdoch, the able editor of the Natural History column in the *Yorkshire Weekly Post*, is preparing a guide to Lakeland, shortly to be published by Messrs Ward, Lock & Co. It will contain an exhaustive chapter on the zoology and botany of the district.

The Asa Gray Bulletin is a botanical journal, formerly the organ of a chapter of the Agassiz Association in the United States. With its June number it began to appeal to the general public in the shape of a bi-monthly magazine, published at Washington, D.C., price 50 cents. It is edited by G. H. Hicks with the help of A. J. Pieters, C. C. Dubois, and L. H. Dewey.

The *American Journal of Archaeology*, hitherto edited by Professor Forthingham, is now under the control of the Archaeological Institute of America, which will for the future issue its papers, reports, &c., in a regular and uniform publication. An editorial board, elected by the Council of the Institute, with Professor John H. Wright of Harvard as editor-in-chief, will conduct the new series of the journal. The Macmillan Company, New York, are the publishers.

With the December number of our lively and useful contemporary, *Nature Notes*, the editorship passes out of the hands of Mr James Britten, who has held it for six years, into those of Mr J. L. Otter, of 16 Vernon Terrace, Brighton. This journal has hitherto been remarkably successful in excluding faddists from its pages, and we trust the new management will be equally skilful in avoiding an obnoxious race who too frequently mar the pages of popular magazines.

L'Intermédiaire des Biologistes is the title of a new journal devoted to biology, and published in Paris. The first number appeared on 5th November 1897, and the second on the 20th of the same month. The aim of the Journal is to be a sort of *Notes and Queries* of zoology in all its branches in the first half of each number, and a review and list of new literature in the second half. Such an aim should find many supporters. *L'Intermédiare* will appear fortnightly; the subscription for the Postal Union is 12 francs; and the publisher is C. Reinwald, 15 rue des Saints-Pères, Paris. The Editors are Alfred Binet and Victor Henri.

FURTHER LITERATURE RECEIVED.

RECUEIL des Lois de la Biologie Générale, A. L. Herrera : Mexico. Nature and a Camera, R. Kearton : Cassell. Petrology for Students, ed. 2, A. Harker : Cambridge Press. Hand-book to the Geology of Cambridgeshire, F. R. C. Reed : Cambridge Press. The Argument of Adaptation or Natural Theology Reconsidered, G. Henslow : Stoneman. The Lepidoptera of the British Islands, vol. iv., C. G. Barrett : L. Reeve. Cape of Good Hope Meteorol. Comm., Rainfall 1885-94, A Buchan. Bericht uber die Verwaltung und Veimehrung der Konigl. Samml. f. Kunst u. Wissensch. Dresden, 1894-95. Museum of S. African Republic, List of Acquisitions, Oct. 1897. Johns Hopkins Univ. Circular, vol. xvii. no. 132. Proc. Roy. Soc. Victoria, vol. x. (n. s.) pt. i.

Earthquake of 12th June, R. D. Oldham : *Rec. Geol. Surv. India.* Fractional Crystallization of Rocks, and The Witwatersand Banket, G. F. Becker : *Amer. Journ. Sci.* and *18th Ann. Rep. U. S. Geol. Surv.* On *Spencerites*, D. H. Scott : *Abstr. Proc. Roy. Soc.* Phylogeny and Taxonomy of Angiosperms, C. E. Bessey : *Botan. Gaz.* Untersuchungen uber die Fauna der Gewasser Bohmens, iii , A. Fritsch and V. Vavra : *Archiv. Naturw. Landesuntersuch. Bohmen.* On *Chlamydoselachus anguineus*, Garm., a remarkable shark found in Norway, 1896, R. Collett : *Univ. Festskrift Christiania.* Morphological and Biological Observations on South African Plants, also Nesting Habits of *Tockus melanoleucus*, S. Schonland : *Trans. S. African Phil. Soc.*

Amer. Journ. Sci., Dec.; l'Anthropologie, Sept.-Oct.; Botan. Gaz., Nov.; Feuille des Jeunes Nat., Dec. ; Halifax Nat., Dec. ; Irish Nat., Dec. ; Journ. School Geogr., Nov. ; Knowledge, Dec. ; Literary Digest, Nov. 13, 20, 27, Dec. 4, 11 ; Naturae Novit., nos. 21, 22 ; Naturalist, Dec. ; Nature, Nov. 25, Dec. 2, 9, 16 ; Nature Notes, Dec. ; Naturen, Nov. ; New Age, Oct. ; Rivista Quind. Psichologia, vol. i. fasc. 12-15 ; Revue Scientifique, Nov. 20, 27, Dec. 4, 11 ; Science, Nov. 12, 19, 26, Dec. 3. Scientific American, Nov. 13, 20, 27, Dec. 4 ; Scot. Geogr. Mag., Dec. ; Scot. Med. and Surg. Journ., Dec. ; Victorian Nat., Nov. ; Westminster Review, Dec.

OBITUARIES

RUDOLF P. HEIDENHAIN

BORN AT MARIENWERDER, JAN. 29, 1834. DIED OCTOBER 12, 1897

SON of a medical practitioner, Heidenhain was from the year 1859 until his death, professor of physiology and histology at the University of Breslau. He became celebrated principally through his work on the physiology of the muscles and of the nervous system, and on the functions of glands.

The chief of his important researches on the muscles were his *Mechanische Leistung, Wärmeentwicklung und Stoffumsatz bei der Muskelthätigkeit*, Leipzig, 1864; his original experiments on the tone of the muscles (1856), on muscular tetanus (1858), on pseudo-motor nervous actions (1883), and a remarkable series of studies on the innervation of vessels (1869-1878). To this series may also be added the memoir, written in collaboration with Bubnoff—*Ueber Erregungs- und Hemmungsnerven innerhalb der motorischen Hirncentren* (*Archiv. f. die ges. Physiol.*, 1881),—which propounds and develops, although hypothetically, one of the most important laws of nervous action, namely, that in a normal state every excitation of a nerve-centre gives rise to a process of opposite sensation, consequently tending to put an end to the motion previously provoked.

Amongst Heidenhain's works on the nervous system should be quoted his researches on hypnotism (1880), which are some of the best of physiological investigations of those complex phenomena.

But it is in the vast field of glandular functions that Heidenhain made his greatest discoveries. Most of his experimental results have become classical—the application of the anti-secretory action of atropine (discovered by Keuchel) to distinguish between the vaso-motor nerves of the glands and the true secretory nerves; the determination of the secretory nerve of the parotid (the glosso-pharyngeal); the influence of the nervous system on the pancreatic and bile secretions; the variations of pressure in the excretory passages of the glands; the functional differentiation between the glandular cells of the pyloric region and those at the bottom of the stomach; the formation of ferments in the cells of the pancreas; the function of various elements of the kidneys; absorption in the small intestine; and finally a series of conceptions of deep interest on ferment-producing substances and processes.

Whatever the importance of the above, there are two works which, by reason of their originality and suggestiveness, are superior to all the rest of Heidenhain's results in this direction. It is well known that he was the first to conceive the idea of studying the histological modifications of glands after a prolonged excitation of the secretory nerves. Many valuable ideas on the physiology of glands resulted from his first experiments. Again, during the years 1889-1892, he was brought to consider lymph as a product of the secretion of the cells in the walls of the blood capillaries, and from this all our ideas on the formation and circulation of lymph underwent revision. The theory may be contested, but it has evoked many new facts.

The discovery of lymph-producing substances made in the course of these researches is not the least considerable. A new chapter of physiology was opened. Thus Heidenhain appears to us not only as a skilful and successful experimenter, but also as a clear-sighted intellect.

A view now generally held, that of the specific activity of cellular elements, resulted from Heidenhain's numerous researches on the functions of the glands. He contributed to the abandonment of purely physical explanations of the phenomena of secretion.

Almost all the works of Heidenhain were published in the *Archiv. f. die ges. Physiol.*, a few in the *Archiv. f. die mikros. Anatomie.* A large proportion were collected and published in four volumes at Leipzig from the year 1861 to the year 1868, under the title *Studien des physiol. Institutes zu Breslau.* Every physiologist has read and thought over his fine and complete study of glandular functions, published in Hermann's "Handbuch der Physiologie."

Great activity prevailed in Heidenhain's laboratory. Many well-known physiologists of the day studied under his direction. Amongst these may be mentioned Grützner, Hürthle and Rohmann.

[The above is largely taken from an article by Professor E. Gley in *L'Intermédiaire des Biologistes*, No. 2.]

OSCAR FRIEDRICH VON FRAAS

BORN JANUARY 17, 1824. DIED NOVEMBER 22, 1897

THE veteran director of the Royal Würtemberg Museum of Natural History in Stuttgart, who had been in failing health for some time, died on November 22. Dr von Fraas was a native of Lorch in Würtemberg, and at an early age entered the theological faculty of the University of Tübingen in preparation for the Church. While there he came under the influence of Quenstedt, who inspired him with zeal in the study of geology and palaeontology. In 1847 he proceeded to Paris for a year, where he pursued his studies at the School of Mines under d'Orbigny and Elie de Beaumont. At the conclusion of his university career in 1850 he was ordained, and became a pastor in his native country, devoting his leisure only to his favourite scientific pursuits. In 1854, however, he left the Church and was appointed Conservator of the minerals and fossils in the Würtemberg Museum, an office which he held until a few years ago, when he succeeded Dr von Krauss as Director of the Museum. The researches of Dr von Fraas had reference chiefly to the geology, palaeontology, and prehistoric archaeology of Würtemberg; and in the rich collections of the museum which he did so much to form, he has left an enduring monument and memorial of his labours. But he also carried on important researches in the East, where he was one of the pioneers in working out the geology. He made two journeys through Syria, the first in 1864-65, the second in 1875, and devoted special attention to the Lebanon. He described the results in a valuable little work entitled "Aus dem Orient." He was also much occupied with other researches beyond his special domain, and devoted special attention to the successful cultivation of the vine in Würtemberg.

We regret to learn of the death on October 21, owing to an accident on the lake, of JOHANNES FRENZEL, formerly professor of zoology at Cordoba University in the Argentine Republic, and of late years director of the biological and fishery station on the Müggelsee near Berlin. An account of the work here carried on so successfully by Dr Frenzel, who was only thirty-nine years old, was published in *Natural Science* for March 1897 by Professor Anton Fritsch (vol. x. p. 170).

DR JUSTIN WINSOR, librarian of Harvard University and also an energetic anthropologist and ethnologist, died on October 21, 1897, aged 66. He was a great authority on the early history and cartography of America.

The following deaths are also announced:—Dr HARRISON ALLEN, emeritus professor of comparative anatomy in the University of Pennsylvania, and well-known as a writer on mammals, at Philadelphia on Nov. 14; E. P. FRANZ, nerve-anatomist, and assistant to Professor E. A. Schafer at University College, London; Dr MARK OLIVET, professor of psychiatry in the University of Geneva, and author of several medical and hygienic works, at Genoa on Oct 24, aged 75; JAMES MURRAY, professor of pathology in the Lahore Medical College, on Nov. 8 at Simla, aged 32; ALBERTO PERUGIA, ichthyologist, at Genoa on Sept. 24, aged 51; ALEXANDER SOLON, student at the Brussels University, in Banana at the mouth of the Congo, in Oct., aged 22; Dr FRANKLIN STORY CONANT, student of chaetognathous worms, at Boston, on Sept. 13, in consequence of an illness contracted during the Johns Hopkins University Expedition to Jamaica in the summer of 1897; Dr WILHELM MOERICKE, privat-docent in mineralogy, at Freiburg i. B., on Oct. 8; JAMES WINDOES, a well-known collector of fossils, at Chipping Norton, aged 58; Dr GEORGE H. HORN, the eminent entomologist, one of the secretaries of the Philosophical Society, and formerly corresponding Secretary of the Academy of Natural Sciences, at Philadelphia, on Nov. 25, aged 58; GUSTAV STANDFUSS, lepidopterist, on Oct. 6, aged 82; FRANCISQUE GUILLEBEAU, entomologist, author of the catalogue of the Coelcoptera of Ain, at Plantay, aged 77; Dr G. LICOCOPOLI, assistant professor of botany at the University of Naples; HORACE W. L. BILLINGTON, director of botanical gardens, Royal Niger Coast Protectorate, on Nov. 18, aged 28; KARL MUELLER, director of the Agricultural Experimental Station in Hildesheim, and agricultural chemist, on Oct. 26, aged 50; FRIEDRICH STOHMANN, professor of agricultural physiology and agricultural chemistry in the University of Leipzig, on Nov. 1, aged 65; Dr BERNARDIN A. MARTIN, a zealous student of the flora of the Cevennes, on March 31, at Aumessan (Gard); and Dr ALBRECHT SCHRAUF, the mineralogist of Vienna, aged 59, in the middle of December.

NEWS

THE following appointments are announced :—

Dr J. Joly, to be professor of geology in Trinity College, Dublin ; Dr Wm. G. Smith, to be assistant-lecturer in botany, at the Yorkshire College, Leeds ; Dr J. D. E. Schmeltz, keeper at the Rijks Museum of ethnography in Leiden, to be director in the place of Dr L. Serrurier, resigned ; Dr Otto Fuhrmann, to be professor of zoology at the Neuchatel Academy ; Dr Max Dessoir, to be associate-professor of psychology in Berlin University ; Dr George Kraus, to succeed J. von Sachs as professor of botany at Halle ; Franz Mattouschek, assistant in botany at the German University in Prague, to be professor at the gymnasium in Linz ; Dr A. Ostroumoff, to be professor of zoology in the University of Kasan ; Dr Polumordwinow, to be privat-docent of histology and prosector in the University of Kasan ; Dr Alexis E. Smirnov, to be professor of zoology in the University of Tomsk ; Dr Guido Schneider, to be director of the Biological Institute at Sebastopol ; Clarence L. Herrick, to be president of the University of New Mexico in Albuquerque ; Dr Geo. T. Kemp, to be professor of physiology in the University of Illinois ; Dr John Y. Graham, of Princeton, to be professor of biology in the University of Alabama ; Robert B. Yound, to be assistant-biologist in the Department of Agriculture, at Washington ; A. A. Tylor, to be instructor in biology in the Union College, Schenectady, New York State ; Dr Frederick D. Lambert, to be assistant in biology at Tufts College ; Dr Lafayette B. Mendel, to be assistant-professor of physiological chemistry in Yale University ; T. A. Reakard, to be State geologist of Colorado ; Dr Schmitz-Dumond, of Tarand, to be director of the Agricultural Experimental Station to be established in Pretoria ; Dr Philippi, to be director of the National Museum in Santiago, Chili, in place of his father, resigned.

A CHAIR of geography has been established in Wurzburg University.

THE seventh International Congress of Geography will be held in Berlin in the year 1899.

MR MARTIN FOUNTAIN WOODWARD will succeed Mr E. R. Sykes as Secretary to the Malacological Society.

MR SCHAAFFHAUSEN'S valuable anthropological collection has been left to the Museum of Bonn University.

DR RICHARD SEMON has resigned his office as prosector and professor of anatomy in the University of Jena.

MANY fish were brought alive to the New York Aquarium, but the attempt to transport invertebrates was unsuccessful.

DR SVEN HEDIN, the Swedish explorer of Central Asia, is the recipient of the Danish Geographical Society's gold medal.

PROFESSOR SCHAFER of University College, London, is the recipient of the Baly Medal of the Royal College of Physicians.

THE Senckenbergische Naturforschende Gesellschaft of Frankfurt has purchased the library of the late Professor Carl Vogt.

THE Durham College of Science, Newcastle-on-Tyne, has, on October 21, established a marine biological station at Cullercoats (Northumberland).

THE Hallett Philips collection of Indian implements and antiquities from the Potomac Valley has, says *Science*, become the property of the Smithsonian Institution.

THE University of Lyons is, says *La Feuille des Jeunes Naturalistes*, devoting a sum of 42,000 fr. to the completion of the biological laboratory of Tamaris, near Toulon.

THE general plans of the Zoological Gardens in Bronx Park, New York, were approved without opposition by the New York Park Board at the public hearing on November 22.

PER DUSÉN, the Swedish engineer and bryologist, has returned from Tierra del Fuego and Patagonia, where he has been engaged in scientific research since September 1895.

THE John Lucas Walker Studentship for Research in Pathology at Cambridge has been awarded to Mr J. W. Stephens, B.A., of Caius College, and Dr Hamilton Wright of Montreal gains the Exhibition of £50.

THE chair of psychiatry will shortly be vacant in the University of Zurich, owing to the resignation of Dr August Forel, who is also well known as a student of the habits of insects, especially of ants.

MR W. P. PYCRAFT has left Oxford, and has been appointed temporary assistant in ornithology in the British Museum (Natural History). He will devote his attention specially to the arrangement of the collection of skeletons of birds.

THE Danish Government is organising for 1898 a geographical and ethnographical expedition to the Pamirs, where two years will be spent in research under the direction of Lieut. Olufsen. Two specialists will accompany the expedition.

MR J. B. HATCHER, we learn from *Science*, has returned to Patagonia to make further investigations in geology and palaeontology. He hopes to add natural history and ethnological collections to the palaeontological collections already made in his recent successful expedition to that country.

BY the will of the late Sir William Alexander Mackinnon the University of Edinburgh receives £2000 upon trust to found prizes or scholarships to be named the "Mackinnon Scholarships," for proficiency in geology, natural history, and modern languages. Bequests are also made to the Royal Society of London.

ON July 29th a Museum of Natural History and Anthropology was opened at Wernigerode, in Brunswick. The museum, says *Science*, comprises the following collections:—Mineralogical, of Count Heinrich Ernst, Dr Doring and Councillor Jasche; zoological, of Dr Muller; anthropological, of Dr Augustin and Dr Friedrich; and the Herbarium of Dr Sporleder.

DR JOSEPH GEDGE, who died in 1870 while with Sir Samuel Baker at Khartoum, left a sum of £1000 to the University of Cambridge for the establishment of a biennial prize for original physiological research, open only to graduates of more than five and less than seven years' standing. Owing to a long delay this bequest has only lately been received by the University.

THE Boston *Transcript*, as quoted by *Science*, states that owing to the disastrous effects of the yellow fever epidemic in the South, the American Public Health Association appointed a committee of seven to urge on President M'Kinley the absolute necessity of a special commission of experts, to be appointed by Congress, to make a thorough study in Havana of the cause and prevention of the disease.

AT the Annual Meeting of the Scarborough Field Naturalists' Society last month, a most gratifying report of the past year's local scientific work was read. The Society is fortunate in possessing at least one member to undertake systematic observations in nearly every group of the local fauna and flora, and records are carefully kept.

A SHOCK of earthquake was felt at Kudat in British North Borneo, as also a slight tremor at several places along the coast on September 21. About the same time a new island was thrown up from the sea between Lambeidan and Mempakul, opposite Labuan, and about 50 yards from the mainland. The island emits inflammable gas, with a strong smell of petroleum, and appears to be increasing in size.

THE Regents of the University of the State of New York have, says *Science*, authorised relief maps of the Adirondacks and another of Manhattan Island, showing its physical features before alteration by civilisation. They have also issued a notice to University institutions that duplicates of these maps will be furnished to the schools for cash or as a part of their apportionment, at a trifling cost.

WE understand that Colonel Bernier will leave England on March 1, in Mr Harmsworth's ship, the *Windward*, in an attempt to reach the North Pole. According to his present programme, he will first proceed to the north coast of Siberia, and thence will journey towards the Pole over the ice. Assuming that he will move along at the rate of six miles a day, he estimates that he will reach it in 120 days. He will then return to Spitzbergen.

THE Pirogof Museum of Surgery and Anatomy, which was opened in St Petersburg at the beginning of December, will not only serve as a museum but also as a place of meeting for all the medical societies of that city. The cost of the building deducted, there remains a margin for endowment from the £12,000 devoted to it, half of which was bequeathed by Mrs Musin-Pushkin, the remainder being in the form of subscriptions.

THE Biological Department of New York University selected the Bermudas as the ground for its first expedition, under the direction of Dr C. L. Bristol, who has given an account in *Science*. Harrington House, about six miles from Hamilton, was made headquarters, and a laboratory installed near Castle Harbour. It is proposed to establish a station in these islands, and to make them the centre for further exploration in the West Indies. Dr W. M. Rankin of Princeton will report later on the collections made.

WASHINGTON will henceforth be the headquarters from which will be carried on the tropical work of the Sub-tropical Laboratory of Eustis, Florida, under the direction of the Division of Vegetable Physiology and Pathology of the Department of Agriculture. Work can be done and experiments made at Miami, Florida, where a small garden tract and a laboratory have been secured. Experiments have been made in hybridising the orange and other citrous fruits, and the resulting hybrids will be cultivated on this land. It is also proposed to introduce and test varieties of tropical plants which can be successfully grown in the tropical and sub-tropical portions of the United States.

THE new regulations for the Intermediate Science Examination of London University (which are destined to come into force in 1899) allow candidates to offer any three of the following four subjects :—(*a*) Mathematics, (*b*) Chemistry, (*c*) Physics, (*d*) Botany *plus* Zoology. A meeting of teachers of botany and zoology engaged in training students for the examinations of London University, thinking this arrangement prejudicial to the interests of the biological sciences, thus held a meeting on November 30, 1897, to discuss the subject. The meeting was of opinion that (1) zoology and botany being separate branches of knowledge, taught in separate laboratories by separate teachers, and tested by separate papers in the examination, should be treated as independent examination subjects ; (2) that the necessary result of the new regulations would be, that students, on finding that to offer botany and zoology would entail upon them the study of four subjects instead of three, would be unfairly diverted from these two sciences to the three which entailed less examination work. The standard of knowledge required for the examination in botany and zoology has been in both cases admittedly as high as that demanded in each of the other subjects, and will doubtless be maintained by the examiners. A memorial embodying these views has thus been presented to the University Senate.

We have frequently noticed the admirable publications of the Institute of Jamaica, and we have now received its Annual Report for the year ending March 31, 1897, which is a record of great and successful activity. During the year changes have been made in the arrangement of the Museum, and numerous important additions are recorded. In the Department of Zoology many birds' nests have been mounted and displayed with the specimens of birds. The latter have been re-labelled. All the fishes in alcohol and formalin have been mounted on glass, re-labelled, and several additions have been made. The Decapod Crustacea have been named and arranged on tablets. Many of the Echinodermata, Actinaria, and Corals have been provisionally named and displayed. A four to six per cent. solution of formalin has been mostly employed as a preservative fluid and found to work very satisfactorily, though the colours of the objects are not retained for long in specimens exposed to strong light. For delicate objects such as jelly-fishes and sea-anemones, it is found to be extremely serviceable, perserving perfectly the natural form and histology. In the Department of Botany additions have been made to the Herbarium by the Hon. Wm. Fawcett, Director of Public Gardens and Plantations. In the Department of Geology an extensive students' collection of fossils has been purchased and arranged, in two cases with descriptive labels. The series illustrates by actual fossils and casts the typical forms of life met with throughout the geological formations. The mineralogical collection has been grouped so as to illustrate, in an elementary manner, the different branches of interest in the subject. Mr F. C. Nicholas has presented an elementary series of fossils, with illustrative recent shells, and these have been biologically arranged. Some of the specimens from the Survey Geological collections have been lent to Professor R. T. Hill of the American Geological Survey, who is at present engaged upon a study of the Geology of Jamaica, and has already carried out two explorations. Various collecting expeditions have also been made by the Curator, Mr J. E. Duerden, with very satisfactory results.

Our American contemporary *Puck* prints the following jest regarding the numerous attempts to reach the North Pole :—

The North Pole Calendar

(*For the next Nine Years*).

1898.—Expedition of Haans Haansen, from Haamerfest, Norway, to find the North Pole.

1899.—Expedition of Torm Tormpsen to find Haansen.

1900.—Expedition of Jhim Jhornsen to find Tormpsen.

1901.—Expedition of Jjaik Jjaksen to find Jhornsen.

1902.—Return of Jjaksen with relics of Jhornsen. Marble monument erected to the memory of Jhornsen.

1903.—Return of Jhornsen with relics of Tormpsen. Brick monument erected to Tormpsen.

1904.—Return of Tormpsen with relics of Haansen. Wooden monument erected to Haansen.

1905.—Return of Haansen, dead-tired. North Pole still standing.

1906.—North Pole discovered by an American. Auction sale of monuments at Haamerfest.

CORRESPONDENCE

WASPS

MR O. H. LATTER (*Natural Science*, vol. x, p. 368.9) invites information on the subject of the effect of climatic influences on wasps, and as the past summer was certainly a remarkable one in this respect, perhaps the following observations may be worth publishing.

My experience here agrees with Mr Latter's to some extent, but not entirely. Queens were fairly abundant in May, but from the time the last queen disappeared till the beginning of September I don't think I saw two workers except from a single nest of *Vespa [illegible]*, which was taken in a hedge. But in my diary on September 4, I have the following entry: "Wasps, which have been extraordinarily scarce this year, have suddenly become very abundant in the house, though I seldom see one outside." This was during a spell of unusually cold weather for early September.

As to the climatic causes which produced the summer scarcity, it is not very obvious how the wet of the winter of 1896-7 could have affected the hibernating queens, as Mr Latter suggests, because he says they were abundant in spring; moreover, they generally hibernate in places where they are safe from rain. It is generally supposed that a cold spring is destructive to the queens, with the natural consequence of a scarcity of workers in the summer. But Mr R. Newstead, F.E.S., who has paid much attention to wasps, writes me that he finds in his notes that "in 1889 wasps were exceptionally abundant in Denbighshire and Cheshire." It will be seen from the annexed table of temperature (which are from corrected instruments in a Stevenson screen in my garden) and rainfall, that the spring of 1889 was on the whole rather colder and very much wetter than that of 1897, April in particular being the wettest I have recorded in eighteen years.

	MEAN TEMPERATURE			LOWEST IN SHADE			RAINFALL		
	Mar. Degs.	April. Degs.	May. Degs.	Mar. Degs.	April. Degs.	May. Degs.	Mar. In.	April. In.	May. In.
1889	41.[illegible]	[illegible]	[illegible]1	[illegible]6	[illegible]2	[illegible]	1.90	1.[illegible]9	2.91
189[illegible]	4[illegible]	51.[illegible]	[illegible]6.0	31.[illegible]	30.0	[illegible]	0.[illegible]6	0.62	1.38
1897	45.0	45.0	50.1	[illegible]	31.8	[illegible]6.0	[illegible]2	2.[illegible]3	0.99
Average of 1[illegible] years	41.1	46.1	[illegible]				2.06	1.68	2.05

It would seem therefore that climatic influences do not altogether account for the abundance or scarcity of wasps, still less for their late appearance this year. A satisfactory explanation is still wanted.

ALFRED O. WALKER.

Nant-y-Glyn, Colwyn Bay.

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.

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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.

NATURAL SCIENCE

A MONTHLY REVIEW OF SCIENTIFIC PROGRESS

FEBRUARY 1898

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NATURAL SCIENCE

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NOTES AND COMMENTS

The Deep-sea Shark, *Chlamydoselache*

[illegible] lately we have alluded to the remarkably wide [illegible] in distribution of the fishes of the deep sea. Still [illegible] illustration of the fact has just been published [illegible] Collett of Christiania, who has received the largest [illegible] of the eel-shaped frilled shark, *Chlamydoselache*, from a depth of about 150 fathoms in the Varanger Fjord, northern Norway (*Universitetets Festskrift*, 1897). When this remarkable shark was first discovered by Dr Samuel Garman in 1884 among some fishes from the seas off Japan, it was at once recognised by him as a very ancient form of life which must have survived by taking refuge in the deep waters. The discovery thus led to a long and animated discussion as to its nearest extinct representatives, and so far as the teeth were concerned, nothing more similar could be found than the well known fossils from the Carboniferous rocks named *Cladodus* and *Diplodus*. Shortly afterwards three more specimens of the same strange animal were received by the British Museum from Japan, and these enabled Dr Günther to give some further account of its anatomy in his well-known 'Challenger' volume on deep-sea fishes in 1887. The general result was to prove a close relationship between *Chlamydoselache* and the more familiar existing primitive sharks termed Notidanidae. Until 1890, however, the genus was known only from the neighbourhood of Japan, and in that year zoologists welcomed the interesting news of the discovery of a young specimen by the Prince of Monaco in the seas off Madeira. Finally, in 1896 came the new and comparatively gigantic individual now described by Prof. Collett; and this is particularly interesting as having been obtained in a sea so far north as latitude 69° 45′. Fossil teeth, it may be added, prove that the shark lived in the Mediterranean area in Pliocene times (*Chlamydoselache lawleyi* of J. W. Davis from Tuscany), but hitherto it has not been found surviving in that region.

The specimen of *Chlamydoselache* from northern Norway is much larger than any of those from Japan, being indeed nearly two metres in length. But it is a very rare circumstance to be able to capture so large a fish at so great a depth; and as it scarcely differs from the Japanese specimen except in size, Prof. Collett concludes that it is merely a more mature individual of the same species, *C. anguineus*. It is a female specimen, with immature eggs in course of development in the oviduct, and thus apparently full grown: but that is, nevertheless, no reason why it should not attain still larger dimensions.

A Veritable Sea-serpent

Prof. Collett thinks, indeed, that large individuals of *Chlamydoselache* may have given rise to some of the stories of a sea-serpent. At any rate, an unusually large specimen of this shark would serve very well for a so-called sea-serpent which was captured and unfortunately, thrown away by an American fishing boat on the coast of Maine in 1880 (*Proc. U.S. Fish Commission*, vol. iii. 1883, p. 407). The strange animal in question is described as having been a long, eel-like fish, about 25 feet in length, with a flattened head, round body, and roughened skin like that of a shark. It had one dorsal fin, and several gill-clefts on each side of its head. Unfortunately it was thrown away as of no commercial value to the fishermen, and a subsequent search for the dead monster proved in vain.

The Mechanism of Living Protoplasm

So much progress has been made during recent years in explaining some of the simpler phenomena of life or vitality on mechanical principles, that biologists are prepared without surprise for an early further development of the subject. Notwithstanding its deep interest, there is thus nothing really startling in a communication we have received from Prof. A. L. Herrera, the ingenious biologist of the Mexican National Museum. The professor has lately been making experiments with the object of explaining on mechanical principles some of the phenomena observed by Dr Fol in the fertilisation of the eggs of Echinoderms. He takes two liquids of different density, such as olive oil and water or yolk of egg, pouring out the water upon a plate and allowing a thin layer of oil to float on its surface. On the centre of this he drops a little yolk of egg, which easily glides upon the oil. On the approach of any pointed body, the yolk rises into a bubble quite similar to the cone of attraction which Fol has observed before the penetration of the spermatozoid into the vitellus. Sometimes it is possible to see the emission of pseudopodia, which also fall back into the yolk as soon as the attractive force is removed. With albumen the phenomenon is still more remarkable, and if a coin, for example, is brought near it, the drop is slowly distorted, attracted by the metal, and becomes pear-shaped. Again, if instead of a pointed body an artificial spermatozoid approaches the yolk to a distance of four or five millimetres, as small as possible and floating on a cork, it penetrates slowly, and eventually reaches the centre of the drop. The analogy with the natural phenomenon is said to be complete. The oil or less dense liquid represents the albumen, while the drop of yolk serves for the vitelline sphere.

The Development of a Crane-fly

Prof. L. C. Miall, who has so often insisted on the study of life-histories as the most desirable field for the work of entomologists, sets an excellent example by the publication (in collaboration with Mr R. Shelford) of a paper on the larval and pupal structure of a crane-fly, *Phalacrocera replicata* (*Trans. Ent. Soc.*, 1897, pp. 343-361, pls. viii.-xi.). This larva is destitute of limbs, but it bears on every segment many long and slender transparent processes. These become covered with algae and other organisms, and the larva is consequently very hard to distinguish amid the aquatic mosses whereon it lives and feeds. The head is retractile within the prothorax; it is without eyes, but provided with a pair of small slender antennae. The last body-segment bears a pair of large curved chitinous anal hooks, used for grappling and locomotion; also, on its dorsal surface, a pair of spiracles. The larva was observed to come to the surface in the night to breathe by means of these, but the long body-processes have probably a respiratory function, and the creature can live submerged for several days. The most noteworthy feature in the internal structure of the larva is a pair of cylindrical cellular cords, of epidermal origin, attached to the body-wall at the last segment between the spiracles, passing into the heart by a pair of openings in the hinder wall, and lying freely in the cavity of that organ, which they traverse from end to end. The homologies and function of these cellular cords, which seem almost unique in the animal kingdom, are left in doubt by the authors, but the suggestion is made that they serve as a source of reserve food material to the blood during the rapid changes of the pupal stage, during which they break up and disappear. The ovary is peculiar in structure, and unlike that of most other insects. Instead of a few long ovarian tubes opening together into the oviduct, *Phalacrocera* possesses many short follicles opening along the tube. This arrangement seems adapted for the simultaneous discharge of the eggs. The pupa is comparatively active in its habits, grasping aquatic weeds with its dorsal hooks, so as to keep the prothoracic respiratory tubes at the surface of the water, or floating freely. Unlike the larva, the pupa is killed by submergence for a few hours.

'The Pioneers of Natural Science'

In his presidential address to the ninth annual meeting of the Association of Economic Entomologists, Mr F. M. Webster discusses the value of systematic work in natural science, comparing the species-makers to pioneers who make possible the progress of the army of biologists who follow. We fear there is some truth in his suggestion that the pioneers are not all as well trained as they should be, and that they waste much valuable time and energy in disputing among themselves, "one faction having no sooner constructed a portion than another faction tears it down and builds after its own ideas." Proceeding to discuss the prospects of economic entomology in America, Mr Webster utters a needed protest against scientific appointments being bestowed according to the party politics of the applicant.

Artificial Ants' Nests

Mr Charles Janet, whose work on the social Hymenoptera has been often mentioned in *Natural Science*, has published (*Mém. Soc. Zool., France*, x., 1897, pp. 302-323, pl. x.) full descriptions, with figures, of the artificial nests which he has used for his observations on the habits of ant colonies. He obtained the best results with blocks of plaster, provided with suitable hollows covered with a sheet of glass. No earth is needed with this form of nest, and a proper degree of moisture is ensured by pouring water into a tube sunk in one side of the plaster block.

Catching Plankton

There is an interesting short paper in *Science* (3rd December 1897), by Mr Charles A. Kofoid of the Illinois Biological Station, "on some important sources of error in the Plankton method." It is of interest mainly to those who examine the organisms of drinking water, from the suggestions as to improved methods of filtering out the minutest organisms and all of them. Such methods are hardly applicable to tow-nettings on a large scale such as interest most naturalists—they are valuable to the laboratory man rather than to the collector; and though the naturalist ought to be both, he is limited to being only one at a time. The point of most interest is the open declaration against the assumption that supports the Hensen method—viz., that No. 20 silk stops almost all organisms. If one estimates the bulk of plankton on this basis there can only be error, as the author points out. One might go further than he does and insist that the filtering power of such silk varies very greatly with its condition. For example, a new tow-net of No. 20 miller's silk (the best procurable) stops comparatively few organisms. When it has been used for a time it improves vastly as the meshes get clogged with diatoms, Peridiniae, etc. If it be carefully washed the net will then be at its best, and after reaching its optimum will soon decline—wear out. This may be tested most easily by pumping water into a net with another outside, or even with two outside. Organisms, and these not the smallest only, will be found in the outer bag even at the best of times. Similarly one may test a new net against an older one by towing them at the same depths. Estimates of plankton therefore by the Hensen method are bound to be misleading, and, however carefully the error may be calculated for this or that size of mesh, it is impossible to give the error exactly, since the silk itself varies with use.

Fermentation and Ferment Action

In the recent issue of the *Annals of Botany* (Dec. 1897) Prof. Reynolds Green gives an account of some experiments with yeast. They were made with the object of testing the statement of a German observer, Dr Buchner, who claims to have extracted from yeast a liquid having the power of setting up fermentation in sugar solution. The importance of such a statement will be evident to every student of botany, who, if properly brought up, realises the great difference between the alcoholic fermentation induced by the living yeast plant, and the ferment action set up by a series of ferments or *enzymes*, which are merely formed substances, and can be extracted from the plant or animal tissue, and made to perform their peculiar functions quite apart from the living organism. Such are the diastatic, peptic, and tryptic ferments which are universally present where, in the first instance, starch or some forms of insoluble carbohydrate food, and in the other two, proteid material has to be brought into a soluble form like sugar, peptone or amide, before it can be assimilated. For details of the experiments, we refer the student to the short paper in the *Annals*. He will, however, be relieved to hear that, for the present, Pasteur's theory of the vital fermentation in yeast must still be retained, since, at any rate in the case of English yeasts, Prof. Green has found it impossible to extract an active ferment. Death of the plant still means loss of power to excite fermentation.

In this same number, Prof. Vines returns to the investigation of the excretion found in the pitchers of the pitcher-plant (*Nepenthes*). Since Sir Joseph Hooker first showed, in 1874, that cubes of white of egg, fragments of meat, and pieces of fibrin are eaten away or completely dissolved by immersion in the pitcher, several observers have taken up the subject, and two opposite views have been held. In 1877 Prof. Vines showed an important analogy between the pitchers and the gastric mucous membrane of animals, by obtaining from the secreting areas of the pitcher wall a glycerine extract which had a distinct digestive action on fibrin, and indicating its origin from a zymogen formed in the cells and subsequently decomposed by the action of acids. More recently a French and a Russian observer have denied the presence of an active ferment, and declared that the disappearance of proteid matter when placed in an open pitcher is merely a putrefaction set up by bacteria which have been introduced. Goebel, however, criticised adversely these conclusions, and now Prof. Vines brings additional evidence which confirms his original conclusion, that there is set up in the pitcher an active process of digestion by a proteolytic ferment formed in the gland-cells of the walls. One of his most conclusive experiments shows that digestion is set up by a glycerine extract of the pitcher in a solution containing 1 per cent. of prussic acid, and he suggests, with some reason, that the *onus probandi* may now be considered to rest on the exponents of the other theory, who must bring forward a bacterium capable of digesting fibrin in such a solution, and of retaining its digestive activity when kept for several weeks in pure glycerin.

THE JAMAICA BOTANIC GARDENS

THE *Botanical Gazette* for last November contains an interesting account of the Public Gardens and Plantations of Jamaica by the Director, the Hon. William Fawcett. The paper was prepared for, but owing to a delay not read before, the recent Toronto meeting of the Botanical Society of America. The aboriginal name of the island was, we are told, Xaymaca, denoting "a land covered with wood, and watered by shaded rivulets." The differences in elevation from sea level to the Peak (7400 feet), the varying exposures to sunlight, the abundance or want of rain, and the different geological formations, supply conditions of so diversified a character as to render the cultivation of economic plants from all parts of the world an easier task than in most other places. The first botanic garden was founded about 150 years ago by a private individual, a Mr East, on his own property, about nine miles from Kingston. In 1775 a government garden was formed adjoining Mr East's, and Dr Thomas Clarke came out as island botanist. He introduced the tea-plant, camphor, litchi, and 'sago-palm' (*Cycas circinalis*), and a few years later the clove-tree. In 1779 another garden was started at Bath. Three years later many valuable economic plants were added to Mr East's garden, being part of the spoil from a captured French ship bound from Mauritius for Haiti. Finally, on Mr East's death, his garden was purchased by the Assembly. Among the introductions of the next few years we notice the bread fruit, brought by Captain Bligh from Otaheite. Cinnamon and mango were also thriving. In the general depression experienced at the beginning of the century the gardens were much neglected, the original one being sold in 1810. In 1825 Dr James MacFadyen was appointed island botanist. He commenced a *Flora of Jamaica*, of which only the first and part of the second volume appeared. He was succeeded in 1828 by Thomas Higson, and in 1829 the Bath garden was increased. In 1846 it passed under the care of Nathaniel Wilson, who had been trained at Kew and Kensington, and is described as a most capable man. He introduced a large number of plants, especially fibre-yielding. In his report for 1861 he notes the receipt from Sir W. Hooker of seeds of several species of *Cinchona*, and the fact that several hundred young plants were now ready for planting out. In 1862-6 a new garden was started at Castleton, and in 1868 Cinchona plantations were formed on the slopes of the Blue Mountains, at 4000 to 6000 feet above sea-level, under Wilson's successor, Robert Thomson. A plantation of Assam tea was also formed. During the next ten years considerable advance was made under Mr Thomson's *régime*. A great number of valuable plants were introduced from Kew and all parts of the world, and among other items we note the arrival in 1874 of a skilled European gardener at Cinchona, where all kinds of 'English' vegetables are now grown. At the present time Mr Fawcett is responsible for no less than six separate gardens, ornamental and experimental, representing very different conditions of climate. Some idea of their value and the amount of work done may be gathered from the following account of the Hope garden, which is situated in the dry Liguanea plain, about six miles from Kingston: "From being at first a small nursery and an experimental ground for sugar cane, it has now developed into a large garden with 6 acres of lawns, 3½ acres of ornamental borders, also ferneries and orchid houses; collections of roses, crotons, and palms; plantations covering 7½ acres of sugar cane, Arabian and Liberian coffee, oranges, ginger, tobacco, ramie, and 5 or 6 acres of teak. Two and a half acres are given up to the nurseries, which contain about 70,000 plants, such as cocoa, nutmeg, clove, orange, vanilla, cinnamon, Liberian coffee, rubber plants, &c. It is the distributing centre, and on an average 40,000 plants are sent out all over the island each year."

Natural History in the School-room

There has come into our hands a copy of *The Kentish Mercury*, which contains an article on a Voluntary School at Lower Sydenham. The writer points out the good work being done among the lads of that school by the headmaster, Mr G. E. Dibley, who has laboured unceasingly for the past fourteen years in combining general education with a grounding in the Natural Sciences. We can endorse all the writer says as to the value of including Science in the curriculum, as inducing a cultivated memory, habits of close and accurate observation, ability to give a definite and more complete description of a given object, ability to handle delicate and fragile things without damage, dexterity in manipulation, capacity to form correct estimate of dimension, and general interest in life and matters with which the eye comes in contact. Nothing but good can come of such instruction, and school loses half its dryness if object-lessons are combined with general learning.

We have heard before of Mr Dibley, his collections, and his school, and have admired his efforts to gain an insight into the geographical and stratigraphical distribution of the fossils of the Chalk—efforts made in the brief leisure of a schoolmaster's busy life. There are many rewards for such a worker, but the chiefest of all are those proud moments when an 'old boy' is found still patiently studying the workings of Nature, to which he was first introduced in a ramble with his old master.

A Great Work on Early Cartography

Baron A. E. Nordenskiöld, besides being a practical geographer and explorer, takes an antiquarian interest in the beginnings and development of geography. His researches in this direction, based largely on material collected for his own library, have long been known, especially the fine Facsimile Atlas to the early history of Cartography, published eight years ago, and reproducing many of the older printed maps. At the close of last year there was published by Mr J. E. Bergsjö, 6B Nybrogatan, Stockholm, a complementary work from the pen of Baron Nordenskiöld, entitled "Periplus; An Essay on the Early History of Charts and Sailing-directions." This was issued in both a Swedish and an English edition simultaneously. The volume, which is in royal folio, contains 212 pages of richly illustrated text and sixty double plates, reproducing by phototype or photolithography, portolanos, manuscript charts, and other maps. The price is £10.

The work is no mere detailed description of ancient maps, in dry bibliographic style, but contains many a chapter of wider interest, and numerous paragraphs full of suggestion for the naturalist. Among such we may mention the history of the invention of the compass; the discussion as to the first colonisation of Greenland by Norsemen, in which Prof. Nordenskiold's practical experience stands him in good stead; the strange, half mythical, and romantic sagas of the early discoveries along the coast of Africa; the vivid sketch of the rise and fall of Portuguese dominion in the East; the wonderful adventures of Marco Polo, the man who always reckoned in millions. One side of this study may interest the geologist, for many of these old charts and sailing-directions afford evidence of changes in the relations of sea and land that have taken place during the historic period. Naturally the outlines of the maps are scarcely to be regarded as absolutely accurate, but much can be learned from the sailing-directions as to the former existence of channels, of bars, or of rocks, in places where such are no longer found. This is especially the case with the valuable chapter on "Sailing-directions for the Northern Seas," contributed by Dr E. W. Dahlgren, the learned librarian of the Swedish Academy of Sciences.

It is strange how little the English figure in this work. Their chief effect on the early history of navigation seems to have been, through their acts of piracy, to bring about subsequent diplomacy, and so pave the way for an international maritime code. This need not prejudice English readers against the book, which, though too expensive for most private pockets, will doubtless be demanded for the larger libraries. It forms a worthy companion to the fine Facsimile Atlas, and, though some of its views challenge criticism, we are sure that all interested in geography and navigation will give the veteran explorer hearty congratulations on the completion of this magnificent volume.

The Geology of Franz Josef Land

One of the most interesting and valuable results of the Jackson-Harmsworth Expedition to Franz Josef Land is the collection of rocks and fossils reported upon by Messrs E. T. Newton and J. J. H. Teall in the last number of the *Quarterly Journal* of the Geological Society (vol. liii. pp. 477-518, pls. xxxvii.-xli). All the marine fossils, including some characteristic Ammonites, prove the sedimentary deposits examined to be of Upper Jurassic age; and the marine strata are intimately associated with a series of shales and sandstones containing plant-remains, beds of lignite, and other evidences of littoral or estuarine conditions. The soft Jurassic sediments seem to have been soon covered up by great flows of basaltic lava, and thus preserved from destruction; and Messrs Newton and Teall allude to the curious fact, that rocks of approximately the same age have been preserved in exactly the same way in districts so far removed from Franz Josef Land as the north-west of Scotland and Abyssinia.

The present configuration of the archipelago of Franz Josef Land conclusively proves that it is formed of the fragments of an old plateau. The land frequently terminates in abrupt cliffs, capped with sheets of basalt which must have extended far beyond their present limits. Moreover, when one compares the topography of this district with that of the Faeroe Islands and the west of Scotland, one is much inclined to believe that these now well-separated land-areas were once continuous, and the northernmost part of the Atlantic is of comparatively recent origin, as suggested by Professor Suess in *Natural Science* four years ago (vol. ii. 1893, p. 185). In any case, it is clear that the various islands of Franz Josef Land were united at the time of the volcanic disturbances and have subsequently been separated. The numerous raised beaches on the coasts also imply comparatively recent changes of level, as in many other parts of the extreme north.

The Structure of Gold Nuggets

The last number of the *Journal* of the Royal Society of New South Wales (1897, pp. 70-79, pls. i.-xvi.) contains a beautifully illustrated paper by Prof. A. Liversidge, on the crystalline structure of gold and platinum nuggets and gold ingots. One explanation of the origin of nuggets, which has received wide acceptance, is that they have been formed *in situ* in the gravels and alluvial deposits in which they are found; that starting with a nucleus they were gradually increased in size by the successive deposits of gold from solution, *i.e.*, that they were built up of superimposed coatings analogous in structure to an onion. This theory is now exploded. Dr Liversidge's researches prove that the nuggets have a well-marked internal crystalline structure (reproduced in an exquisite series of photographs); and this structure is also observable in gold ingots which have been fused. The nuggets are shown to commonly enclose foreign substances, such as quartz and clay; and Dr Liversidge is inclined to think that the gold was originally deposited from solution in veins or pockets in the solid rock. During denudation the nuggets would thus find their way into the gravels and alluvium when already completely formed.

Fatigue in Reading

The *Psychological Review* contains in its number of September 1896 an article, left too long unnoticed, "On the Conditions of Fatigue in Reading," which is of some practical importance, as well as of theoretical interest. The authors, Messrs Harold Griffing and Shepherd J. Franz of Columbia, following the idea suggested by Prof. Cattell in a well-known paper, and supplementing his work, show how facility of reading is affected by size and quality of type, by 'leading,' by the intensity and quality of the illumination, and by the quality of the paper. The result to which they come is that "the size of the type is the all-important condition of visual fatigue. No type less than 1·5 mm. in height (eleventh point) should ever be used, the fatigue increasing rapidly even before the size becomes as small as this." The intensity of illumination is "of little consequence within the limits of daylight in well-lighted rooms. Very few intensities less than 3 to 10 candle-metres (a candle-metre being the light of a standard candle at a perpendicular distance of one metre) are sources of even greater fatigue than small type, and 100 candle-metres may be considered a type limit." The experiments on the relative legibility of different kinds of type were carried out by different methods, the results of which agree fairly well—by determining the times of reading certain passages, by finding the percentage of words which could be seen in certain phrases when cards containing them are exposed for a given time, by determining (through the falling chronometer) the time of exposure necessary for reading certain words in different type, or the amount of illumination necessary to see letters of different sizes. The experiments are of a careful character. But it would have been well in determining the effect of these various conditions to try a parallel series with nonsense-words instead of real ones, so as to eliminate the element of familiarity, and the various accidental elements arising from special association, though this has been done in part by taking a considerable number of observers.

Publication or Manuscript?

How many copies of a printed book need be issued to constitute publication? This question seems to be raised as a side issue in a paper by Mr Davies Sherborn on Thomas Martyn's "Psyche," in the January number of the *Annals and Magazine of Natural History.* Mr Sherborn states that only ten copies of the book were issued; and the names of the species were, with six exceptions, written in ink either below the figures themselves or on fly leaves. The fact that the names were not printed is sufficient for Mr Sherborn to stamp them as manuscript, despite the fact that they have been used by entomologists. We wonder what entomologists will say. One interesting fact in connection with the matter is that the author of this paper had actually three out of the ten copies, side by side for comparison, and has been able to trace five out of the original ten. Mr Sherborn does not mention the copy of Part I. in the Hope collection at Oxford, but that evidently falls under his 'specimen' copies, of which it is quite likely others may turn up.

Now let us suppose this to be a printed book. What happens? Four entire copies out of the original ten are in England, one is in Holland, the rest are unknown. What possible chance has an American or an Australian of seeing such a book? Without seeing it his work must be imperfect. We offer no decision of the difficulty ourselves, but think the point sufficiently interesting to call attention to it.

Natural Science in Lincolnshire

The naturalists of Lincolnshire are to be congratulated on having recently established a central society in their county-town in close association with similar societies throughout the county. It has long been felt that the scattered energies of numerous isolated workers might be directed into more profitable channels by combination and organised co-operation. The "Lincolnshire Science Society" has thus been founded and the programme for the first session is now before us. This interesting little card displays a new feature, in that it not only enumerates the subjects for the general meetings, but also proposes a definite task which the members of each of the six sections of the society are expected to perform. The geologists are to limit their attention during the forthcoming year to the glacial deposits; the botanists are to collect and mount algae and fungi, and make a list of the flowering plants, with localities; the entomologists are expected to pay special attention to the butterflies and moths; and so on. Some of the best literature is also prescribed, and there are competent advisers to whom to apply for help. It is a very interesting experiment which we shall watch with interest.

The objects of the new society, however, are still wider. Lincoln is one of the few county-towns in England without a museum, and the naturalists of the county propose to remedy this defect. They will not only advocate an adequate representation of the geology, zoology, and botany of the county, but also hope to form an archaeological department where many local treasures, now scattered and in private hands, can be safely kept and made accessible. We wish this important scheme all success. It is quite possible to combine the functions of a store-house of local collections with those of an educational institution. The cases can be arranged and illustrated in such a way as to interest both the beginner and the scientific expert. It therefore behoves all who are interested in education and intellectual progress in Lincoln, whether devoted to science or not, to use their best endeavours in furthering the work of the new society.

The Genera of Rodents

Dr T. S. Palmer has brought together, in the *Proceedings* of the Biological Society of Washington (vol. xi. pp. 241-270, 1897), a list of the generic and family names of rodents, from which it will be seen that more than 600 names have been coined for this group of mammals between 1758 and 1897. This list is important and valuable in many respects, but chiefly in showing the amazing carelessness of systematic zoologists, and the difficulty of accepting any nomenclature that is not based on absolute priority. And many of these 600 names are due to the vanity of their authors, who seem to regard the sole aim of life to be summed up in the imperfect description of supposed new forms, the validity of which is a matter of perfect indifference, so long as they have to be quoted with the author's name attached. This list includes recent and fossil genera, is arranged systematically, and is provided with an alphabetical index. Dr Palmer has helped zoological science by compiling and publishing it.

A Devonian Fish-spine

In the last number of the *Journal* of the Cincinnati Society of Natural History (vol. xix., pp. 95-98, pl. vi.) Dr Josua Lindahl describes a new species of fish-spine, *Heteracanthus uddeni*, from the Devonian rocks of Buffalo, Iowa. It is the finest specimen of its genus hitherto discovered, and seems to belong to the same group of shark-like fishes as *Psammosteus*, from the European Devonian, and *Oracanthus*, from the British and American Lower Carboniferous. Dr Lindahl, it may be remembered, succeeded A. H. Worthen as State-geologist of Illinois, but was turned out in consequence of political changes, to the great loss of the State. We are glad to find that he now has a position as Director of the Museum of the Cincinnati Society, 312 Broadway, Cincinnati, O.

I

The Classification of the Day Butterflies

(*Concluded from p. 26.*)

I RECOGNISED in my studies from the first, the fact that the veins or nervures may be approximately divided into two groups: the movable and the stationary rods sustaining the wing membrane. The veins of the first class constantly shift their position and occasionally vanish. The veins of the second class form part of the more durable pattern of the wing, and remain in the same relative position through a single or through many groups and divisions. The cubitus, and its two branches, is a stationary vein, and this quality belongs also to the radius except so far as the branches are concerned. The infracubital veins have in part only a comparative stability. The median series belongs now, though probably it did not always, to the movable veins. A summary of the veins and branches of the wing may be here given for convenience of reference.

FOREWINGS

I. *Costal* nervure or vein, seldom present.

II. *Subcostal* nervure, simple and constant.

III. *Radius*, normally five-branched, becoming by reduction three to four branched.

IV. *Media* obsolete from the cross vein to the base in most lepidoptera, this basal portion being only represented by a double or single fold or scar, traversing the medium cell; normally with three branches, the middle one sometimes fading out in its original central position as the result of the disintegration of its support, again all there being drawn into the system of the Radius or Cubitus and thus preserved.

V. *Cubitus*, two-branched.

VI. The *internal fold.* This, very often obsolete, becomes hardly ever a vein, so far as I know, and its enumeration might be thought superfluous.

VII. The *internal vein,* simple and constant.

VIII. A reduced loop at the base of VII., joining outwardly this vein, sometimes prolonged, again abbreviated, oblique, sometimes in a state of degeneration, sometimes wanting.

IX. Applied only by me to the more or less curved vein in the Papilionides, running from base to internal margin. Whether this is homologous with VIII., to which it is opposed in position, is uncertain.

HINDWINGS

The samė system prevails, since the veins are homologous with those on forewings. In *Hepialus* and *Micropteryx* the radius is generalised and exhibits five branches. In all other lepidoptera, except probably some Tineidae like *C. familiella*, the radius is single-branched. Since the five-branched radius represents a generalised condition, we infer that the primitive lepidoptera had a five-branched radius on the hindwings and that the front and hind pair of wings were in this feature alike, as is the case in *Hepialus*, which we accordingly treat as a specialised survival of a once generally prevailing type. Most specialisation seems to be inaugurated on the hindwings and we find, generally, that here the details of the specialising movements are more clearly expressed. While the same system of enumeration applies equally to the veins of the hindwings, the costal vein has a different expression from that on primaries. The frenulum of the moths is a specialised bristle or hair, not a vein; while the original clothing of the lepidoptera seems to have been hairy, the scales being probably a more recent specialisation. The costal vein forms, on the hindwings of the diurnal butterflies, a salient projection at base, where it generally issues from vein II. Its evolution is shown by the Parnassi-Papilionidae; in *Papilio* it appears as a blunt separate process; in *Thais* its fusion at extreme base is partially, in *Parnassius* fully accomplished. It is sometimes called the 'praecostal spur.' The internal veins below VII., probably originally parallel rods, usually present but varying in number or in completeness, are sometimes all wanting, as in the Papilionides and most of the Saturniades. Besides these parallel veins there are a few cross-veins on both wings, connecting two parallel veins, as the intercostal vein in the hindwings of the Sphingides, and the vein connecting cubitus and vein VIII. in the forewings of *Papilio*. According to my view, which is here opposed to that of Professor Comstock, these are all, including the usual cross-vein closing the medium cell outwardly, relics, perhaps secondarily modified, of former systems of cross-veins, which have partly disappeared with time. However, this conclusion is based on analogy. But the question becomes important, because, under my view, vein VI. will have quite vanished in *Papilio*, while a former cross-vein will have been here retained.

The whole system of neuration, in respect to the position of the veins, may be divided as follows: (1) Primary veins and their branches: radius, media, cubitus. But the media is now in a state of flux, of gradual wearing out, a process which culminates in genera like *Nymphalis, Naias, Potamis*,[1] *Rothschildia, Samia, Philosamia, Attacus*, where the media ceases to exist as a distinct system; (2) Subprimary veins, longitudinal nervures lying between the radius and costal edge of the wing (subcostal and costal veins), and again between the cubitus and internal margin (veins VI., VII., etc.). Veins II. (subcostal) and VII. on the forewings are constant in all lepidoptera I have yet examined; but, on hind-wings, VII. is sometimes shortened, tending to obliteration as in *Parnassius*; VI. has almost everywhere practically passed away. The condition which VII. displays in *Parnassius* shows us how VIII. has been gradually got rid of in the Papilionides.

Variation occurs sparingly with the movable veins within the limit of the same species. Here these veins feebly take the same course in variation which I have observed taking place as between different species and higher assemblages of forms. These variations do not affect the general stability of the wing pattern; they seem corroborative of the view of the general direction of the changes which I have worked out. They also sustain a view that the differences we remark are quantitative rather than qualitative. Aberrational stumps of veins may be also found; their unexpected presence being perhaps owing to excess of nutriment or to mechanical, irritational causes. Again, rarely, a normally occurring vein will suddenly cease, probably owing to defective tissue.

To guide the student of the neuration, we have laid down the following axiom: The extent of the absorption is everywhere the measure of the specialisation.

Now, taking the venation of all the forms of the day butterflies together, and comparing them, we find only one character which allows of an apparently sharp dichotomy. This is presented by the short, spur-like, stout vein which runs from the base of the wing in the Papilionides and, in stronger or gentler curve, meets the internal margin, not far from the base of the forewings. This character distinguishes the Parnassiidae and Papilionidae from all the other day butterflies, including the Hesperiadae. This vein belongs to the substationary series of independent unbranched veins, which vanish by disintegration and connect with the base of the wing. The characters we get from it are therefore of subprimary value.

We make here our first main division of the mass of the day butterflies, and, before we go on any further, we must take up the

[1] Throughout this paper I use generic names as designating a specific type which has been ascertained by historical methods and published in entomological literature. My principal authority in the day butterflies for the correct use of generic titles is Mr Scudder's "Historical Sketch," etc., Salem, 1875, a useful work which should be brought up to date and reprinted.

movable veins and show the general progress of their evolution. The characters which we find in the movable veins are then secondary, since they are recurrent, exhibited by the varying positions of the veins, but always exhibited in a greater or less degree, and thus opposed in value to the fixed subprimary character of the internal vein of the Papilionides, which is of higher importance and points to a diphyletic origin for our day butterflies.

An extended comparison of the wings of butterflies and moths, shows us that the main movement lies with the media and its branches. The theory of the media and its system, based on its characters in the moths, is, that it was formerly, as it is now in *Hepialus*, an independent system, with its root in the base of the wing. That portion which formerly connected the cross-vein with its base, has vanished in most lepidoptera, leaving its traces, in the shape of longitudinal scars, on the membrane of the median cell. The cross-vein remains as a base or support for the three remaining branches; and, at first continuing to supply with nutriment these three branches, tends ever to yield to disintegration and set them free. Thus the original movement which led to the loss of the base of the median system is still in operation. After the disintegration of the base, it next attacks the cross-vein. After the cross-vein has disappeared, it next attacks the branches. If these remain in their original position, relying upon the cross-vein, they submit in turn to the process of obliteration, from want of support, supply, and connection, and the first to give way is the middle branch, vein IV_2. This is naturally so, because the cross-vein usually falls away first upon both sides of this branch, where this latter is persistent, while still retaining its tubular shape and active physiological use for the shorter pieces between IV_1 and the radius, and IV_3 and the cubitus. We see this state of affairs in the Blues and Skippers, where vein IV_2 becomes a mere scar. This worn-out vein IV_2 speaks for the age of the group. In *Hesperia* the short portion between IV_1 and the radius shows also signs of disintegration, and vein IV_1 will undoubtedly in time here share the fate of IV_2. Now, in the majority of lepidoptera, the branches of the media do not await this impending fate. They move away and attach themselves to either of the two stationary veins, the radius and cubitus. The movement commences by the upper median branch approaching the radius and the lower the cubitus, keeping up their supply of nutriment from these sources. And this addition to the systems of the radius or cubitus progresses so far that the origin and source of the two-branch veins of the media becomes at times obscured. That portion of the cross-vein, which formerly lay between these branches and the chief nervure on either side, is transformed into the true continuous base of the branches in their new position. Morphologically this base must be traced to the cross-vein, physiologically it becomes an active portion of the branch in its new attachment. All the angles of the former joining of the branch to the cross-vein, soften and at length depart. Sometimes the upper branch of the media is not content with this union, but becomes so greatly absorbed by the radius as to be emitted from the lower stationary branch of the latter, vein III_5, beyond the cell, *i.e.*, in the Pieridae and Nemeobiidae.

In the meantime what becomes of the middle branch of the media, vein IV_2? It, in its turn, yields to the attraction of either radius or cubitus and saves itself, ensures its perpetuity by attaching itself to one of these two controlling systems of the wing, the cross-vein which formed its base changing its relations in the same manner above described. If vein IV_2 stayed where it was, at the centre of the cross-vein, we see that it becomes eventually destroyed, as in the Lycaeni-Hesperiadae. Saving itself, in either direction, up or down, it becomes preserved as a functional vein. No better instance of this can be cited than that offered by the common 'Skipper,' *Augiades sylvanus*.. On its forewings, vein IV_2, still keeping central, has succeeded in retaining its connection with the cubitus after the disintegration of the cross-vein above its original point of issuance; and this through a transformation of the lower part of the cross-vein, which latter has now assumed function and appearance of the true base of the branch, here bending the base of the branch down towards the cubitus. On the hindwings vein IV_2 has failed to retain its connection, the source of supply has given out with the disintegration of the cross-vein, and in consequence this middle branch of the media has almost entirely vanished, through degeneration, from the surface of the wing.

To resume, the cross-vein eventually entirely melts away between IV_2 and IV_3, or between IV_1 and IV_2, as the case may be, and the cell opens. In this state of affairs the media, as a system, has disappeared. Its first base, at the root of the wing, first went, and its second, artificial base, the cross-vein, now goes by the board. The two surviving primary veins, the radius and cubitus, become its residuary legatees and receive a larger or smaller share as they have induced the odd, middle branch of the three, vein IV_2, to join their respective systems. This process is unequally expressed in the wings of all the various forms of the lepidoptera. The movement is not regular, seems arrested in a number of genera at certain points, but it is always displayed in the day butterflies, of which I am now especially writing, and the amount of its progress is the evident gauge of the specialisation of the butterfly in this direction. I have called this the first direction because it seems to be more important, more susceptible of use in classification, by reason of the number of details, the number of different stages, which its study reveals to us.[1]

Probably more ancient in its origin, is what I have called the second direction in which the movable veins express the evolution of the primary wing. This is the reduction of the number of the branches of the radius of primaries by absorption. The original, generalised number is five. This becomes reduced to four and even three. An eminent case of the result of absorption, where the two directions join forces, so to speak, is offered by the large, common white butterfly *Mancipium brassicae*. Here the evolution of the primary wing has proceeded in the first direction, so that the upper branch of the media, vein IV_1, has become absorbed by the stationary branch of the radius, vein III_5, to a point nearly one-third of the length of the latter outside of the cell. At the same time the evolution in the second direction has reduced the radial branches to three from the original five. In consequence we have for vein III_5 the formula $III 3 + 4 + 5$, III_5 being the absorbing vein; now, if the absorption of IV_1 by this vein became complete, we should have, for the single resulting vein, the formula: $III 3 + 4 + 5 + IV_1$ We may here stay our remarks on the second direction afforded by the evolution of the forewing with the observation: that it appears sporadically, perhaps in connection with the original effort which apparently transformed the five-branched radius of the secondaries in the primeval lepidopteron into the single branched radius which we find in the vast majority of the lepidoptera of to-day. According to our view it is the normal march of wing evolution in the order, that it proceeds from the hindwings, and that the primaries consequently remain in a generalised condition in having the radius three to five branched, where the same forms show a single-branched radius in the secondaries. It is far from certain, however, that a single-branched radius on the primaries is the 'end' aimed at by the reduction of the branches. The morphological difficulties have been discussed by us in our original communication.[2] The second direction appears independently in the Parnassiinae, the Pierinae (except Anthocharini), the Riodinidae and Lycaenidae and in the Saturniades.[3] That this is a specialisation is plainly apparent from the sequence in the Whites from a five- to a three-branched

[1] Those authors, who speak of the 'cubitus becoming four-branched,' do not appear to have understood the evolution of this portion of the wing, or appreciated the direction of the slow and halting movement which leads to the disappearance of the median system of the veins.

[2] Compare *Mittheilungen aus d. Roemer Museum*, No. 8, 6. From the movement of the radial branches no less than the greater absorption of vein IV_1, I would disagree with Comstock's statement that the Nymphalidae show a greater specialisation of the wings than the Pieridae.

[3] In the Agliadae the movement in the first direction seems to lag behind the movement in the second, the reduction of the radial branches. The Citheroniadae resemble also the Pieridae in the absorption of IV_1.

radius of the forewings, and we can here notice the grades of absorption by which this change is finally accomplished. In the presumed ancestors of lepidoptera the network of the wing was more complex, and the general tendency in the lepidoptera would be thus evidently towards a simplification of the neuration. The second direction is apparently of lesser importance than the first, for the reason that it is confined by its nature to the forewings of a minority of the lepidoptera; whereas what we have called the first direction is now active everywhere on both pairs, and is seemingly indicative of a process inaugurated on the hindwings, to be echoed by the forewings. The second direction, it may be hypothetically considered, has completed its task with the hindwings of such lepidoptera which have the radius already reduced to a single unbranched vein.

To summarise the principal openings through which I have tried to carry the working theory of the evolution of the neuration beyond what had been previously attained :—

1. I try to show that the suppression of the media is the result of a continuous movement, variously expressed by the host of forms, which, after absorbing the connection of the system with the base of the wing and thoracic source of supply, next disintegrates the cross-vein, and finally distributes the outside branches of the media between radius and cubitus. I designate and explain the process by which the cubitus becomes three or four branched.

2. I try to show that the piece of the cross-vein closing the cell and lying between the branches of the media and either radius or cubitus, becomes functionally the base of the branches in their new position after the disappearance of the central connecting portion of the cross-vein. Its formed morphological character as a portion of the cross-vein becomes gradually lost, the angles softening, and all trace of its former condition becoming obliterated. This process may be clearly traced on the forewings of *Lycaena*, *Thecla*, *Zephyrus*, and of *Augiades sylvanus*.

3. I try to show that the absorption of the radial branches is due to a sporadic movement upon separate lines of descent, and that this movement is probably reminiscent of the similar action which has apparently very generally been fully carried out on the hindwings of most existing lepidoptera. It is questionable whether we can assume that a corresponding simplification can be attained by the radius of the forewings, and this from the different conditions of the front portion of the forewings as compared with that of the hindwings. There seems ground for believing that the absorption of the media has been commenced at a later period than the reduction of the branches of the radius.

4. I try to show that the evolutionary movements are, generally

speaking, inaugurated with the hindwings, and that these usually exhibit the effects more plainly than the forewings in one and the same specimen. We should logically expect this to be the case from the entire direction of specialisation in insects, and we may regard it as arising ultimately from mechanical causes.

5. The general parallelism of the evolutionary movement of the neuration in the two main directions throughout the lepidoptera, as explained by me, exposes its secondary nature.

We may now apply these observations to the classification of the Palaearctic and Nearctic butterflies especially, premising that the facts recorded here do not furnish everywhere the basis for a new system, but are mainly of use to throw light on the phylogeny and correct the sequence of all the groups and forms. We endeavour to exhaust the capabilities of the neuration as a guide to the understanding of the butterflies. We use the characters of the neuration especially as the readiest index to decide upon the position of genera, in order that we may avoid the common mistake of deriving the generalised from the specialised genus, a mistake we believe to find in the recent 'Handbook' of Mr Meyrick, and elsewhere. Commencement is made with the more specialised groups in each family, and the sequence is the one recommended to be followed in books and collections.

A. PAPILIONIDES. Vein IX of forewings present, a last more or less curved and shortened, free and longitudinal vein running from base of wing to internal margin.

Fam. I. *Parnassiidae*. Vein IV_2 of forewings inclines towards the cubitus. No cross-vein between cubitus and vein VII.

Sub-fam. 1. *Parnassiinae*. Vein IV_1 springs from the radius ; the radius is specialised and four-branched : $III_1 +_2$, III_3, III_4, III_5

Sub-fam. 2. *Zerynthianae* (*Thaidinae*). Vein IV_1 springs from cross-vein ; the radius is five-branched. Vein III_3 shows a diverging movement, and has slipped forward to beyond the cell.

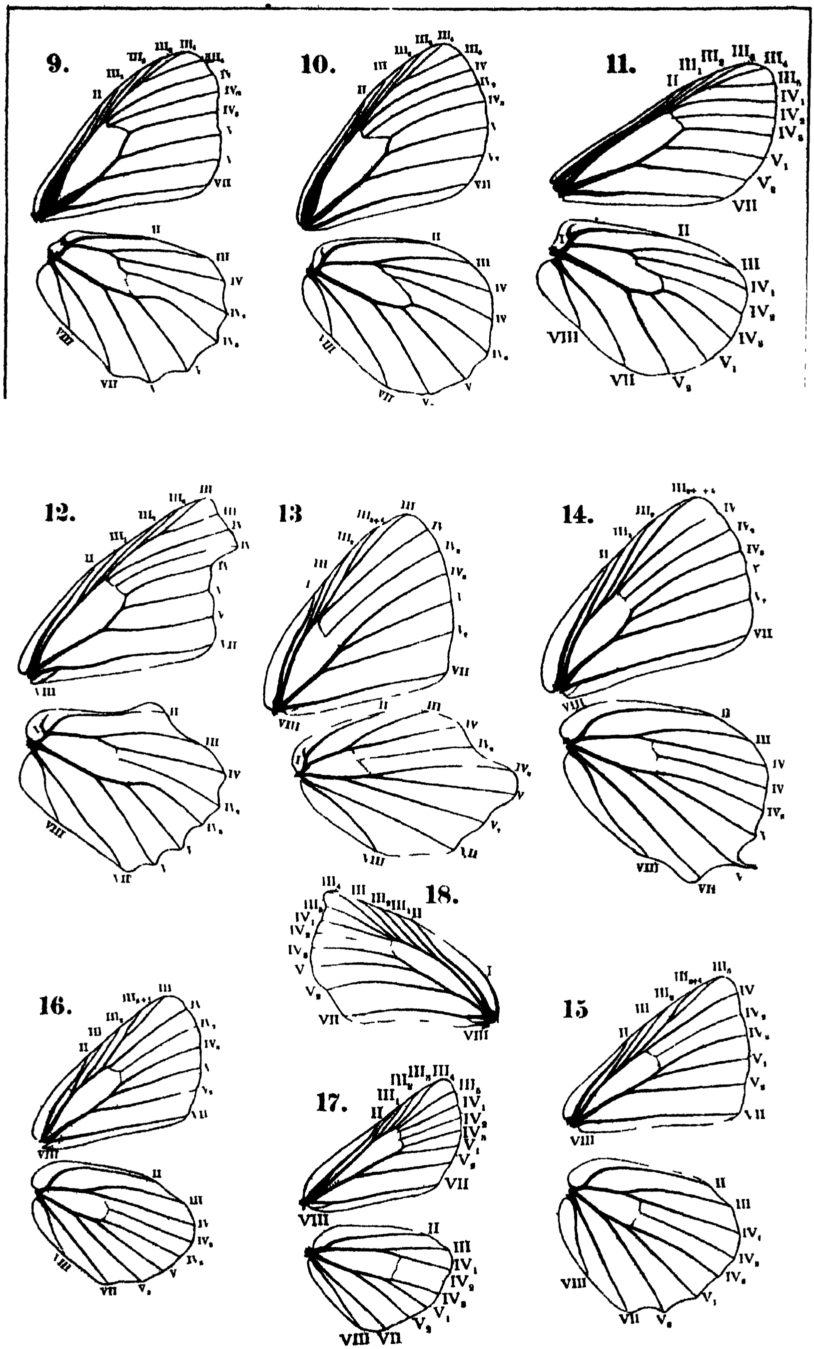
Fam. II. *Papilionidae*. Vein IV_2 maintains a central position on forewings ; on hindwings it inclines towards the cubitus. A cross-vein between cubitus and VII. Radius five-branched, generalised.

B. HESPERIADES. Vein IX of forewings absent ; vein VIII forms a loop at base of VII, sometimes reduced to a scar, again vanished. This character is not exclusive of the moths ; Sphingides, Saturniades, etc.

[Group *Pieri-Nymphalidae*, characterised by the more unequal spacing between the veins and the special line of progress in the disintegration of the media and its system, which runs parallel to the course pursued in the Papilionides, as well as in most other lepidoptera, by the branches not remaining stationary, but moving to radius or cubitus, and ultimately fusing with the systems of the latter primary veins.]

Fam. III. *Pieridae*. Radius three- to five-branched. This family differs from the succeeding four by its specialisation in two directions. It differs from the brush-footed butterflies by the movement of IV_1, which leaves the cross-vein and emerges from radius in the Pierinae ; the Nemeobiidae repeat this movement on hindwings.

Sub-fam. 1. *Pierinae*. The first branch of radius springs from upper margin of cell. The absorption of veins gives no tribal characters. The mass of genera belong here.



Photolith F H Bödeker, Hildesheim.

A RADCLIFFE GROTE. WINGS OF BUTTERFLIES

Sub-fam. 2. *Leptidianae.* The first branch of radius springs from a point beyond the retreating median cell. Radius five-branched. Vein IV_2 central on forewings, inclining to cubitus on hindwings. Type peculiar, and relatively generalised by the five-branched radius and position of IV_2.

Fam. IV. *Nymphalidae.* Vein III_4 of forewings thrown off upon external margin below, exceptionally at apices. Radius generalised, five-branched.

Sub-fam. 1. *Nymphalinae.* Radius and subcostal vein on hindwings fused to a point opposite the issuance of the abbreviated vein I[1]. To this group belong the Limenitid genera, such as: *Nymphalis, Naias, Limenitis, Basilarchia*, etc.

Sub-fam. 2. *Argynninae.* Radius and subcostal, on hindwings fused to a varying point, but always before that of issuance of vein I. This group contains the mass of forms, the tribes into which it may be resolved are not usually definable taxonomically by distinctive characters of venation. The smaller groups vary in respect to the degree of specialisation in the one direction of the disintegration of the media. The 'Emperor' butterflies appear to be most specialised; the 'Fritillaries' are most generalised in the holarctic fauna. Here, among these latter, vein III_4 seems exceptionally to attain apices from below in certain exotic genera.

Fam. V. *Agapetidae* (=*Satyridae* preocc.). Vein III_4 of forewings thrown off to apex of wing; all traces of Vein VIII appear to have completely vanished. Radius generalised, five-branched.

Sub-fam. 1. *Pararginae.* Vein IV_3 of hindwings joins cubitus, thus agreeing, by convergence, with the position of this vein in the Nymphalidae.

Sub-fam. 2. *Agapetinae.* Vein IV_3 of hindwings joins cross-vein, as in the following family and in the Whites (Pierinae).

Fam. VI. *Heliconidae.* Vein VIII absent on forewings; II and III on hindwings hardly absorbed at base.

Fam. VII. *Limnadidae* (*Danaidae*). Vein III_4 of forewings thrown off to apex of wing. Vein VIII present. Radius generalised, five-branched. II and III of hindwings absorbed as in Nymphalinae.

Fam. VIII. *Libytheidae.* Vein III_4 of primaries thrown off to costa, just before apex of wing. Vein VIII present. Vein IV_3 of hindwings joins cubitas, as in the Nymphalidae and Pararginae, not as in the Limnadidae. Radius, five-branched.

Fam. IX. *Nemeobiidae.* Vein IV_1 on hindwings absorbed by the radius as in Pieridae. In preceding three families this vein does not leave the upper and outer angle of the median cell. Radius, five-branched.[2]

[Group *Lycaeni-Hesperiadae*, characterised by the equal spacing between the veins, and by the peculiar progress in disintegration of media, of which the branches remain *in situ*, leading generally to their obliteration (especially of IV_3 on secondaries) on disintegration of cross-veins, which

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[1] Prof. Comstock also gives this comparative character for the 'Purples' in the Manual, 406, a work with which I was unacquainted when writing the "Tagfalter von Hildesheim." Comstock follows (l.c. 410) with *Anaea*; but, alone on this character, the Goatweed butterflies are clearly more generalised forms. The latter are also much more generalised than *Potamis iris*, and probably have no proper claim to the fantastic title of 'Emperors.'

[2] The immature stages of *Nemeobius* seem to show the Lycaenid character, while the wings of the butterfly appear to be developed upon the Pieri-Nymphalid pattern involving the movement of the branches of the media. It might be thus a survivor of a separate line connecting with the ancestral stem of both groups, since it shows features of each. In any case, whether the pattern of the wing is here primary or not, *Nemeobius* represents a distinct family type, which is my original contention. It cannot well be derived from the Lycaenid type, since in this the radius is specialised, while in *Nemeobius* the five-branched condition is retained.

latter serve as their *point d'appui*. In rare instances, such as *Augiades sylvanus*, and perhaps the Megathymidae, IV_2, while retaining its central position, has maintained on forewings its connection at base with cubitus, while disappearing, from want of this, on hindwings.]

Fam. X. *Riodinidae* (=*Lemoniadæ*, *Erycinidae*, preocc.). Vein I of hindwings present, not absorbed. Radius specialised, four-branched. Specialisation of the type, *Riodina lysippus*, is comparable with that of the Zephyrini and Chrysophanini, tribes of the Theclinae, from which it may prove ultimately difficult to separate the Riodinidae by the neuration alone.

Fam. XI. *Lycaenidae*. Radius specialised, three to four branched; vein III_5 reaches apex. Vein I of hindwings absorbed.

Sub-fam. 1. *Theclinae*. Vein IV_1 of forewings fuses with radius. To this belong the Theclini, Zephyrini, Chrysophanini.

Sub-fam. 2. *Lycaeninae*. Vein IV_1 of forewings not fused with radius. Comprises the typical 'Blues.'

Fam. XII. *Megathymidae*. [Neuration not examined.]

Fam. XIII. *Hesperiadae*. Radius generalised, five-branched, all the veins separate. Vein III_5 reaches external margin below apex. On hindwings vein I absorbed.

Sub-fam. 1. *Pamphilinae*. Vein I of forewings absent.

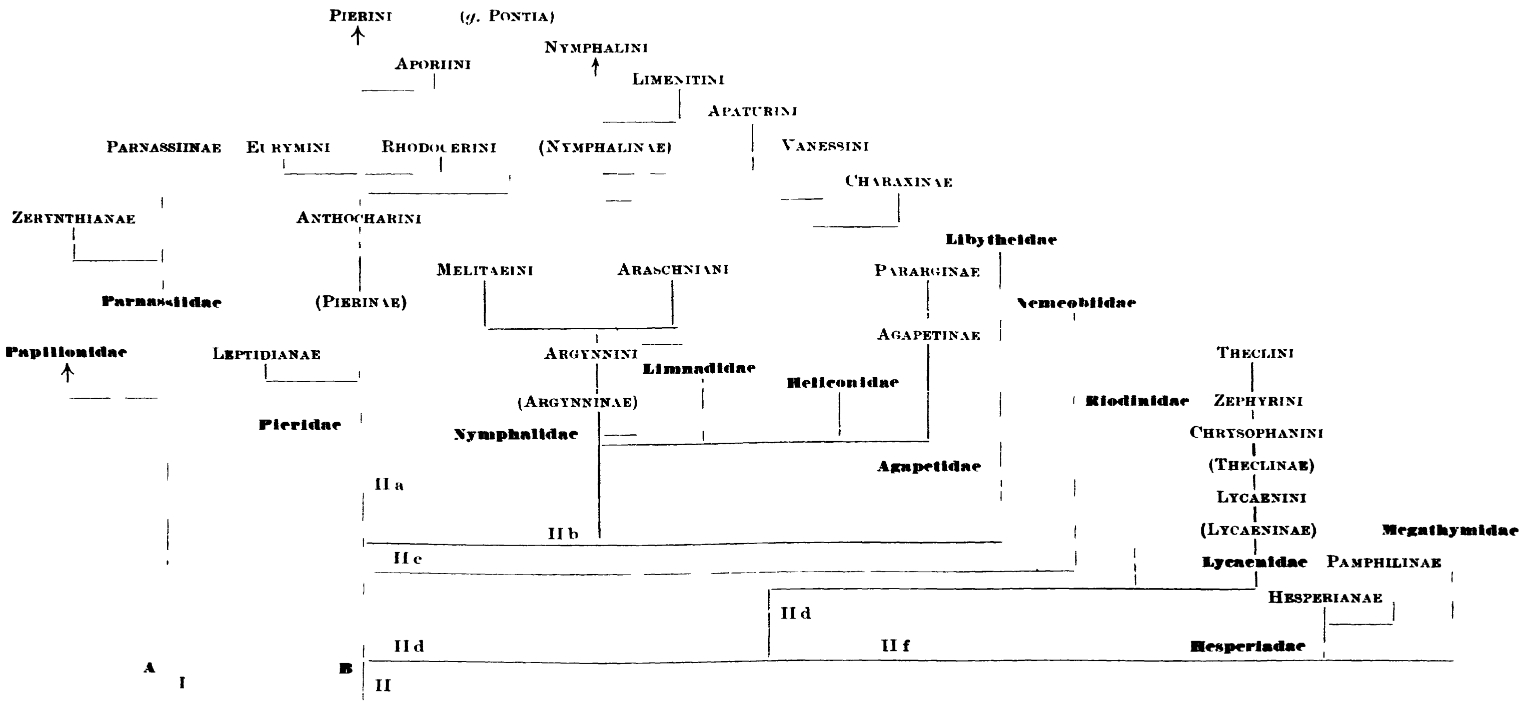
Sub-fam. 2. *Hesperianae*. Vein I of forewings present (a 'thickening of the costal edge').

The neuration, as thus explained, gives the following results:—The Parnassi-Papilionidae are separated from all other day butterflies by the presence of vein IX (the short vein on internal margin) on primary, and by the absence of vein VIII on secondary wings. *Parnassius* may be aptly compared with Attacid genera, the most specialised among the moths. The brush-footed butterflies resemble the Pieridae, and, except by the radial specialisation in the latter family are difficult to separate by neurational characters. In the position of vein IV_3 of the hindwings, the Nymphalidae, Pararginae, and Libytheidae agree. A change in the same character brings the Pieridae, Agapetidae, and Limnadidae together. The Heliconidae also share this same change of character, and are generalised butterflies as compared with the Nymphalidae proper. Unless proof can be offered that the pattern of neuration has been here secondarily acquired, we must class the Nemeobiidae with this group of the Hesperiades, the Pieri-Nymphalidae. The Lycaeni-Hesperiadae meet upon a common distinctive wing-pattern; the Lycaenidae differ in the main by the reduction of the radial branches. The character separating the Theclinae and the Lycaeninae lies in the degree of absorption of vein IV_1 by the Radius, the typical 'Blues' being the more generalised group. The value I would give the Papilionides in classification is that of a super-family. Whether Dr Dyar can show us any character in the position of the larval tubercles to aid us here is not yet ascertained. The Hesperiades divide into two groups, having a higher value than that possessed by the modern 'family' idea. I call them simply the Pieri-Nymphalidae and Lycaeni-Hesperidae. Notwithstanding the independent specialisations of the other body parts, the retained common features of neuration seem to show that the two series have a common phylogeny, and that their ancient progenitors, displaying no such disparities in head, feet, and antennae, as we find to separate them to-day, flew together over unremembered pastures —consanguineous. The 'Least Skipper,' *Ancyloxypha numitor*, lacking the recurved hooks to the tips of the antennae, may help us here, as this little butterfly has aided us to see the inapplicability of such a great term as 'Grypocera.' Since the furcations of the veins are the result of absorptions, those species, for instance, having a long bifurcation of III_4 with III_5 (i.e., *Charaxes*) cannot have been thrown off by forms in which the fork is short, as is now the case with the mass of holarctic Nymphalids in which the absorption is in an advanced condition. The long forks precede the short. To find relatives of *Charaxes*, forms in which the long furcation is likewise retained, we have even to go to South America, to butterflies like *Hypna clytemnestra* and *Consul hippona*. The general condition of the neuration in *Charaxes iason*, is a nearest approach to that we find in the separated veins of the Hesperiadae.

Now, it may be assumed, that in former geological periods the grouping of the butterflies was laxer than it is to-day, and, therefore, that many of the family groups we seem now able to separate, more or less clearly, were probably at one time so interconnected, by forms which have since dropped out, that their separation was not possible. Or, the characters we now apprehend had not then assumed their apparent importance and fixity. At that time the ancestors of the brush-footed butterflies may well have been connected with the main line of the six-footed butterflies, and possibly intermediate grades, showing also the more or less gradual acquirement of brush-footed character, may well have existed, since even now there is no uniformity in the exhibition of the aborted front feet. From small and specialised groups we can hardly expect the birth of such a feature, rather from large assemblages seeking fresh advantages, thus with a proneness to change, and already presenting a wide range of other structural modification. Such an assemblage may have existed in the ancestors of the Pieridae, since we have here the testimony of *Leptidia* to make it probable. The abyss separating *Leptidia* from the rest of the 'Whites' is profound, and this butterfly (*Leucophasia sinapis*) appears now as an isolated and probably relatively specialised survivor of what was possibly once a more or less extended group of the Pierids. So different is it, that *Leptidia* is excluded under Comstock's definition of the Pieridae (Manual, 375) so that either the diagnosis must be changed or the genus must form a new family. In the Papilionides vein IV_2 is central or cubital; in the Hesperiades it is central or radial, except apparently in *Leptidia*, where it is central on fore and cubital on hind wings, affording a parallel with *Papilio*. Otherwise the neuration of *Leptidia* is so alien to the rest of the 'Whites' as even to recall that of *Macroglossa*. It is thus a distinguishing character of the Pieridae that they offer a wide range of character, making them difficult to diagnose and giving the group a certain incongruity. The Pierids, then, may reasonably represent the matrix from which the brush-footed type has formerly emerged. The practical identity, in main features, of the wings of the Pieridae, Agapetidae, Heliconidae, and Limnadidae assists this conclusion. The inequality of the specialisations makes a linear sequence very difficult to defend the nearer the groups or forms come together.

Since I believe that we must regard the three-branched *Pontia* butterflies as a more modern and probably direct development from the Anthocharini, which have a five-branched radius, it may be that sometimes, at least, the loss of orange or yellow tints and the substitution of white may be looked upon as a specialisation. There remain the five-branched *Anthocharis* species which want the orange colour. But these are also more specialised than *Euchloe*; in these a movement of III_2 has taken place, carrying it at length beyond the cross-vein and cell. A tendency is thus indicated, prevented from equal step by the special causes determining colour and pattern. Somewhat similarly we find in the 'Large Cabbage White' a three-branched radius and in *Pieris rapae* a four-branched. Thus, in the more modern *Mancipium brassicae*, the specialisation of the radius would also be accompanied by a greater extent of white colour on the upper surface. On another separate line of immediate descent we have the North American *Nathalis iole* with a three-branched radius and its more generalised ally, *Eurema delia* with a four-branched radius. Here I find the yellow colour also to fade in the line of specialisation, although it does not become white; but the rosy fringes of *Eurema* have also gone and there is less black on the wing of *Nathalis*. There is perhaps not sufficient evidence amassed, since no dependence can be placed upon the assumed relationship of forms in literature where the specialisations of the wings have not been minutely studied, but what I have gathered points in the direction that increasing pallidity runs parallel with specialisation, and is developed independently on the generic phylogenetic lines. This is clearly the case with the Parnassiidae, where I find the white forms to be the most specialised. Roughly speaking, we might say that the butterflies tend to losing colour. I may add here that the examples of almost certainly direct descent, cited in the foregoing paragraph, bear out the secondary nature of the reduction of the radial branches, already mentioned and also pointed out by me in

PROPOSED DIPHYLETIC GENEALOGICAL TREE OF THE HOLARCTIC DAY BUTTERFLIES ARRANGED BY GROUPS BEYOND GENERA OR SPECIES



A. Vein IX of forewings present (Papilionides). **B.** Vein IX of forewings absent (Hesperiades). II a. Pierid stem. II b. Nymphalid branch. II c. Nemeobiid branch. II d. Hesperid main branch. II e. Lycaenid specialised branch. II f. Hesperid generalised branch.

the case of the Attacinae (l.c. 30, 31), and that we are not warranted in using this character alone for associative purposes.

In offering, in this paper, a reading of the facts presented by the study of the structure of butterfly wings, I have tried to give a fresh turn to the subject and to supply a firmer basis for classification. And while the results I have reached can find in this place only a partial application, and, indeed, while the neuration itself may not ultimately in every case give clearly the clue to phylogeny, yet the facts brought forward here cannot be disregarded in any system purporting to arrange the butterflies according to a probable blood relationship.

A. Radcliffe Grote.

Roemer Museum, Hildesheim.

EXPLANATION OF PLATE II.

The figures are obtained by photographic process.

III = Radial veins ; IV = Median veins ; V = Cubital veins.

Fig. 9. *Pararge aegeria.* Type of sub-family and genus. Size 1½/1. Attention is called to position of cross-vein on secondaries, where vein IV_3 has effected junction with cubital system as in the Nymphalidae, also relatively great absorption of II and III at base.

Fig. 10. *Erebia ligea.* Type of genus of Agapetinae. Size 1½/1. Compare condition of cross-vein of secondaries, typical of sub-family, the same as in Pieridae, Heliconidae, Limnadidae, where vein IV_3 still leaves cross-vein.

Fig. 11. *Oeneis aello.* Size 1½/1. Introduced to show its generalised character; cells strongly closed, absorption of II by III at base only partial as in *Erebia*, which it most resembles. Compare text.

Fig. 12. *Libythea celtis.* Type of genus and family. Size 2/1. Compare text.

Fig. 13. *Riodina lysippus.* Type of genus and family. Size 2/1. Compare this with figures of Lycaeni-Hesperiadae and especially with *Thecla* given here. Note that pattern of wing is same and the main difference is displayed by the reduction of radial branches in which the Riodinidae are equalled by the Zephyrini and beaten by the Theclini. Note retention of vein I on hind-wings, which has become absorbed in the Lycaenidae and Hesperiadae.

Fig. 14. *Thecla spini.* Type of genus and sub-family. Size 2/1. Compare above.

Fig. 15. *Lycaena endymion.* Considered by Scudder type of genus and family. Attend to four-branched radius, IV_1 not joined to radius, its base between junction of punctated degenerate cross-vein and radius being morphologically part of cross-vein.

Fig. 16. *Rusticus argus.* Type of genus. Size 2/1. This common 'Blue' differs a little from *Lycaena* in the position and condition of vein IV_1.

Fig. 17. *Pamphila palaemon.* Type of genus and sub-family. Size 2/1. Note absence of costal vein on forewings and presence of VIII.

Fig. 18. *Plathesperia* (olim *Eantis* vel *Achylodes*) *busiris*, Cram. sp. Type of genus. Primary wing. Size 1/1. Introduced here to show more clearly the typical hesperid pattern. Compare with *Hesperia malvae.*

II

The Geographical Distribution of the Actiniaria of Jamaica

AS bearing in some measure upon the question of a former connection between the Pacific and Atlantic Oceans across Central America, referred to in the September and October numbers of *Natural Science*, it may be of interest to give the results already obtained from a comparison of the Actiniaria occurring around the island of Jamaica with those met with elsewhere. Collections made at but a few localities, and from only shallow water, have yielded at least thirty-four species. With the exception of *Palythoa mammillosa* (Ellis & Sol.), none of these have been previously recorded from the island.

P. mammillosa is of peculiar local interest, in that it was obtained so long ago as 1687 by Sir Hans Sloane, when, as physician in the service of the Duke of Manchester, he visited Jamaica. It is roughly represented in his "Voyage" ('07), with the imposing title of *Lapidis Astroitidis sive stellaris primordia*; an excellent figure is also given by Ellis and Solander. Sloane's specimens were deposited in the British Museum, presumably along with his other collections, which went to form the nucleus of that institution.

The general Actinological features of the West Indies have long been known from the researches of Messrs Duchassaing and Michelotti ('60, '66). These authors, however, confined their attention mostly to Guadaloupe, St Thomas, and two or three other islands of the Lesser Antilles; and it has now become necessary to re-examine their types and determinations in order that they may be placed in the more modern systems of classification, founded upon a combination of external and anatomical characters. Within the last few years valuable researches of such a nature have been conducted by Professor J. P. McMurrich upon the Anemones of the Bahamas and the Bermudas ('89, '89a, '96).

Coming, as Jamaica does, somewhat midway between the islands studied by these workers, we are now in a position to compare the Actinian fauna of practically the whole Caribbean area, and as far north as the Bermudas. The result of such a comparison is to demonstrate a striking similarity throughout. Of the ten Bermudan species described by McMurrich, at least seven are found to be represented in the West Indian area, "sufficient to show that the Actinian fauna of the Bermudas has been derived from that of the West Indies" ('89a, p. 125). With two or three exceptions, all of the eighteen representatives which the same author describes from the Bahamas have now been found in Jamaica; while of the thirty-four species from the latter island, only nine—and of these one or two are doubtful—are not mentioned by Duchassaing and Michelotti from the southern extremity of the area under consideration.

Without any further detail, this brief summary is amply sufficient to show that a common origin must be assigned to the assemblage of Actiniae in the whole Caribbean region, including in this the Bahamas and the Bermudas. This is, however, what might certainly be expected considering the chain-like continuity of the islands, and the fact that practically the same currents pass along them all.

Turning now to comparisons with localities more distant some interesting results are obtained; though, owing to the lack of much information from even large areas, they are necessarily very incomplete. Most northern species are well known. In addition to these Professor Verrill ('96) has described a number of forms from the Pacific border of Central and South America; Ehrenberg ('34) and Klunzinger ('77) have made known those of the Red Sea; while Professor Haddon ('93 and '96) and Mr W. Saville-Kent ('93) have made us acquainted with abundant Australian and Papuan representatives.

In the sub-tribe Actininae most of the genera of the large families Sagartidae and Bunodidae appear world-wide in their distribution, though *Aiptasia* and *Bunodes*, so common in the North Atlantic, are rarely mentioned from the Southern seas. The Antheadae and Dendromelidae are more restricted to warmer parts, *Condylactis* and *Actinioides* [1] occurring in the Caribbean and Australian regions, as is also the case with the genus *Lebrunea.* [2]

The Aliciidae ('97), likewise limited to more tropical seas, appear in some ways to connect the Mediterranean with the Caribbean, Pacific, and Australian areas. Its genus *Bunodeopsis* is at present known only from Naples and Jamaica, but *Alicia* has specific representatives in the Mediterranean, at Madeira, Panama, Peru, Fiji, and Torres Straits.

Taking the sub-tribe Stichodactylinae, the genus *Corynactis* is

[1] The West Indian *Anthopleura pallida*, D. and M., will, I find, have to be transferred to the genus *Actinioides*.

[2] Haddon states that his *Viatrix cincta* is allied to *Hoplophoria coralligens*, H. V. Wilson. This latter I have lately been able to demonstrate is certainly a *Lebrunea*, and it is likely the Torres Straits specimen will turn out to be the same. The other West Indian species is the remarkable *L. neglecta*, D. and M.

evidently cosmopolitan,[1] with characters presenting but little variation. The families Discosomidae, Rhodactidae, and Phymanthidae are not represented in the North Atlantic,[2] except in the Caribbean area; and while some of the species occurring there are very similar to examples from the Red Sea and Australia, others exhibit important differences. The recent recovery in Jamaican waters of the *Actinoporus elegans*, of Duchassaing, enables me to assert that it bears no close relationship to the British *Aureliania*, as was supposed by Andres. Its anatomical structure shows it will probably necessitate the erection of a new family.

One important distinction between the Actiniae of the eastern and the western hemispheres can now be made in regard to the Thalassianthidae, Cryptodendridae, and Phymanthidae, families with arborescent or warted tentacles. So far as known, American waters are devoid of genera such as *Heterodactyla, Thalassianthus, Megalactis,* and *Actinodendron,* all bearing more or less complicated branching tentacles, while they are characteristic Indo-Pacific types.[3] The only West Indian species with well developed arborescent organs is *Lebrunea neglecta,* and these are columnar not tentacular in their origin; the frondose areas of the Phyllactidae, at one time regarded as tubercular and papillose tentacles, have been rightly compared by McMurrich with the acrorhagi of the Bunodidae, and are therefore also columnar.

Comparing the polyps of one area with those of the other the gradation from the simple to the complex tentacle can be traced in several genera and families. It is well illustrated by the genus *Phymanthus.* The West Indian *P. crucifer* has mere simple or bilobed tubercles along the oro-lateral aspect of its tentacles; indeed, as I have frequently found among the abundance of specimens at the Port Royal Cays, even these may be entirely absent, or individuals may be obtained representing all conditions in their numerical development. The tentacles of *P. loligo*, of the Red Sea, on the other hand, present us with all stages from a mere tubercular to a dendritic condition; while the so-called disc tentacles may be also branched, a condition which never occurs in the Western species. What may be regarded as the final stage of the series is exhibited by *P. mucosus* from Torres Straits, where Haddon found the lateral appendages to be even more dendritic than in *P. loligo.* The marginal and disc tentacles of the Caribbean *Rhodactis Sancti-Thomae* should be similarly compared with the irregularly palmate or

[1] *Draytonia myrcia*, D. and M., now obtained in Jamaica, has proved itself to be a *Corynactis*.
[2] Dr O. Carlgren, in his "*Nordische Actinien*," mentions no Stichodactylinae nor any Zoanthidae.
[3] Kwietniewski has just described a *Thalassianthus* and a new genus of *Discosomidae* from Ternate, one of the Moluccas Isles.

pinnatifid forms in the Australian *R. howesii*; or even with the still further exaggerated condition met with in *Crambractis arabica* of the Red Sea, and in *Sarcophianthus sertus* from New Ireland. The vesicle-like nodulated tentacles of the West Indian genus *Actinoporus* may in a like manner probably be regarded as a stage towards the complication found in *Cryptodendron* of the Indian Ocean. It may be further noted that the majority of the Discosomae of the Australian area are more gigantic than those of the Caribbean, and present tentacular differences.

From these examples it is manifest that the conditions favourable for the production of Actinian complexity of form have been better realized in the Australian and Red Seas than in Antillean waters.

Of the Zoanthidae, Haddon has remarked that, "so far as our knowledge at present extends, the Macrocneminae [*Epizoanthus*, *Parazoanthus*] alone are represented in the North Atlantic, although they are world wide in distribution."[1] This is in strong contrast with the Caribbean area, where, excepting *Sphenopus*, all the genera of the other sub-family, the Brachycneminae,—*Zoanthus*, *Isaurus*, *Gemmaria*, and *Palythoa*—occur in the greatest abundance, the first and last especially covering enormous patches of rock in shallow water. Similarly in the Pacific and Indian Oceans the Zoanthidae appear to be numerously represented, often by examples not greatly differing from the Western species.[2]

From his examination of the Bahaman forms Professor McMurrich was struck with the resemblance which the Actiniarian fauna of those islands presents to that of the Pacific, and its decided difference from that of the eastern coast of America, and, as shown above, the Bahaman species closely correspond with those of the more southern West Indian Islands. So far as the Actiniaria are concerned McMurrich ('89, p. 69) defines two great areas of distribution: "the Indo-Pacific, including the Indian and Pacific Oceans and the seas connected with them, such as the Red Sea; and the Atlantic, including in this the Mediterranean." His important conclusion is, however, that the Caribbean region of the Atlantic should be separated from the Atlantic region and united with the Indo-Pacific. Subsequent investigations carried out in other areas, particularly those at Torres Straits and on the Barrier Reef of Australia, serve but to emphasise this twofold division. But to my mind, a further distinction must be made between the Caribbeo-Pacific area and the Indo-Pacific, mainly in regard to the genera

[1] A *Gemmaria* has since been obtained from the Canary Islands ('96).

[2] It is indeed difficult to distinguish by any external or anatomical character *Isaurus asymmetricus*, H. and S., of Torres Straits from the West Indian *I. tuberculatus*, Duch. The close similarity of the species of *Zoanthus* and *Palythoa* is also well known.

with branching and arborescent tentacles; these, as already shown, being absent in the former, but prevalent in the latter.

As yet we know very little of the Actinology of the South Atlantic, that is, along the West Coast of Africa and the East Coast of South America. Collections from these parts and the intervening islands are great desiderata.

The present state of our knowledge with regard to the geographical distribution of the most important groups of the Actiniaria appears to be best summarised as follows:—

(1) *North Atlantic Region.* — Abundance of Sagartidae and Bunodidae; practical absence of Stichodactylinae and Zoantheae, except Macrocneminae.

(2) *Caribbeo-Pacific Region.*—Numerous Sagartidae and Bunodidae;[1] most of the Stichodactylinae and all Zoantheae; absence of Thalassianthidae and Cryptodendridae.

(3) *Indo-Pacific Region.*—Fewer Sagartidae and Bunodidae; numerous Zoantheae, Stichodactylinae, and especially Thalassianthidae and Cryptodendridae.

Descending to greater detail in connection with the second region we find that several of the Jamaican species are of importance on account of their direct resemblance to others occurring on the more immediate Pacific border of America. I have lately found *Sagartia nivea* (Lesson, non Gosse, 1860)[2] in some abundance on mangrove roots in Kingston Harbour, while the only other known localities are Paita and Callao, Peru. A new Jamaican *Epizoanthus*, living on Pinna shells, can scarcely be distinguished from *E. humilis*, Verr., occurring at Panama. The genus *Asteractis* has known representatives only at Panama and Jamaica. Also with many others of the species given by Verrill from around Panama and Peru, there is not the slightest doubt that when they come to be more thoroughly and anatomically examined they will be found to show a closer agreement with Caribbean forms than a present comparison will permit. The West Indian Actiniaria, as was first demonstrated by McMurrich on more general grounds, undoubtedly offer in detail additional evidence in favour of a past communication between the Atlantic and Pacific Oceans across what is now the Isthmus of Panama. This conclusion but supports the results already arrived at by investigators in other groups; by Dr Gunther from his study of the fishes; by various workers on the marine Mollusca, the Crustacea, and the Holothurians. While, as the pages of *Natural Science* have revealed for some time back, the continuity or discontinuity of the two Americas appears to be necessary for the solution of many of

[1] Jamaican waters contain at least seven species of Sagartids, and four Bunodidae.

[2] Histological examination shows that it will have to be removed from its present genus.

the problems bearing upon the migrations of the terrestrial mammals of Cretaceous and Tertiary times. Professor R. T. Hill, of the American Geological Survey, is at present engaged upon a comparative study in the field of the geology of the various West Indian Islands and that of the mainland; his results in this connection will be awaited with interest.

Although the Caribbeo-Pacific region is so distinct in its general facies, suggestions are not wanting of an admixture of certain Mediterranean forms. Previously known from the latter locality by only a single species, the genus *Bunodeopsis* has now been increased by two closely allied forms discovered in Jamaica. Living as the polyps do, attached to the leaves of marine plants such as *Posidonia*, *Zostera*, *Thalassia*, and *Ruppia*, which are often uprooted and float long distances, we can readily conceive of their travelling along with the equatorial currents from the one continent to the other. On such an explanation we may expect to find similar species around islands which, like Madeira and the Canaries, lie in the course of these currents. The occurrence of the allied genus *Alicia* in the Mediterranean, at Madeira, and at Panama, as well as at Fiji and Torres Straits, may perhaps be accounted for in some such way.[1]

Such instances must probably be looked upon as dependent on peculiar or accidental circumstances and not as interfering with the broad distinctions already laid down. J. E. DUERDEN.

[1] Mr J. Y. Johnson states that *A. mirabilis* frequently changed its position in confinement and may even float base upwards. Its vesicles also would assist it in floating.

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III

The Authenticity of Plateau Implements

I

THE paper of Mr W. Cunnington in the November issue of *Natural Science* calls for a reply. Perhaps it is all the more interesting because it is a recantation of former belief, and as some of us still retain the belief which Mr Cunnington has abandoned, it may not be out of place for one who was able to be of some use to Sir Joseph Prestwich to take up the gauntlet which Mr Cunnington has thrown down.

I must confess at starting that I do not understand what Mr Cunnington means by a silicious deposit on the surface of the ochreous flints. Some hundreds of these have passed through my hands, and I have never observed any such deposit. What Sir Joseph Prestwich did notice was that, "owing to a molecular change of the surface, the flint has sometimes assumed the white colour and glazed aspect (patina) of porcelain."[1]

The origin of the patination of flints has been frequently discussed, and as the conditions necessary for its formation obtain upon the plateau, the existence of this feature does not invalidate the claim to human work; but when it is borne in mind how much more readily patination takes place upon a sharp man-struck facet, it points rather the other way.

But, even if deposits of silica were proved to exist on the flints, this might be a matter of interest to the mineralogist, but cannot affect the question of the human workmanship of some of the flints, which stands on its own merits.

As for the difference in size of the flakes removed from the plateau flints, they can be easily paralleled in any collection of Neolithic flints of the same general appearance and age. From Cockerhurst, *e.g.*, from some of the larger neoliths, the flakes are quite Brobdignagian, and from others contrastedly Liliputian. Here, at all events, there is no question of ice-action.

Nor can the existence of striae, glacial or otherwise, prove or disprove the human workmanship of the bulbed flakes over which they pass.

Mr Cunnington states (p. 332) that "there are no known

[1] Prestwich, "Controverted Questions in Geology," p. 61.

instances in which the flint has been artificially flaked into the form of a weapon." This, of course, is incorrect (*vide Natural Science*, vol. iv. pp. 261 to 265). Here also (see accompanying Plates) are two instances in which the form is a very passable, rough spear-head, allowing for the blunting of the edges by abrasion in a river channel. No. 18 is from the plateau at Sepham Heath. No. 19 is from Broom Ballast Hole, Devon. No. 20 is a similarly worked neolith from Preston Farm, Shoreham. The Broom implement is of chert, much abraded evidently in a stream. No. 44 is put in for contrast.

Mr Cunnington again states (p. 332) that "why Eolithic man should have worked only on one surface of the stone is not explained." In general perhaps he did, but always he did not. Nos. 7 to 14 are a series of drills or 'rimers,' which, commencing with Eolithic man, pass on to Neolithic forms, and find their consummation in the nineteenth century engineer's bit. In all these specimens the surface of the flint is worked in one direction on one side of the point, and in the opposite direction on the other side, so that, in boring, the cutting edges of each side of the point follow each other exactly as the cutting edges of an engineer's bit do. Nos. 7 to 11 are eoliths. Nos. 12 to 14 are neoliths. These also occur, of chert, at Broom, Devon.

Again, Mr Cunnington says, "if the flints were worked, used, and then thrown down again, we should expect them to be widely scattered." So they are. In addition to the classical localities of Mr Benjamin Harrison, and also of Messrs W. J. Lewis Abbott, de Barri Crawshay, Montgomery Bell, H. Lewis and A. S. Kennard, I append a list of places at which I have found worked flints and chert of the plateau types:—

Above 600 feet contour line, Stockham Wood and Sepham Heath; above 500 feet contour, Shepherd's Barn, Preston Hill, Great Northfield, Goodberry Farm, Well Hill, Halstead, Borstal Hill, all in Kent; above 450 feet, Cockerhurst (Kent) and near Amersham, Bucks. Derived specimens, in valley gravels or on surface, Rampisham (Wilts), Micheldever (Hants), Broom (Devon), Jumper's Heath and Bromwich (Hants), Aylesford and Stud Hill (Kent), Cadamy's Pit and Railway Cutting, Wells, Norfolk, Mautort and St Riquier, near Abbeville, and St Acheul, near Amiens. The character of these implements, wherever found, is of the same primitive type as Mr B. Harrison's South Ash and other specimens.

Those found on the plateaux are in patches of old praepalaeolithic high level river gravels left isolated and discontinuous by later earth-sculpture.

Mr Cunnington objects to the uselessness of the shapes into which they have been made. I think that if Mr Cunnington had read Professor Prestwich's last words upon the subject,[1] he would have found his objections answered. It is candidly admitted that the uses of some forms have not been explained, but it merely begs the question to assert that their forms are useless. Mr W. J. Lewis Abbott points out that the same argument might be raised against the exquisitely worked trapezoidal forms from the Hastings kitchen midden, the valley of the Meuse and elsewhere.

The question of the authenticity of workmanship is substantiated by the parallelism of the chipping round the edges. It is simply impossible that the mere blind and unintelligent natural forces (1) of contusion in a torrent, or (2) crushing under ice could have produced that parallelism, often taking a flake off nowhere else, and at the same time giving rise to a well-defined outline. So that, although we may not be able to explain the use of each and every eolith, they group themselves into certain well-defined classes, which are as certainly not any more the result of chance contusion or ice-pressure than the parallelism of the chipping.

But are the plateau implements of useless forms? In many instances, at least, we can be certain that they are not.

(a) **Squared skin-scrapers.** *See* Prestwich, "Controv. Questions," plate i. No. 1. This specimen is in the Prestwich collection, British Museum, and is squared on three sides. So are specimens Nos. 15 to 17. No. 16 is from the 1894 pit, near Bat's Corner, and was taken out by me in presence of Mr. B. Harrison and three other men, December 23rd, 1894, from the bed of plateau gravel. These squared scrapers, Professor H. G. Seeley suggested to Professor Prestwich, are analogous to the square wooden skin-scrapers formerly used by tanners, who now use a wooden-backed steel one. I have verified these facts by a visit to Mr Barrow's tannery, Red Hill, Surrey. These eoliths are also similar to the square metal scraper still used by carpenters. Eolithic man, like the modern tanner, only used the long straight edge, and it was unnecessary to dress the fourth side at all. Eolithic men also used a blunt scraper, as the modern tanner and carpenter do.

(b) **Drills or rimers.** (*Vide* anteà.)

(c) **Hammers, ice-crushers, bone-breakers.** Specimens 1 to 5 are eolithic. No. 6 is a neolith from Castle Farm, Shoreham. No. 5 I took out of the plateau gravel bed, Bat's Corner, on the same occasion as mentioned above.

(d) **Flakers.** Eolithic specimens 41 to 43 all from the chalk plateau. No. 41 has a greenish polish on its flaked edge similar to No. 21. No. 43 is partly bleached.

(e) **Gouge.** Eolithic specimen No. 39 (*vide Journ. Anthrop.*

[1] "Controverted Questions," pp. 69, 70.

Institute, vol. xxi. pl. 20, No. 6), from Shepherd's Barn, analogous with neolithic gouge No. 40, from Pakefield.

(f) **Ovoidal and accepted forms.** (Not figured.)

(g) **Round scrapers.** Nos. 21 to 23 are eolithic; 24 and 25 neolithic. A triangular steel scraper, with wooden handle, with three slightly convex scraping edges, used to be in vogue thirty years ago (and probably is still) among painters and boat-builders.

(h) **Bone needle-makers.** Mr W. J. Lewis Abbott first explained these puzzling forms. Eolithic specimens are Nos. 32 to 36; 37 is a neolith; No. 38 modern Red Indian, certainly chipped into shape, though whether actually used for needle-making is uncertain.

(k) **Stick and sapling scrapers.** These small characteristic hollow scrapers are all eolithic, Nos. 25 to 31.

Mr Cunnington (p. 332) again says: "There are millions of flints, and it is not surprising that a large number occur in which the shape resembles palaeolithic implements." Sir Joseph Prestwich used to say that it would be such that primitive man would use, and instanced (on Professor Leidy's authority) that the North American Indians, on occasion, would take up and use unworked the first stone that suited their purpose. (*See* also Prestwich, "Controv. Questions," p. 69, on Pashoas.)

As to the immense number of eoliths, this need cause no more surprise than the vast number of palaeoliths found at Broom Ballast Hole, Bournemouth, or Abbeville. Last August, near Lowestoft, from one field, in about six visits I procured over 700 neolithic implements and flakes, and doubtless left thousands behind. Supposing a line of drainage to be formed in the field by a stream, all these, if undisturbed by man, would eventually find their way into the water channel, and if the water supply were subsequently cut off, would be gradually covered up by rain wash, to be dug up by some future anthropologist as an evidence of man's occupation. This is exactly what has happened at Bat's Corner and other places on the plateau of the North Downs. The pits were not dug haphazard, but a depression was selected on geological evidence on a spot that presented evidence of being an ancient line of river erosion, and it was here that the masses of ochreous flints and flakes were discovered exactly as geologists expected. The sides of that ancient Wealden river valley were long ago denuded, and only its bottom left. But it furnishes evidence of a people whose existence would have been unsuspected by many, but for Mr B. Harrison's earnest labours, interpreted by Sir Joseph Prestwich, Mr. W. J. Lewis Abbott and others. The same evidence is available on the southern half of the Weald.

Assuming for the moment that the natural forces of ice-pressure in frozen gravel produced the chipping on the edges of the plateau flints, we must all, I think, agree that those blind, natural forces show marked intelligence in (1) producing parallel flaking; (2) making tools similar to neolithic needle-makers and other hollow scrapers; (3) squared scrapers of flint (and of other materials), such as carpenters and tanners use; (4) flints worked on both sides of a point in opposite directions, so as to simulate the action of a modern steel engineer's bit; and (5) dress the stone in such a way that they performed these functions in the best manner and wore the largest possible time. No wonder that most of us still spell these "intelligent, blind forces" of ice-pressure in a simpler and more exact way in three letters—viz., MAN.

R. Ashington Bullen.

LIST OF SPECIMENS FIGURED (Plates IV.-VII.).

[*N*=neolith ; *D*=derived]

Hammers

1. Bower Lane, Kent, above 400 ft. contour.
2. Mautort, near Abbeville.
3. Halstead, Kent, 500 ft. contour.
4. Well Hill, Kent, 500 ft. do.
5. Bat's Corner Pit, 600 ft.
6. Castle Farm, Shoreham, Kent (N).

Drills or rimers

7. Shepherd's Barn, Shoreham, 500 ft. contour.
8. Preston Hill, Do., do.
9. Cockerhurst, Do., do.
10. Aylesford, Kent (D).
11. Otford Gravel Pit, Kent (D).
12. Walton Heath, Surrey (N).
13. Cockerhurst, Kent (N).
14. Lemsford Vicarage, Herts (N).

Squared skin-scrapers

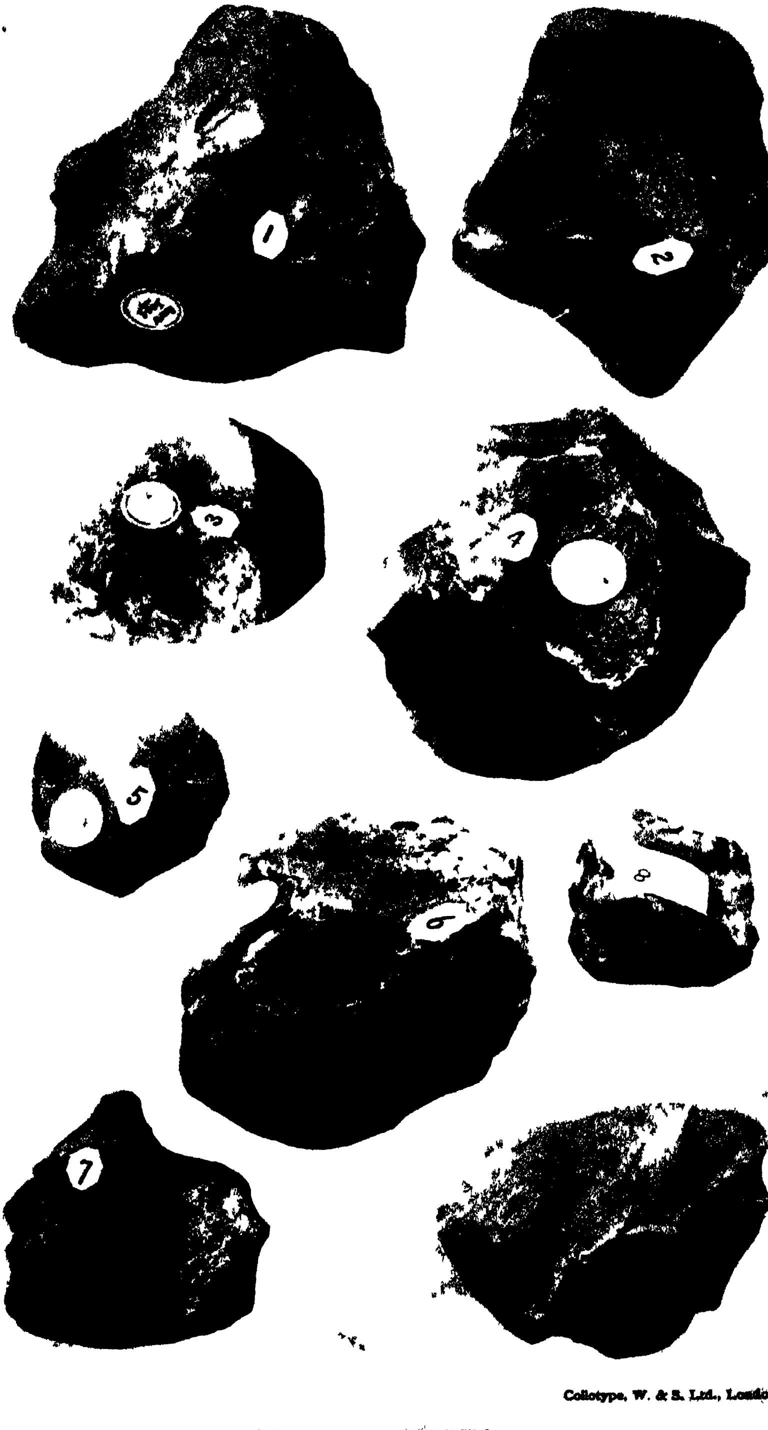
15. Shepherd's Barn, Shoreham, Kent, 500 ft. contour.
16. Bat's Corner Pit, Do., 600 ft. do.
17. Cockerhurst, Do., 450 ft. do.

Early form of spear

18. Sepham Heath, Shoreham, Kent, 600 ft. contour.
19. Broom Ballast Hole, Devon (D).
20. Preston Farm, Shoreham, Kent (N).
44. Chert Palaeolith, Broom, Devon.

Round (convex) scrapers

21. Wells, Norfolk (D); railway metalled with gravel from deep cutting.
22. Stockham Wood, Shoreham, Kent, 600 ft. contour.
23. Cockerhurst, Do., 450 ft. do.
24. Stockham Wood, Do., (N).
25. Oare Common, Dorset (N).



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Collotype, W. & S. Ltd., London.

FLINT IMPLEMENTS.

Stick and sapling (concave) scrapers

26. Jumper's Heath, Christ Church (D).
27. Near Amersham, Bucks, 400 ft. contour.
28. Hordwell Cliff, 60 ft. above sea.
29. Grange Pit, Hants (D).
30.
31. } Aylesford Pit, Kent (D).

Bone-needle-makers

32. Preston Hill, Shoreham, Kent, 500 ft. contour.
33. Cockerhurst, Do., 450 ft. do.
34. Goodberry, near Do., 600 ft. do.
35. Grange Pit, Rowner, Hants (D).
36. St Acheul, near Amiens, France (D)
37. Reigate, S. of L.G S. Escarpment (N).
38. Sacramento Valley, Ga, U.S.A. (white quartz, Red Indian).

Gouges

39. Shepherd's Barn, 500 ft. contour.
40. Pakefield, Suffolk (N).

Flaking tools

41. Cockerhurst, 450 ft. contour.
42. Jerkin's Wood, Shoreham, 500 ft. contour
43. Cockerhurst, Do, 450 ft. do.

[*N.B.*—All except No 1, front view, are much reduced. The Eoliths figured, except No. 22, passed through Sir Joseph Prestwich's hands and were approved by him.]

II

WHEN at a meeting of the British Association an Indian officer suggested that the plateau tools had been brought into their present forms by ice action, it provided a subject for merriment, as things sometimes do at those mixed meetings. But not even the bitterest opposers of the human origin of these things, who would gladly have welcomed any word that could have been urged against them with the slightest resemblance to reason, considered the idea as worthy of a moment's serious consideration. When, however, after a lapse of a few years it is again revived by one like Mr W. Cunnington, whose earlier labours command our admiration, the case becomes different, and one can only regret that he should have descended into the arena so ill-armoured, and with no weapon save four small stones; and although he at times calls in chemistry, geology and mechanics to his aid, in every case they turn upon him, and it is at their hands he dies. But the most painful features of his paper are his lamentable errors in matters of fact. The case is so wrongly stated that almost every assertion can be met with a flat denial, and as one's desire to be tender and respectful in this would draw out this article to undue length, I propose to confine myself to an examination of his main theory and its terms of statement.

After reading his own interpretations into the characters presented by his four Rosetta stones, he says :—"We therefore see that these plateau flints have been subjected to six different processes, all of which are undoubtedly natural, and which have left clearly recognisable traces" "by which we can determine the relative dates of the various chippings of the flints which are affirmed to be the work of man." Yet not one of these agencies ever did or could 'chip' a flint in the sense used by prehistoric anthropologists. Without pointing out the discrepancies between the text and his own summary, but confining ourselves to the latter, we find a splitting of the flint into slabs and tablets, two silicious incrustings, a staining, one or two sets of glacial striae, but not one word about the very action which alone characterises man's work. As to these six or seven processes, one is tempted to say a word or two upon them. Firstly, the flints of the plateau drifts are neither 'slabs' nor 'tablets,' they are of all shapes from rounded Eocene to hardly worn and sub-angular pebbles, differing but little from the mean of a score of Palaeolithic gravels. This presumed tabloid condition is brought about by a presumed 'Extreme Cold'; which, of course, is warmed into sunshine by the light of actual fact. We next have the extraordinary silicious depositions, which will doubtless surprise and amuse every chemist and petrologist of the day. What he here refers to is by no means clear to my mind. I do not know the Devizes specimen, but I have seen many scores of flints crushed and re-cemented in the Chalk, and have seen these after they had been reduced to pebbles in gravels, and so presume the Wiltshire specimen to be of Cretaceous age. Such re-cementing of crushed and fractured quartzites and agates is well known from the older rocks. I have frequently described them under the heading of faulted and brecciated agates, and have a large collection of them. I can confidently say that I have carefully examined, microscopically, the surface of the plateau flints, worked and unworked, for years, without seeing any trace of this brown- or white-washing. There are structures to which Mr Cunnington may refer, but their origin is so obviously different that one is afraid to credit an old observer with so great a mistake. Certainly the idea is altogether at variance with the researches made during the last generation upon the nature and properties of silica. As to the manner in which the flints were coloured, it must be admitted no great concession on Mr Cunnington's part to allow that "the staining is no doubt due to ferruginous solutions," and one would be tempted to ask, to what else should it be due, were it not for the fact that in the next sentence the iron is traced back to an earlier source in which the flints were once embedded?

As to the glacial striae, I fully endorse his opinions that specimens are abundant with striae, in no way distinguishable from those found upon hard stones from glaciated districts. I have considerably over a hundredweight of glacially striated flints and implements; these are often upon blocks off which several parallel flakes, five or six inches long, have been flaked, sometimes over bulbed flakes, six to eight inches long, and sometimes upon well-made implements. So that, while the striations show that the flaked surfaces antedate the conditions to which the implements have been subjected, they do not in any way militate against the human origin of striated worked flints.

As to the action of blown sand. While such a process would have no weight either one way or the other in connection with the authenticity of plateau man, may I ask Mr Cunnington if he is not confounding this with patination? A closer study of desert pebbles and the Dewlish gravels might perhaps convince him that this is the case. We thus see that these postulated conditions have either no existence at all, or have nothing to do with the authenticity of plateau man.

Mr Cunnington next attacks the chippings which brought the flints into their present shape and condition. Unfortunately, in his descriptions he is a little ambiguous, and, so as to prevent myself falling into the same error, perhaps I may be allowed to define the sense in which I employ similar words to those used by my esteemed fellow-worker. By a flake I mean a piece of flint removed from a block by a free blow or percussion, moving or applied in a given direction. The place of actual contact of the power will be marked by a distinct point, which upon the flake is known as the bulb of percussion, and upon the block as the pit of percussion. Around this point a series of waves or ripplings may be observed, the shape and direction of which are subject to mathematical laws and the homogeneity of the 'flaked' substance. I may say that, given any two of these quantities, the third can be obtained mathematically. Such being the case, we have often—though by no means always—a crucial test for man's work. The so-called frost pits arise in another way altogether; the action has many points in common with perlitic structure, but is too long a subject to digress into here. I have a collection of flints pitted into the shape of practically every type of implement. In other cases paleoliths are often pitted away, until only here and there are left traces of man's flaking. Without entering into further descriptions, we may say that in a frost pit or a 'pot lid' there is no pit or bulb of percussion. Depending upon the unaltered and homogeneous conditions of the flints, there are well-marked conchoidal ripplings, not so uniform as those produced from a blow, often extremely excentric, but they originate at a point seen on the base of the pit, and not at the periphery of the flint, where a little single or double isthmus will be seen rising as a little teat, with a corresponding depression upon the other part of the flint. For all these processes and structures I have more or less satisfactory names, which I hope some day to publish, but sufficient to say that this alone enables us to distinguish between frost pits and man's flaking, a task very often by no means so easy at it might be imagined, especially as we have 'starchy fissure' and several other phenomena to add to the complexities of the case; and to these may be added crushings and batterings.

It is, however, obvious that we should thoroughly understand all these processes, to be able to correctly decide in every case that may arise. I have recently described the secondary hall-mark of a man-made flake, the *craillure*,[1] but we need not be surprised at Mr Cunnington not referring to this, when we see how little he has made himself acquainted with the facts presented by the Plateauliths. But let us see how Mr Cunnington attacks the chippings of these things. His first argument is that it is often of different dates; he then proceeds to give evidence of this, and even tells us to what these were due. He says "the earliest fractures are the largest, as might be expected, since the conditions were the most vigorous. As the climate became milder the forces that acted on the flints became feebler, and the chips were therefore smaller." Who would have thought that from the size of the flakes struck from a flint geologists would have been informed of such fluctuations of climate, about which nature has otherwise been perfectly silent?

It probably never occurred to Mr Cunnington that, supposing there had been a set of small flakes removed first, these would have all been obliterated by the removal of subsequent larger flakes; and that supposing a flint in the first case were to be a fairly large one, with a set of small flakings first, and then a very large flake were removed, that upon that removed large flake the small chippings would not be the newer, but the older. But I fear all readers of *Natural Science* will have seen this is the logic unfortunately employed all through this article. His next great objection is that the chippings are of various ages. First of all, let me say at once he has not, nor can he show this to be the case, with more than a fair proportion of plateau specimens, and not upon one of them unless his opponent accepts his grounds upon which he estimates the dates. He then urges the fatality of the three or four different ages of chipping to the work being of human origin. Surely Mr Cunnington has seen specimens of boldly worked Palaeoliths re-worked and polished by Neolithic man, and again subsequently re-chipped. I suppose he would tell us the broad flakings were the result of rigorous cold, the polishing due to a glacial excursion, by

[1] *Natural Science*, vol. x. pp. 89 to 98.

which stones do get smoothed, and the smaller re-chippings to a 'milder' climate. When he attacks the features presented by the chipping one is quite pained. He says :—

(*a*) "The chipping is limited to the edges of the slabs." This is utterly false.

(*b*) "There is no known instance in which the flint has been artificially flaked into the form of a weapon." This is equally incorrect. I have numbers.

(*c*) The asserted human workmanship is limited to the chipping of the edges of naturally shaped flints. This is equally untrue. I have numbers of bulbed flakes which have been worked into shape, and many bearing an *èraillure.*

(*d*) The chipping being confined to one side of the flint is also misleading and incorrect, and only holds good in some types. Others are worked on both sides and pass up insensibly through a very extensive group I have called the transitional forms, when we have Plateau outlines with work essentially Palaeolithic. It is when we come to the causes which produced these chippings that we are most astounded. The first postulate is that "the chipping was due to some pressure which acted more or less at right angles to the surface of the flint." Surely Mr Cunnington can never have read anything that has been written on this point. Let him try the experiment, and when he can strike off a flake by a right-angled blow, let him show us how he does it. The next hypothesis is that the flakes were thus removed under glacial conditions. Here, fortunately, we pass from matters of mere conjecture to one to which we can appeal to nature herself. But, firstly, we may ask, do the four sets of chippings, separated by long intervals, represent four great glacial periods ? According to the various postulates they do. Secondly, can he give us any method by which we can distinguish the chippings of plateauliths from the palaeoliths ? Before me are specimens bearing the old Plateau work and subsequent Paleolithic and Neolithic work. As these are all mathematically similar, are we to add yet another three or four glacial periods ? But why raise matters of conjecture when we have nature herself to appeal to ? We have our glacial deposits passing over rocks of all ages, and in East Anglia these are largely flint-bearing. Surely if these plateauliths were of glacial origin we should find them here. But do we ? Certainly not.

If other arguments were wanted I might urge that quartzites of similar work to the flints of the plateau have been found in regions in Africa where no evidence of glacial action is known to occur. Dr Leith of Praetoria has sent several consignments of these to this country, to which I hope to refer at greater length on a future occasion.

I see there are several points raised by Mr Cunnington, upon which I have not touched, to which I could give a flat contradiction if space and patience allowed. It is useless to refer to his reading of the four specimens figured, seeing the conclusions must be worse than useless when drawn from such invalid postulates. I must, however, in conclusion, raise a protest against the misleading way in which Mr Cunnington brings Sir John Evans to his support. May I not ask if the communication which he cites does not refer to some particular specimens and not to all and everything that has been found upon the plateau? I can say that I have shown specimens to Sir John which, to use his own words, "no one need be ashamed of." I am further in the position to state that Sir John, instead of supporting this last glacial nightmare, like everybody else that I know utterly rejects it. I sincerely trust my esteemed old fellow-worker will not consider I have been hard upon him; but were I to hold my tongue when a cause in which one has laboured so hard, unremittingly, and conscientiously, and with perfectly unbiassed mind is so misstated—one feels it difficult not to say outraged—the very stones themselves would cry out.

W. J. LEWIS ABBOTT.

IV

Suess's Theories of Geographical Evolution[1]

IN spite of the apparent fickleness and inconstancy of the sea, the idea recurs throughout poetic literature that its main character is really its immutability. From Homer to Kipling, from Job to Matthew Arnold, poets have repeatedly expressed the idea,

> "Time writes no wrinkle on thine azure brow,
> Such as Creation's dawn beheld thou rollest now."

The teaching of uniformitarian geology supported the old notion of the poets. The change from

> "There where the long street roars, hath been
> The silent stillness of a central sea,"

was attributed to an oscillation of the land, not a variation in the level of the sea. The one level in nature that was taken as a reliable constant was the mean sea level. Gradually, however, the view has grown that Ordnance Datum is as inconstant a constant as most earthly guides. Gradually the idea has been accepted that the surface of the sea is no more an absolute plane than is Salisbury Plain, but that it is heaped up against the margins of the continents in a manner analogous to the upraising of water against the margin of a basin. As soon as belief in the fixity of sea level was shattered, many an apparently well established geological hypothesis was shown to require modification or fresh proof, and many a geological principle to require restatement. If the water level in the Central Pacific could rise owing to a reduction in the attractive force of the land masses on its margin (as for example by the sinking of an Antarctic Continent) then the formation of Coral Atolls might be formed, not by the slow subsidence of the sea floor, but by a gradual rise of the sea surface, as water flowed into the Central Pacific from its borders. Again, the apparent upraising of northern Scandinavia and subsidence of southern Scandinavia might be due not to an actual movement of the land, but to variation in the level of the two halves of the North Sea under the influence of changed winds and

[1] Ed. Suess, "La Face de la Terre (Das Antlitz der Erde)." Traduit sous la direction de Emmanuel de Margerie avec un preface par Marcel Bertrand. Vol. I. pp. xv. 835., 8vo, with 2 coloured maps and 122 figures. Paris: Armand, Colin & Cie, 1897. Price, 20 fr.]

ocean currents, and the formation of a fresh outlet through the Straits of Dover.

The first geologist to realise the full geological significance of the inconstancy of the sea level was Professor Eduard Suess, of Vienna. Recognising the importance of this fact he set to work to enquire if it could yield any help in developing a theory of geographical evolution. Geographers have always agreed that the distribution of land and water on the earth is not a haphazard arrangement, but is governed by some principle or law. There is, it is true, a remarkable dissimilarity between the different continents; but a closer comparison reveals many striking repetitions of the same arrangement. At first sight no two structures could look less alike than a quartz crystal, with its solid form and its simple outline, its flat faces and its straight edges, and a complex crystalline flake of snow, with its radiating cluster of feathery tufts of delicate filigree. But the crystallographer recognises that the quartz crystal and the snow flake have the same simple hexagonal symmetry, and are built on the same fundamental plan. So the geographers have felt that if we neglect accidental topographical details, we find so many points of striking resemblance between the great land masses, that there must be some underlying symmetry in continental form. A convincing statement of these coincidences was made by Professor Lapworth in a lecture to the Geographical Society in 1894, and formed the text of his presidential address to the Geological Section of the British Association at Edinburgh in 1892.

Quite early in the century, geologists set to work to construct theories that would explain continental forms, but with little success. The well-known southward direction of all peninsulas was stated in elementary text-books of geography, and was often explained as due to the southern hemisphere having a larger share of the ocean than its due, owing to its being heavier than the northern hemisphere. Several efforts have been made to attribute the direction of the main mountain chains to lines of weakness by torsion in the original crust of the earth. The theory is still popular, in spite of the overwhelming weight of palaeontological evidence against it, that the ocean basins and the continental masses were determined in the pre-zoic period, and that they have been permanent throughout geological time. These theories of continental form have, however, been either so vague as to be useless, or if sufficiently definite to be helpful, they have been shewn inconsistent with essential facts.

As M. Bertrand shows in an admirable preface to the French translation, Elie de Beaumont's brilliant speculations failed owing to his having filled the gaps in his foundation of facts by guesswork. Suess realises this danger, and accordingly sets to work on a different plan. As he tells us at the outset, he repudiates preconceived notions; and the first two volumes of his work tries to state the problem, rather than to solve it. In the spirit of the founders of the Geological Society, he holds that synthesis must precede analysis. "Das Antlitz der Erde" was therefore planned to consist of four parts, of which only three have yet been published. The first volume was issued in Vienna in 1885, and contained the first two parts; the third part, forming the second volume, followed three years later. A French translation is now issued for the first two parts. The work has been admirably done by M. de Margerie and a group of collaborators, who add many references and footnotes, and sometimes important interpolations in the text, in order to bring the work up to date. One very valuable addition to the French edition is an increased number of sketch-maps. Many of the new figures are well chosen, and are very clear. The paucity of maps in the original issue was its one fault.

We owe M. de Margerie and his colleagues so much gratitude for the great labour of this translation that it is ungracious to criticise. But there is one improvement that might perhaps be made. The first volume left Suess's hands more than thirteen years ago. Many statements in the text he would now, no doubt, wish to qualify or withdraw. There is no word in the volume from its author to suggest what corrections he would wish to make, and how far it represents his present views. It might have saved much future misunderstanding if we had been told whether the re-issue of some of the suggestions is to be taken as a proof that they are still regarded as probable by Professor Suess.

The volume, of which the French translation has just been issued, consists of a short introduction followed by the seventeen chapters of the first two parts. Each chapter forms a masterly geological essay, and may be read separately with profit by specialists on the subjects discussed. Professor Suess's knowledge of geological literature is colossal, and he illuminates every subject he treats with the light of his poetical imagination. Each chapter is a gem; but the thread by which they are to be strung into a connected chain has not yet been completely spun. It is not very easy, therefore, to summarise the work into a connected argument, which may, however, be stated somewhat as follows.

It is known that in many areas as, *e.g.*, on the eastern coast of the Tyrhenian Sea, there are detached fragments of ancient shore lines which rest in one place on the face of an abrupt spur from the Apennines, in another traverse a cliff of limestone round an old bay, and elsewhere lie on the old Archean rocks of Calabria or on the late Cainozoic tuffs of Etna. The old shore-line, however, maintains its absolutely horizontality. Suess contends that it would be a physical impossibility for so complex an area, composed of beds of such different compositions, hardness, and dip, to have been upraised without any relative displacement of the different parts. Therefore, argues Suess, as the land cannot have been upraised, the sea level must have fallen. That alternations in the position of land and water are due to movements of the land is one of the fundamental principles of Lyellism. Suess, therefore, proceeds to enquire whether geological evidence supports Lyell's view, or whether there is any proof of actual variation in the form or position of the hydrosphere, which would locally alter the height of its upper surface. The introductory chapter states the conclusions of the geodesists as to the existing inequalities in the sea surface. Then he asks is there any historical evidence as to the flooding of land areas without subsidence of the land? He tells again the Chaldean story of the Noachian deluge as revealed by the *Daily Telegraph* tablets; he concludes that the absence of similar traditions in Egypt proves that the flood was local, and he shows that the whole of the facts in the Chaldean version are explicable by a flooding of the Mesopotamian plain caused by earthquakes in the Persian Gulf. The great shock was, no doubt, preceded by preliminary shocks, which may have acted as a warning to the wise, and the great flood may have been increased by an accompanying cyclone. The author then proceeds to discuss some of the principles of dynamical geology, in chapters on the forces that move the land masses. He first considers earthquakes, and describes four typical earthquake areas. He concludes that earth movements are of two kinds; foldings produced by tangential thrusts, as in mountain building and subsidences produced by radial contraction. The uplift of large, uncontorted superficial areas he declines to accept. He discusses the oft-quoted assertion as to the elevation of the South American coast by earthquakes, and denies that the evidence supports that conclusion. He is here opposed to Darwin, so he goes into the case fully and appears to prove his contention. In order to get light as to the internal nature of the earth, he then turns to vulcanism. He describes the laccolitic habit of acid lavas and the broad flows and sheets of basic lavas, and proposes the term 'batholites'[1] for those great masses of granitic rocks, which may perhaps be most briefly explained as plutonic laccolites, in which the igneous rock occupies a pre-existing cavity which it did not itself form. The existence of such cavities must be inferred in order to explain vertical subsidences. Hence, from a study of a series of typical earthquakes, geognostic dislocations, and vol-

[1] An illustration of the extent to which Suess' work has been neglected in England is shown by the fact that, in the Geological Society's last discussion on the nature of the Dartmoor granite, though the question of its laccolitic origin was considered, the term batholite was not mentioned in the report.

canic phenomena, Suess concludes that, in the processes of the earth's contraction by cooling, vast subterranean hollows are left, which are usually filled by a sinking of the superficial crust; while owing to tangential thrusts caused by the contraction of the outer crust, violent foldings are produced along certain lines. In some cases, the lateral thrusts and the vertical subsidences are combined; but Professor Suess can find no agency that will account for the uplift of large areas in mass and undisturbed.

The rest of the present volume is devoted to a series of descriptive chapters on the mountain system of the world. They are of high value as a summary of knowledge of the geology of the world up to the date at which the book was written; while M. de Margerie and his collaborators have introduced a series of footnotes, giving additional references to literature, and, in some places, incorporated important additions in the text. The descriptions are of high value, not only as a statement of facts, but for the original insight which enables Professor Suess to point out the connection of distant and now isolated areas. The author begins with a description of the Alpine system and adjoining country geologically connected with it, of the fundamental geological structure of the middle zone of Europe. He describes the main structural lines of the Alps and of the great plateau belt (the Alpine Vorland), which sweeps across Europe from the high, treeless wastes of the Spanish meseta, the chateau-crowned crags of the central plateau of France, and the pine-clad "horsts" of the Schwarzwald and Thuringia, into the level, wind-swept Russian plain. Eastward he follows the Alps into the multiserial chains of 'the world's white roof-tree' of Northern India and Thibet. South of the Alpine area he describes the subsided trough of the Adriatic, and the great basin of the Mediterranean; he shows that the latter is only the eastern arm of a long sea, which once extended from the Levant to Yucatan, and from the central part of which the Atlantic grew by the gradual enlargement of two gulfs that ran out north and south. South of the Mediterranean is the great tropical tableland; its northern part forms the deserts of the Sahara, Kordofan and Arabia; the rest forms the great block of equatorial Africa, which once probably extended eastward to Southern India and westward to join the similar eastern highlands of Brazil.

In this manner Professor Suess sketches out the development of existing continents. From the high standpoint of his wide knowledge, by the far-reaching penetration of his mental vision, he surveys all the mountains of the world; with splendid self-restraint he leaves behind him the temptation to premature theory; and he clearly brings out the essential facts upon which any theory of geographical evolution must be based. Into the apparently bewildering variety of topographic accidents he introduces harmony, bringing out the unity of structure, movement, and relations of the continental masses: thus "he draws the world together link by link." It is impossible to give any adequate idea of this work in a short summary. I feel it almost impertinent for a young geologist like myself to praise it; I can only recommend others to read it.

J. W. Gregory.

SOME NEW BOOKS

Australian Tertiary Mollusca

Catalogue of Tertiary Mollusca in the Department of Geology, British Museum (Natural History). Part I. The Australian Tertiary Mollusca. By G. F. Harris. Pp. xxvi + 407, 8 pls. London: Trustees of the British Museum, 1897. Price, 10s.

In the long series of catalogues issued by the Geological Department of the British Museum (Natural History) the Mollusca have scarcely received their fair share of attention, though they have been well treated as compared with some other groups of Invertebrata. The volume before us inaugurates a new series, destined to treat of the fossil Mollusca of different geographical regions. The first to be undertaken is Australasia, which is here regarded as including "the Australian Continent, Tasmania, New Zealand, and the Chatham Islands," and its Tertiary Mollusca present many interesting features largely due to the perfect preservation of a large variety of forms.

Dr Woodward is to be congratulated on having enlisted the services of so painstaking and philosophical a conchologist as Mr G. F. Harris for this work. To look over a collection, identify all the forms that can readily be determined, and assign new names to the others, is one mode of making a catalogue, but it is a very different and more arduous task to compare carefully all the related forms with each other, and to work out the relationships in regard to growth and development between them, so as to ascertain which are to be regarded as stages in the evolution of the individual and which as possible stages in the phylogeny of the group. Mr Harris has chosen this better part and he has had his reward, for his catalogue is no mere list of names and diagnoses, interesting only as material for further investigation—as raw material for scientific work—like too many lists. It is a text-book which must be studied by every one who wishes to be *au courant* with recent advances in the morphology and morphogeny of the shell. If some other workers would imitate his example we should soon cease to hear conchology spoken of as intellectually on a par with the collecting of postage stamps.

Let us illustrate these remarks by a few examples taken from the work itself. The names adopted for different stages of growth are in general those of Hyatt, as modified by Buckman and Bather, and it is pointed out that many characters which have commonly been regarded as differentiating genera and species are in reality indications of stages of growth or mere individual peculiarities. In the account of *Conus cuspidatus*, for example, it is shown that the elevation of the spire belongs to this latter category.

In the family Pleurotomidae, which is, comparatively speaking, of modern origin, and is remarkable for the wonderful diversity of types it has produced in a short space of geological time, there seem to be no constant criteria by which *Pleurotoma* can be separated from *Surcula*, a state of things which is not surprising to the thorough-going evolutionist. It occasionally happens "that the main features of the sculpture of the adult are foreshadowed even in the protoconch," but, on the other hand, it is by no means infrequent for them to appear but scantily even in the brephic stage, and then the ornament is usually not of a permanent character, but may be "modified at the caprice of the individual." Hence the author deduces the rule that when the scheme of ornament appears early in the course of individual development, it is a criterion for the discrimination of species, but when it does not appear till the later neanic stage, and is only completed late in life, it "merely characterises the individual, and is only of negative use for the purposes of classification."

In *Mitra multisulcata* the columellar plaits in the brephic stage number only two, whilst, as the shell grows, they increase to five, whence it is fair to conclude that in this genus at all events their number is not a matter of systematic importance. We might multiply such extracts indefinitely, but the above will suffice to indicate the nature of the work.

It is not surprising that the author has found it difficult to decide how far the beds from which his specimens came are really homotaxial with the European strata of the same name. Indeed, in considering the family Volutidae, of which he had large series of larval shells for examination, he definitely states that their development "is much more advanced than one would have expected to find in beds as old as the Eocene."

The dry but important matter of nomenclature has been conscientiously studied, and some inevitable alterations in well-known names have been introduced, but Mr Harris is to be commended for the care with which he has given the type of every genus. In conclusion, we desire to congratulate Miss G. M. Woodward on the excellence of the plates, and Messrs Stephen Austin & Sons on the typography of the volume. W. E. H.

Soils

Soils and Sub-soils from a Sanitary Point of View; with especial reference to London and its neighbourhood. By Horace B. Woodward. Mem. Geol. Survey England and Wales, 1897. Price 2s 6d.

The Director-General of the British Geological Survey is to be congratulated on an important new departure. He has not only issued a small popular handbook relating to Economic Geology, separate from the technical and purely scientific matters with which the Memoirs of the Survey usually deal; he has also succeeded in breaking through the traditions of the Public Office over which he so worthily presides, and has been able to distribute the valuable little work to the scientific press for review. The typography of the publication is also a great improvement upon most of the works issued from the same office—less battered type and the illustrations carefully printed—while the printing of the map in colours proves eminently successful.

This memoir, as the title indicates, is a practical treatise on soils and sub-soils, with special reference to London, by one of the most experienced and distinguished members of the Survey, Mr Horace B. Woodward. As the author remarks:—"The problem of choosing a place of residence exercises the minds of many whose homes are not fixed by the bonds of inheritance nor by the necessities of their mode of livelihood. In every civilised country a constantly increasing proportion of the population has to seek abode within a certain limit in or near some large town or city. Together with this aggregation of humanity around crowded centres it has slowly been realised that especial attention requires to be paid to the sanitary conditions which depend on the nature of the sub-soil. Around London, for example, the idea has become widespread that a site on Chalk, on gravel or sand, or on some other dry and porous material is to be preferred to one on clay. Increasing attention is now given to the subject by Architects and Physicians. Nevertheless, a good deal of misapprehension exists with regard to the advantages of gravel as a sub-soil, and of the disadvantages of clay; in certain circumstances either may be good, or both may be bad as sites for houses The object of the present little work is therefore to supply such information as may be needed by those who are compelled to be careful in the choice of their place of residence. All house-hunters indeed would do well to consider the general sanitary conditions connected with proposed sites, and to bear in mind that a healthy habitation depends on several considerations, apart from the nature of the sub-soil. There are the elevation of the ground and other local circumstances, and, more important still, the construction of the house itself, its damp-proof basement, its airy and sunny position, and the system of drainage. Lastly, the water supply is a question of vital importance."

Commencing with 'Greater London,' that is, practically all parishes included in a circle of 15 miles from Charing Cross, and comprising a total area of 701 square miles, Mr Woodward points out that much of the surface soil is 'made earth,' and that therefore geological maps possess but little value regarding sites for building purposes. Such maps depict an area of gravel or sand or brick-earth, while on the site of a particular row of buildings may have been an ashpit, a brickyard, or even a plague burial-ground. Thus a house standing on many feet of 'made ground' over a plague burial-ground is in a far worse sanitary condition than one built on 'made ground' over clay, or even one built on clean London clay itself.

The 'made ground' of London accumulates at from 6 inches to 1 foot in a century, and has mostly been turned up over and over again, and comprises brick-bats, dead cats, crockery, tobacco pipes, tinned-meat cans, and various rejectamenta according to the special predilections of the savages that dwell upon it. It preserves relics of the Roman occupation in abundance, and over that portion devastated, but put into sanitary order by the Great Fire of 1666, is to be seen a dark deposit of ash and burnt material to this day. At the Bank of England there are 22 feet of this 'made ground.' There may not be much danger on such a site as the Bank of England, but, as Mr Woodward points out, the iniquity of permitting building in these days on the top of pits which have only recently served as holes for decaying vegetable and animal rubbish cannot be too strongly insisted upon. And he quotes Sir Douglas Galton to the effect that builders often remove and sell the clean sand and gravel, filling in the area thus cleared with all kinds of unnameable filth, and then proceed to run up houses into which emanations from the rotting mixture underneath cannot fail to rise, and so produce the sore throat and other diphtheritic symptoms so well known to many dwellers in this great city.

Natural soils are primarily derived from the true sub-soil, which is itself the result of decomposition of the underlying rock. As a rule, it is thin, but it may be 3 feet or more in thickness. A large part of London must originally have been marsh land, but there are, according to Mr Woodward, but few areas now existing in the heart of London—these are Walbrook, Pimlico, Lambeth, Deptford, Rotherhithe and the Isle of Dogs. It is not unusual at the present day for the Thames to rise high enough to flood the lowest parts of Wapping, Deptford, Rotherhithe, Southwark and Lambeth. There is, moreover, a protective covering of 'made ground' on many of the old marsh-lands, reaching even to 6 feet in some places at Pimlico. The Grosvenor Hotel stands on 4 feet of 'made ground,' 11 feet of alluvium, and 9 feet of sand and gravel. At the new Admiralty works, it may have been observed, while the foundation operations were going on, that it was necessary to pump out the old marsh and build what was really a concrete box, so that the cellars and vaults of that structure should possess the necessary dryness for the safe storage of records. Many of these old marshy tracts have been utilised for the building of docks, while others have served for factories, gas and soap works, and other matters not requiring a great resident population. Mr F. J. Bennett has pointed out that one of the chief objections to any large population on marshy ground is the difficulty of introducing any effective system of house drainage, owing to the want of fall to carry away the sewage.

The large gravel areas around London are not always healthy. For instance, there may be a thin capping of gravel over a clay. This, of course, would lead to there always being a certain amount of water in the gravel, and so a dampness would be ever present, which would penetrate the house. Taking in descending order the higher gravels, Blackheath Beds, Bagshots, Thanet Sand, Greensands and Hastings Beds, the various pros and cons for building are discussed by Mr Woodward, and among other curious points he reminds us of a note by the late Mr Topley to the effect, that some of the Hastings Sandstones are so fine in grain as to hold up water almost as well as a clay, thus showing how many points it is necessary to consider when the choice of a site to live upon is important.

Proceeding to discuss the Woolwich and Reading Beds, Brick-earths, Clays-with-Flints, Boulder Clays, London Clay, Gault, and Weald Clays, all of which mean dampness in some degree, and some of which mean ruin in very dry seasons, as many found to their cost in the dry summer of several years ago, Mr Woodward comes to the Chalk. The Chalk seems to hold the palm for healthy and dry sites, but of course one has to be wary of gravel pipes, and must not expect to gain prizes at horticultural exhibitions, for the soil is as a rule poor, and generally unsuitable for plantations in its natural state.

Having thus considered the important interest of Mr Woodward's work, and often in his own words, let us proceed to sketch his third chapter, the one devoted to a practical and economic view of the subject. For instance, he points out that the disadvantages of living on clay are lessened by elevation when there is a good natural drainage; where the clay-tract is much broken up by coverings of gravel; where the clay has been deeply covered by artificial charges. Elevation and situation often become of more importance than original soil. Clay also prevents the water rising from strata below it, therefore, granted a good capping of gravel or "made ground," a gravel and clay soil may be much more healthy than a deep gravel through which deep-seated waters can rise in times of flood. Many more interesting topics are discussed in this chapter, and plenty of references are provided, as is the custom of the author, to papers bearing on the medical and sanitary questions likely to arise. The needs of the builder are discussed as well as those of the amateur gardener, and the views of Dr Poore on the value of creepers, such as ivy, in keeping a house dry and pure are referred to.

The subjects of water supply and drainage are necessarily important to the householder, but Mr Woodward has only been able to barely call attention to them in a pamphlet of this kind, though the admirable sketch he provides is fully amplified by footnotes to other authorities. He, however, says quite enough to wake up those corporations whose neglect of simple sanitary precautions has drawn upon them the calamitous epidemics of such recent date.

Fogs and sunshine, rain and winds, all of which exercise the Londoner, are pilloried in their turn, and their advantages or disadvantages fully set forth. It is always irritating to hear it said by one's friends—Why don't you come and live at so and so—grand morning, this morning, when I left home; and doubtless there are many who will be glad to avail themselves of the hints and advice given by Mr Woodward. Last but not least is a chapter on cemeteries.

A Geologist's Diary.

Kalender für Geologen, Palaeontologen und Mineralogen für das Jahr 1898. Herausgegeben von Dr K. Keilhack, Kgl. Landesgeologen in Berlin. 8vo, 16 × 11 cm.: vi + 130 pp. printed matter; diary; 6 cash-ruled, 64 blank, 32 section-ruled pp.; cloth, pocket-book style, with pockets. Leipzig: Max Weg, 1898. Price, 3s. 6d.

This useful and novel publication contains the following sections:—I. An account of the *personnel* and publications of the official Geological Surveys of Europe. Does not mention the new English lithographed maps, 4 in. to the mile. II. List of professors and lecturers in Geology, Palaeontology, and Mineralogy in the universities and colleges of Europe. States that the Oxford chair is vacant. III. Account of the Geological, Mineralogical and Palaeontological societies of the world. According to this the *Proceedings of the Geologists' Association* is a monthly bulletin, and the Geological Society of London publishes *Memoirs.* IV. List of German periodicals and a few foreign ones, with geological, etc., contents, in addition to those already mentioned under surveys and societies. We hope to find ourselves included in next year's issue, and would also put in a word for the

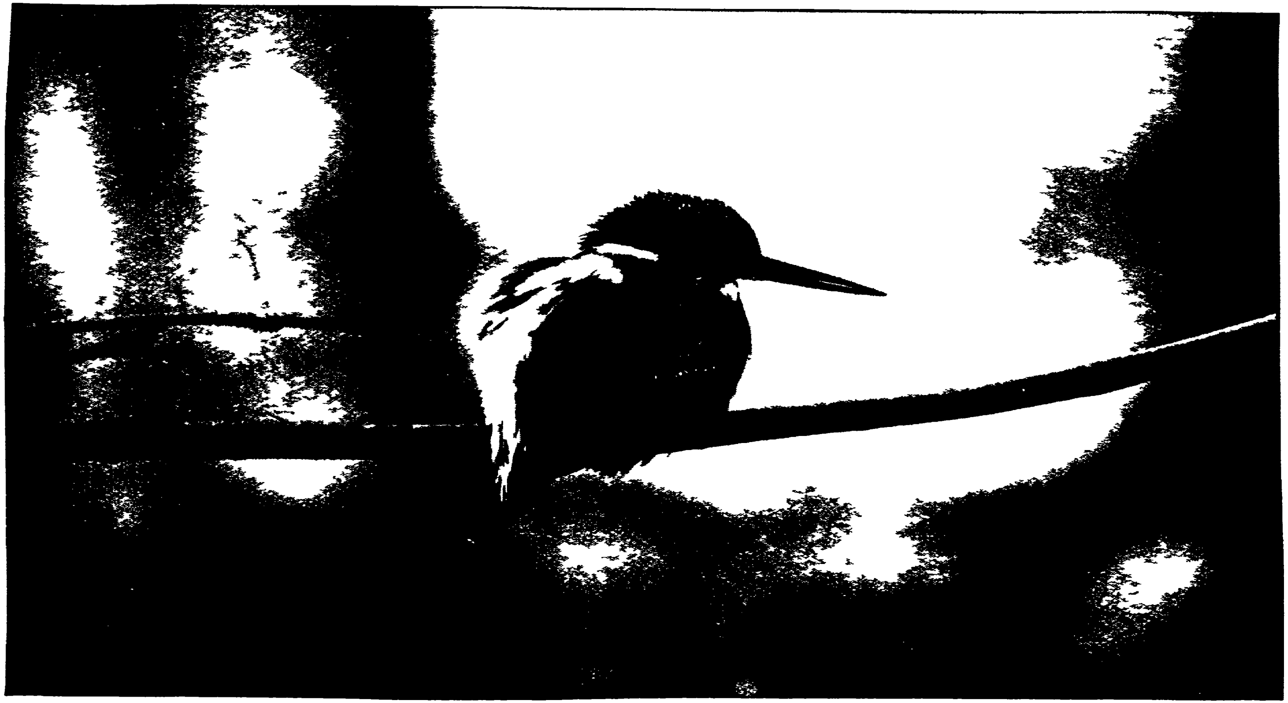
American Geologist. V. The most useful text-books and hand-books. The palaeontological list includes Gaudry's "Essai de paléontologie philosophique," and Von Schlotheim's "Petrefaktenkunde," but neither the "Paléontologie Française" nor Goldfuss' "Petrefacta Germaniae." VI. List of public and private geological, etc., collections in Germany. VII. Address-book of German geologists, mineralogists, and palaeontologists. This includes addresses of people in St Petersburg, Vienna, Holland, La Plata, East Africa, and—no, we cannot find China. VIII. Short report of the Seventh International Geologists' Congress at St Petersburg. Short it is, but finds room for many errors. IX. Table of formations, according to H. Credner. A correlation-table would have been some use, or even a detailed list of German terranes. This is useless. X. Declination-table for 1897. XI. Table of measures of length reduced to the metric system. XII. Specific gravities of many elements, minerals and rocks. XIII. Map-scales.

A smaller percentage of error, a bringing up to date, a stronger flavour of internationality, the omission of the diary and blank pages, and a reduction of price, would ensure a large sale for this publication in future years. A complete address-book of geologists comparable to Friedländer's admirable "Zoologisches Adressbuch" is a want of the day; so also is a complete list of geological periodicals, such as that which used to appear in the defunct "Geological Record." Dr Keilhack may be commended for his attempt, and recommended to try again.

Birds as they Live

With Nature and a Camera, being the Adventures and Observations of a Field Naturalist and an Animal Photographer. By Richard Kearton. Illustrated by 180 pictures from photographs by Cherry Kearton. 8vo, pp. xxi. 368. London, Paris, and Melbourne: Cassell & Co., 1897. Price 23s. 6d.

This volume has been written to show the experiences of Mr Kearton and his brother when searching for and obtaining those beautiful photographic portraits of British birds so familiar to our readers. We sincerely hope that the author and the artist have not laid the seeds of a rheumatic old age, and that one result of their joint productions will be their enfranchisement from the toil for daily bread, with the consequent advantage to science which would undoubtedly accrue. The first 134 pages of Mr Kearton's book deal with St Kilda, whither the brothers journeyed in 1895, to study, among other things, the St Kilda wren (*Troglodytes parvulus*). Of this bird he has given photographs, together with its nest, and noted the peculiarities it shows in comparison with the wren of the mainland. Puffins, fulmars, gannets, shags, and many other birds are delightfully illustrated in this part of the volume. Chapter IV. relates the author's experience of gamekeepers, their friends and foes, and is illustrated by pictures of various unfortunate animals in gins, and other traps. Chapter V. treats of nests, eggs, and young, and here it is that the photographer scores his triumphs; the beautiful little pictures of the coal-tit and the wren entering her nest are the perfection of careful and patient photography. Further chapters deal with catching song-birds on Brighton Downs, duck-decoys, and the various methods employed by the brothers Kearton in cliff-climbing and descending, and in securing their photographs.



KINGFISHER.

From a Photograph in Cherry Kearton's "With Nature and a Camera." (Cassell & Co., 1897.)

Perhaps one of the most successful pictures, and one which, by the courtesy of the publishers, we are enabled to reproduce here, is that of a kingfisher, whose portrait was obtained after six days of careful watching (Plate VIII.). The portrait of a water-vole also should be mentioned as a most successful and beautiful picture.

In a work of this kind it is extremely difficult even to point out the best things. It is a book to be bought and looked at again and again with fresh pleasure. To the field naturalists and the lover of animal life it will appeal most strongly, while to the cabinet naturalist and the museum man it will undoubtedly be a revelation.

North American Bats

A Revision of the North American Bats of the Family Vespertilionidae. By Gerrit S. Miller, jun. 8vo, pp. 1-135, pls. I. to III. North American Fauna, No. 13. Published by the United States Department of Agriculture, Washington, 1897.

Since the publication of Dobson's Catalogue of Chiroptera about twenty years ago, no work on bats has appeared of such importance to the specialist as the present paper, and this in spite of its dealing only with the members of a single family from a single region, while Dobson's grasp was world-wide. For it marks with the utmost clearness the wide difference between the methods and materials of that day and those which the most advanced and most happily situated of modern workers have at their disposal to-day. Dobson in 1878 had, apart from those he saw in other museums, seventy-nine specimens of North American Vespertilionidae to work with; Miller no less than two thousand seven hundred; while in the quality of the material there was, if possible, an even greater difference than in the quantity, owing to the perfection of the modern methods of collecting employed in the United States.

As to the difficulty of the family worked out in the present paper, we may quote Dr Harrison Allen's "Bats of North America" (1893) as follows:—"The difficulties acknowledged in identifying the American species are apparently insuperable," and although no one would accuse Dr Allen, excellent anatomist as he was, of having any special aptitude for species work (as witness Dobson, p. 329; and Miller, pp. 59, 67, 72, 84), yet no one who has tried to name American Vespertilionidae with Dobson's catalogue will deny that the difficulties really are insuperable with any material which is as yet on this side of the Atlantic. Thanks to Mr Miller's painstaking and accurate work, these difficulties have now largely disappeared.

Mr Miller recognises twenty-five species of North American Vespertilionidae, belonging to eleven genera. Twenty-one other forms are considered worthy of subspecific appellations, and a considerable increase in this number is promised when further collections of skins render apparent such geographical variations in colour as no doubt occur, but are as yet not definable owing to the great mass of the material being preserved in spirit. Neither *Natalus* nor *Thyroptera* are included in the family, for reasons which are not stated. The author has tackled *de novo* all the problems of nomenclature that have presented themselves, and he has therefore produced a work which in this respect shows a great advance on Dobson's loose way of dealing with such questions. In fact it is really thanks to Dobson, and not to Miller, that the changes from *Vespertilio* to *Myotis*, from *Vesperus* to *Vespertilio*, and from *Vesperugo* to *Pipistcrullus* will come as such a shock to older workers; for, like those since introduced by Blanford and others, they ought all to have been made twenty years ago, and we should then have had something approaching stability in bat nomenclature.

As usual in this series of publications, the get-up and illustrations are excellent, and the only criticism we have to make is that in the index some typographical indications might have been given to show which of the references is the main one, as when one wants the general account of a species, it is annoying to be referred first to the historical list of names, then to the general list of North American species, then to the synoptical tables, and perhaps last of all to that which one most often wants, the main account of the species.

O. T.

British Butterflies and Moths

The Lepidoptera of the British Islands. By Charles G. Barrett, F.E.S. Vol. iv. Heterocera, Noctuae. 8vo, pp. 404. London: L. Reeve & Co., 1897. Price, 12s. (large paper edition, with 48 coloured plates, £3, 3s.)

It is satisfactory to note that the successive volumes of Mr Barrett's work appear at ever-decreasing intervals. The publication of the fourth volume less than a year after the third is highly creditable, and raises hopes that the entire work may be issued within a reasonable time, though the magnitude of the task which Mr Barrett has set himself would daunt a less industrious and painstaking worker. It is impossible to help regretting the space devoted to the long detailed descriptions of species so well known and so often described before. Could these have been curtailed, the work would have gained in portability, cheapness and rapidity of publication.

The present volume gives us an account of ninety-five species of owl-moths classed by Mr Barrett in thirty genera. The agrotid section is concluded with *Axylia* (doubtless rightly placed here), *Triphaena* and *Noctua;* after which *Eurois* (comprising the species *adusta, herbida, occulta* and *satura*) leads on to those genera which in the old Guenéean classification were placed in the two separated "families" Apameidae and Hadenidae. For adopting a new arrangement of the noctuid genera, Mr Barrett deserves the warmest thanks of all progressive entomologists, even though some of his generic associations may not stand the test of further research. If the great genus *Agrotis*, as understood by Lederer and Hampson, is to be subdivided, our "yellow-underwings" should surely be distributed into two genera, since the front tibiae are unarmed in the broad-bordered, and spined in the narrow-bordered group. Mr Barrett's genus *Mamestra* includes only the species *brassicae, albicolon* and *persicariae.* This last-named moth is, however, held by most authorities to be congeneric with the species for which Mr Barrett retains the name *Hadena*, while *brassicae* with its hooked fore-tibiae is well worthy of generic separation. No one will object to Mr Barrett's division of the old genus *Aplecta* of British entomologists into one genus (*Eurois*) with the naked-eyed species, and another (*Aplecta*) not very nearly allied including the species with hairy eyes. The removal of *cespitis* from the genus *Luperina* also seems to be well warranted; Mr Barrett places it with *popularis* in his genus *Heliophobus.* It is to be regretted that no authorities are given for the generic names, nor is any justification advanced for such changes in nomenclature as that just mentioned, *Heliophobus* in our current lists being assigned to the species *hispidus,* for which Mr Barrett uses the generic name *Ulochlaena.*

A more serious fault, mentioned in the notice of previous volumes, is the absence of any synoptical table of generic characters. In a few cases in the present volume Mr Barrett has given such a table of the species in a genus, but, as a rule, a beginner desiring to name a moth from the plain edition of this work would have to plod steadily through all the descriptions. The summaries of the varietal forms of each species are comprehensive and valuable, and in his descriptions of the caterpillars the author has followed the best authorities. The pupa, however, which is worth careful study, is in most instances passed over with the most meagre notice. The distributional facts regarding each species are stated in detail, and a valuable feature is a summary of the foreign range in addition to the British. A few moths are mentioned—as *Miselia bimaculosa* and *Xylophasia zollitoferi*—of which one or two examples only have been taken in England. Mr Barrett is inclined to explain such occurrences by supposing artificial introduction. But it seems at least as likely that the species are really indigenous, although of extreme rarity, and only able to hold their ground in a few scattered localities which have so far escaped the vigilance of the rapacious collector.

GEO. H. CARPENTER.

SERIALS.

The American Forestry Association has taken over *The Forester* as its publishing organ.

The Plant World was started last autumn somewhere in the United States. We have not yet seen a copy or been furnished with any details.

Our weekly contemporary, *Garden and Forest,* has ceased publication on the close of its tenth volume, having failed to find a paying public in the United States. Those who have paid will undoubtedly regret this decision.

Prof. O. Taschenberg has been succeeded as editor of the weekly journal, *Die Natur,* by Prof. Willi Ule, under whose auspices we may look for the continuance of a career that has already been successful for forty-six years

The Bausch & Lomb Optical Co., of Rochester and New York, published on January 1 the first number of their monthly journal, which is devoted wholly to the microscope, its use, methods of working, fixing, mounting, &c. This is the first paper of its kind published in the United States.

We have received the annual bound volume of our interesting monthly contemporary, *Knowledge,* for 1897. It would form an admirable gift-book for anyone interested in scientific studies. It is well illustrated, and the subjects dealt with in popular language cover a wide field in science, literature, and art.

The first volume of the *Bibliographia Geologica*, just issued by the Geological Survey of Belgium, gives about 2000 titles of works and articles in periodical reviews published in 1896-1897. The second volume (in the press) will give, besides the complement of 1896-1897, all published in 1898, at the time of the issue of this volume.

Five more Lieferungen (30-34) of Simroth's new edition of the Molluscan portion of Dr Bronn's "Klassen und Ordnungen des Thier-Reichs" are now out, and contain pp. 177-224, with plates vi.-ix. The anatomy of the Prosobranchs is concluded, and the principal features of the shell and operculum described.

Mr A. COTGREAVE, chief librarian of the West Ham Public Libraries, sends us sections 1 and 2 of "A Contents-Subject Index to General and Periodical Literature." The sections are composed of two small 8vo sheets, and deal with subjects from Aar River to Arromanches. There are 7 entries under 'Apples,' 3 under 'Anglesey,' and 1 under 'Apoplexy.' The only references to 'Ammonites' are Ewald's "History of Israel," and Gillett's "Ancient Cities." We infer that the Index is intended merely as a help to those who desire a superficial knowledge, *e.g.* leader-writers.

FURTHER LITERATURE RECEIVED.

INTRODUCTION to Organic Chemistry, J. S. Wade: Swan Sonnenschein. Phenomena of Nature, James Walker: Swan Sonnenschein. Tutorial Chemistry, Part II. Metals, J. H. Bailey: W. B. Clive. Lehrbuch der Vergleichenden Mikroskopische Anatomie der Wirbelthiere, Albert Oppel: G. Fischer. Die Farnkrauter der Erde, H. Christ: G. Fischer. What is Life? or Where are we? Whence did we come? and Whither do we go? Frederick Hovenden: Chapman & Hall.

Presidential Address to the Conchological Society, J. Cosmo Melville. An Unusual Phyto-bezoar, W. Trelease: *Trans. Acad. Sci. St Louis*. *Halticus bractatus*, F. M. Webster: *Entom. News*. Present and Future of Applied Entomology in America, F. M. Webster: *Assoc. Econ. Entom.* North American Lemnaceae, C. H. Thomson: *Ann. Rep. Miss. Bot. Gardens.* Additional Notes on the Turbellaria of the L.M.B.C. District, H. Lyster Jameson: *Trans. L'pool. Biol. Soc.* List of Generic and Family Names of Rodents, T. S. Palmer: *Proc. Biol. Soc. Washington. Proc. Biol. Soc. Washington, Vol. XI.*, pp. 241-282. Crustacea of the Plankton, E. A. Birge: *Trans. Wisconsin Acad. Sci.* On the Plankton collected continuously during two Traverses of the North Atlantic, etc., W. A. Herdman, J. C. Thompson, Andrew Scott: *Trans. L'pool Biol. Soc.* Bull. Inst. Internat. Bibliogr. On Crystalline Structure in Gold and Platinum Nuggets and Gold Ingots, A. Liversidge: *Proc. Roy. Soc. N.S. Wales.* Records of the Australian Museum. Proc. Roy. Soc. Victoria, *Vol. X.* A Bibliography of Norfolk Glaciology, including the Cromer Cliffs and Forest Bed Series, W. Jerome Harrison: *Glacialists' Mag.* Shooting Times, Jan. 15. Do the Crystalline Gneisses Represent Portions of the Original Earth's Crust, Joseph Lomas: *Geol. Mag.* The North American Species of Diaptomus, F. W. Schacht: *Bull. Illinois State Lab. Nat. Hist.* Characters, Congenital and Acquired, G. Archdall Reid: *Science.* Catalogue of British Birds in the Collection of Mr E. M. Connop, Rollesby Hall, Norfolk, T. Southwell.

Amer. Journ. Sci., Jan.; Amer. Geol., Dec.; Amer. Nat., Dec.; Botan. Gaz., Dec.; Feuille des Jeunes Nat., Jan.; Illinois Wes. Mag., Dec.; Irish Nat., Jan.; Journ. School Geogr., Dec.; Knowledge, Jan.; Literary Digest, Dec. 18, 25, Jan. 1, 8, 15; Naturae Novit., No. 23; Naturalist, Jan.; Nature, Dec. 23, 30, Jan. 6, 13, 20; Nature Notes, Jan.; Naturen, Dec.; New Age, Dec.; Photogram, Jan.; Rivista Quind. Psichologia, vol. i. fasc. 16; Revue Scientifique, Dec. 18, 25, Jan. 1, 8, 15; Science, Dec. 10, 17, 24, 31, Jan. 7; Scientific American, Dec. 11, 18, 25, Jan. 1; Scot. Med. and Surg. Journ., Jan.; Victorian Nat., Nov., Dec.; Westminster Rev., Jan.

OBITUARIES

NICOLAUS KLEINENBERG

Born 1842. Died November 11, 1897.

If there has been any reference to the death of this accomplished morphologist in any English scientific journal, it has escaped my notice at least. And yet Professor Kleinenberg was committed to the ground in Naples in the early days of last November, having succumbed to angina pectoris at the age of fifty-two.

His name deserves an honoured place in the memories of British morphologists, if only in consideration of the fact that he was, in his time, a close friend of Balfour. Moreover, he was among those who had realised that Huxley could have been a great man, even if Darwin had not existed.

My excuses for attempting an appreciation of Kleinenberg are, firstly, that no one else appears to have done so in this country, and secondly, that some years ago I had the privilege and pleasure of being the recipient of kindness and attention from him.

Kleinenberg's published works were not numerous, but they were choice. Perhaps their essential characteristic may be looked for in their notable capacity for opening new and important vistas to the morphologist.

It is a sad, but undeniable, fact that in the estimation of naturalists in general the "mere morphologist" does not occupy a very high place. Nevertheless, there is not much to choose between a zoologist without morphological knowledge and a morphologist without zoological knowledge. That both species occur is lamentably more than probable.

Kleinenberg was a naturalist who devoted himself largely to morphology. His work on *Hydra* (1872) may certainly be said to rank already as a classic, and its worth as an example of high morphological research to be cited for the emulation of later investigators is not diminished by the fact that his neuro-muscular theory has been superseded, firstly, through the discoveries inaugurated by Golgi, and, secondly, through the discovery of independent nervous elements in *Hydra* (*cf.* Camillo Schneider, "Histologie von Hydra, etc." *Archiv. für mikr. Anat.*, vol. xxxv., 1890).

With what may be described as fatal consistency, Kleinenberg appears to have remained faithful to his famous neuro-muscular theory to the end. In his work on the development of *Lumbricus trapezoides* (1879) the remarkable twinning of the embryo will be [illegible] by all, while his account of the separate origin of the supraoesophageal ganglion and the ventral ganglionic chain is worthy of particular note. His more recent *magnum opus* on *Lopadorhynchus* (1886) is a veritable mine of suggestive morphological speculation of first-rate importance. His discovery of the circular nerve below the praeoral trochal ring in the Annelid Trochosphere-larva, and his comparison of it with the circular nerve of a Medusa, may perhaps be attributed to an effort of genius. Kleinenberg's recognition of the probability of the ✠ shaped 'Anlage' of the mesoderm of the Ctenophora indicating the former occurrence of tentacles in the transverse plane, in addition to those which actually occur in the sagittal plane, is only one other instance of his morphological acumen.

The memoir on *Lopadorhynchus* appeared at a time when that aspect of thought which may be denominated Haeckelism was more or less in the ascendant—in other words, when morphology was, for the time being, bound by the fetters of the gastraea theory, and the coelom theory, and the germ-layer theory. It should not be necessary to add that it is farthest from my thoughts to depreciate the value of these most useful and indispensable theories. But no one can deny that it is very important to guard against the possibility of theory becoming dogma. Therefore, when a man of Kleinenberg's *timbre* boldly proclaimed "Es giebt kein Mesoderm," and, furthermore, undertook to substantiate his position by what will ever remain a masterpiece of embryological research, the effect could only be salutary as giving morphologists pause.

With regard to the specific assertion quoted above, Kleinenberg's conclusions have no doubt had to submit to important modifications (*cf.* Ed. Meyer, "Die Abstammung der Anneliden," *Biol. Centralb.*, vol. x., 1890, p. 296; and E. B. Wilson, "The Origin of the Mesoblast Bands in Annelids," *Journ. Morph.*, vol. iv., 1891, p. 205). That the value of a man's work is not, however, necessarily to be measured so much by its intrinsic, theoretical accuracy as by the effect which it has upon the subject as a whole and upon thought, is amply demonstrated by the vicissitudes to which even the Theory of Natural Selection is constantly exposed in the minds of naturalists.

That Kleinenberg himself smarted under the restrictions imposed by the too dogmatic laws and principles of certain authors seems to be revealed in his perhaps rather impatient *mot*, "Loch ist Loch in der ganzen Welt."

If his wholesale repudiation of the mesoderm cannot be upheld, there can be no doubt as to the lasting value of his principle of substitution as applied to the interpretation of embryological data.

Long before the appearance of Wilson's admirable work on *Amphioxus* (E. B. Wilson, "On Multiple and Partial Development in *Amphioxus*," *Anat. Anz.*, vol. vii., 1892; also, "*Amphioxus* and the Mosaic Theory of Development," *Journ. Morph.*, vol. viii., 1893), Kleinenberg was conscious of the fact that the so-called mesodermic pole-cells of Amphioxus had no definite existence, and his scepticism as to the independent rights of the mesoderm may well have been enhanced by the conviction that structures of such fundamental significance, from the point of view of the germ-layer theory, in such an important type as Amphioxus, were mythical.

The "New Morphology" has not yet come to birth, but when it does see the light, its ancestry will probably be traced to such a man as Kleinenberg. ARTHUR WILLEY.

THOMAS JEFFREY PARKER

Born October 17, 1850. Died November 7, 1897.

By the premature death of Prof. T. Jeffrey Parker of Dunedin, biological science loses one of its most brilliant exponents. The eldest son of the late W. Kitchen Parker, he came early under the influence of Huxley, his father's most intimate friend; and a year after his completion of a distinguished student's career in the Royal School of Mines, in 1872, he returned at Huxley's special invitation to be his Demonstrator. Here Parker remained until 1880, taking an unusually active part in the re-organisation of biological teaching, which was at that time being brought about by his Professor. He was then appointed to the Chair of Biology in the University of Otago, Dunedin, New Zealand, and since that date he has been the most striking figure in the biological world of Australasia. After leaving Europe, Parker still continued to elaborate the methods of teaching he had formed when under Huxley's guidance, and the results were given to the world in his well-known little text-book, "A Course of Instruction in Zootomy (Vertebrata)," published in 1884, and "Lessons in Elementary Biology," of which the first edition appeared in 1891, while we reviewed the third only a few months ago. At the time of his death, it is melancholy to relate, he had only just completed the revision of the proof-sheets of the forthcoming "Manual of Zoology," by Professor Haswell and himself. On reaching New Zealand, however, Parker at once began to investigate the animal life of that part of the world; and many will remember the fine series of fish skeletons (including a shark and a skate, prepared by a new process) sent by him to the International Fisheries' Exhibition in London, 1883. The results of his researches were published in numerous papers and memoirs, contributed chiefly to the publications of the Royal and Zoological Societies of London and to the Transactions of the New Zealand Institute. Among them may be specially mentioned those on the blood-vessels of the sharks, the skeleton of the ribbon-fish (*Regalecus*), the skull of the Moas, and on the anatomy and development of *Apteryx*. These works are illustrated chiefly by the author's own drawings, and are models of what technical scientific memoirs should be. There is the mark of thorough and conscientious research throughout them all; the facts are always marshalled on a laboriously-conceived plan most conducive to clearness, and there is always a cautious reserve in formulating generalisations, such as is rare in most modern exhaustive writings. Biological science has indeed lost a master craftsman, of whom it had expected great things yet for many years to come.

GARDINER GREENE HUBBARD

Born at Boston, Aug. 25, 1822. Died at Washington, Dec. 11, 1897.

The Hon. G. G. Hubbard, son of a doctor of laws, studied law at Cambridge, and was called to the bar in 1843. He practised his profession in Boston for twenty, and in Washington for five years, when he retired and devoted the remaining twenty years of his life to matters of wider public interest. His name is particularly connected with the development of the 'oral speech' method for the teaching of the deaf; and it was through his instrumentality that this method was accepted and officially recognised by the U.S. Legislature. With Alex. Graham Bell he was founder of *Science*. To him also is largely due the introduction of the telephone to the whole world. When the U.S. National Geographic Society was organised, Mr Hubbard became its first president, and in that capacity founded the *National Geographic Magazine*. He is succeeded in the office by A. Graham Bell. Of late years he took much interest in a scheme for greater coöperation between the scientific institutions of the American Government.

SAMUEL A. MILLER, who died on December 19, at Cincinnati, O., aged sixty-one, had contributed very largely to the literature of American palaeontography. The cause of science in Cincinnati was much advanced by his energy. In 1874 he founded and edited the *Cincinnati Quarterly Journal of Science*. This was superseded in 1878 by the *Journal* of the Cincinnati Society of Natural History, of which Dr Miller was ever a prominent member. With a keen eye for the differences between specimens, and a facile pen, he introduced an enormous number of new species in all classes of fossil Invertebrata, but chiefly among the Crinoidea. His work in this direction has been severely criticised by some and scornfully ignored by others; but it should be remembered to his credit that his descriptions were invariably accompanied by figures, which is more than can be said of some naturalists who think themselves his superiors. His really valuable work was the index to the fossils of North America, of which three editions and a supplement were published. This catalogue is one of the most useful books that a working palaeontologist can possess, and though the science and etymology are occasionally open to criticism, still the bibliographic references are rarely incorrect, and this too is more than can be said of many writers with far greater opportunities than ever were possessed by this busy lawyer in the heart of America. F. A. B.

THE death is announced of Professor ERNST LUDWIG TASCHENBERG the well-known entomologist. Born in 1818, he was appointed inspector of the Zoological Museum at Halle in 1856. He was the author of many works on entomology, which, apart from several treatises on hymenoptera, are written in popular form, and deal mostly with obnoxious insects. His first paper appeared in 1857, and his "Was da kriecht und fliegt" in 1861.

The following deaths are also announced:—At Coolgardie, WILLIAM ERNEST POWELL GILES, the Australian explorer, who was awarded the founder's medal by the Royal Geographical Society for having, between 1874 and 1875, twice traversed the W. Australian desert from Adelaide to Perth; Dr WILHELM JOEST, the explorer, in Australia; CHARLES CORNEVIN, professor of hygiene and zoötechny in the Veterinary School of Lyons; RAPHAEL SLIDELL FREIHERR V. ERLANGER, professor of zoology at Heidelberg University, on Nov. 29, aged 33; IVAN OTTO PLEKARSKY, keeper of the Zoological Cabinet of St Petersburg University, at Jelissawetgrad, in January, aged 30;

ALEX. THOMINOT, ichthyologist and herpetologist, and preparator at the Museum of Natural History, Paris; GEORG BERTHELIN, a student of the fossil molluscs and foraminifera of the Paris Tertiaries; J. W. DUNNING, lepidopterist, at Leeds, aged 64; MORRIS YOUNG, entomologist, at Paisley, aged 76; R. MONTROUZIER, entomologist, at Saint Louis (New Caledonia), on May 16th, 1897, aged 76; JAMES BATEMAN, the author of the great monographs on the Orchidaceae of Mexico and Guatemala (1837-1843), and on *Odontoglossum* (1864-1870), at Worthing, on Nov. 27, aged 86; EDM. J. BAILLIE, botanist, at Chester, aged 47; the Rev. CHARLES SAMUEL POLLOCK PARISH, botanist, at Roughmoor (Somerset), on Oct. 18, aged 75; Dr FRIEDRICH OSKAR PILLING, pomologist and author of a text-book of botany, at Altenburg, on Nov. 22, aged 73; Dr FRIEDRICH WILHELM SNYDER, botanist, at Braunsberg, aged 87; J. B. v. KELLER, botanist, in Vienna, on Nov. 17.

NEWS

THE following appointments are announced :—

Jinta Hara, to be professor of zoology in the Agricultural College of Sapporo, Hokkaido, Japan ; Masamuru Inaba, of Mayebashi, to be professor of zoology in Yamaguchi, Japan ; Dr Asajiro Oka, to be professor of zoology in the Higher Normal School of Tokyo ; Dr W. Ophülo, assistant in Göttingen University, to be professor of pathological anatomy in the University of Missouri ; Dr Elisha Gregory, Jr., to be instructor in histology and embryology in Harvard Medical School ; Dr E. G. Lancaster to be professor of psychology and pedagogy at Colorado College ; Dr W. H. R. Rivers, of St John's College, Cambridge, to be University lecturer in experimental psychology; T. Strangeways Pigg to be demonstrator in pathology in Cambridge University ; W. L. H. Duckworth of Jesus College, to be lecturer in anthropology at Cambridge University ; "a person named Bowers, from Martinsburg, W. Va.," to be Fish Commissioner of the U.S.A. ; J. G. Luchman, to be Government Botanist of Victoria ; Dr Alex. Henchel, of St Petersburg, to be assistant in the Botanical Institute of Odessa University ; Miss Julia Snow, Ph.D. (Munich), to be instructor in botany in Michigan University ; Edgar R. Cumings of Cornell University, to be instructor in geology in Indiana University ; C. Knocker to be assistant in the Bristol Museum.

SIR WILLIAM FLOWER has been elected an Honorary Associate of the Belgian Academy of Sciences.

THE British Medical Association will hold its sixty-sixth annual meeting at Edinburgh from July 26-30.

MR A. C. HARMSWORTH has lent the *Windward* to Lieut. Peary for his expedition to the North Pole.

A COLLECTION amounting to £9000 has been made towards the erection of a Science Hall for Syracuse University.

A FISHERIES Exhibition will be held next summer in Bergen, and a similar one in Aberdeen in the summer of 1899.

A FIRE recently destroyed the entire valuable collection, made by Domingo Sanchez, in the Museum of Manilla, Philippine Islands.

RICHMOND COLLEGE, Virginia, has received a gift of £1000 from Mrs Geo. Smeltz, of Hampton, Va., towards the cost of a Science Building.

DR WALDEMAR LINDGREN, of the U.S. Geological Survey, is to deliver a course of lectures on metallurgy and mining at Stanford University.

MR WOLF has been elected President of the Paris Académie des Sciences for 1898, and Mr Van Tieghem, the botanist, succeeds him as Vice-President.

THE valuable collection of Peruvian antiquities, made by Mr Nicholas Saenz, has been purchased by the National Museum of Santiago, Chili, for £1400.

THE sixteenth annual meeting of the American Forestry Association was held at Washington on December 8th. Its summer meeting will probably be held at Boston.

THE Sixth Anniversary of the Harvard Natural History Society was celebrated on December 17, when Professor Shaler gave an account of the History of the Society.

DR KARL FUTTERER, professor of geology in the Technical High School in Karlsruhe, has started on a journey to Central Asia for the purpose of geological research.

THE Zoological Museum at Rome has received from Messrs Lepri and Patrizi a collection of many hundred birds, including several rare specimens, from the surrounding country.

BY the will of E. Czaban, a Warsaw merchant, the Warsaw Academy of Sciences has received a sum of over £7000; and the Universities of Cracow and Lemberg £4000 each.

THE late Col. Theodore Lyman, well known as the author of the 'Challenger' Report on the Ophiurids, gave his scientific library to the Museum of Comparative Zoology at Harvard.

THE U.S. Coast Geodetic Survey are on the point of issuing a new map containing the Yukon River and most of its tributaries, including the Klondike region, on a scale of twenty miles to the inch.

PROF. ANGELO HEILPRIN of Philadelphia has organised an expedition for the scientific exploration of Alaska. He will have a stern-wheel steamer of steel, in ten sections, and of exceptionally light draught.

THE American Society of Naturalists and affiliated societies, which met at Ithaca in December last, will probably meet next winter in New York, at the same time as the Geological Society of America.

BY the will of the late Sir Thos. Elder, of Adelaide, S. Australia, the Zoological and Geographical Societies of that city received £2000 each; and the Medical School of the University, £20,000.

ON condition that Dr Lundstrom be the first to receive the appointment, Franz Kempe of Stockholm has given £9000 for the establishment of an Associate Professorship of Physiological Botany at Upsala University.

A ZOOLOGICAL Society of Western Australia has been founded, with a zoological garden in South Perth. The director of the latter is E. A. Le Souef, son of the well-known director of the Melbourne Zoological Garden.

A FUND has been established in Victoria to erect a permanent memorial to the late Baron F. von Muller. The Secretaries are Prof. Baldwin Spencer and Mr W. Wiesbaden. It is hoped to provide funds for a statue and a scholarship.

THE Triennial Cuvier prize of 1500 francs "for the most remarkable work on the Animal Kingdom or on Geology," was, at the session of the Académie des Sciences on December 13, 1897, awarded to Prof. O. C. Marsh of Yale University.

THE Trustees of the British Museum have decided that the evening opening of the Bloomsbury Institution is a failure, and in future the Museum will be open to the public from 10 a.m. to 6 p.m. on week day evenings all the year round.

THE Biological Survey of Alabama, established a year or so ago, has added to its herbarium during 1897 over 20,000 specimens. It is intended to sell sets of exsiccatae at about thirty shillings a hundred, and to offer some specimens as exchanges.

The Bill for the establishment of the University of the United States, first introduced by Senator Edmonds in 1890, has again been introduced into both Houses of Congress. It is expected that the bill will be passed during the present session.

We learn from the *Shooting Times* that an artesian well has been constructed in the Zoological Gardens supplying 240,000 gallons per diem from the Chalk, at a depth of 450 feet. Hitherto there has been no independent water supply for the London Gardens.

The Walsingham Medals for biological research, at Cambridge University, have been awarded to V. H. Blackman, Hutchison Research student of St John's and of the Botanical Department of the British Museum, and W. Morley Fletcher, Fellow of Trinity.

We have already chronicled one Russian expedition that is starting for Abyssinia, we have read in the papers of a political mission, and now we learn of two geographical parties, under the command of Lieut. Baron Pellenberg and Mr Vlasof, which have just left Odessa for Abyssinia.

The *Shooting Times* says that the protection of the sea-otter is occupying the attention of the American Government as much as that of the fur-seal. New regulations have been made for 1898, and it is proposed, if these do not succeed, to forbid hunting from schooners and to institute a shore-limit of fifteen miles.

At a meeting of the Lincolnshire Science Society on December 10th, Mr J. H. Cooke, one of the vice-presidents, gave a lecture on "Prehistoric Man in Lincolnshire," in the course of which he gave a compendious account of all of the relics of the Neolithic Age that have been found to the present time in Lincolnshire.

Mr Walter Wellman, the journalist, will lead an expedition to Franz Josef Land next June, for which he has purchased the sealer 'Laura.' He will travel northward over the islands, encamping for the winter at the most northerly point attained in the season of 1898. In the following year he will make an attempt to reach the North Pole.

The Association française de Botanique has acquired as its organ *Le Monde des Plantes*, for many years edited by Mr Léveillé, of Mans (56 Rue de Flore). The Association is intended to take the place of the Société française de Botanique, which ceased to exist in 1895. It is intended, says *La Feuille des Jeunes Naturalistes*, to form a central herbarium and library, free to members, and to undertake the exchange and determination of specimens.

The entomological collections, books, and instruments of the late Dr Geo. H. Horn are bequeathed to the American Entomological Society, as also a present endowment of $200 per annum, and a subsequent endowment of $5000 at the death of his sister. Dr Horn left also to the Philadelphia Academy of Natural Sciences $1000, and $500 to the American Philosophical Society; neither bequest to be received until his sister's death.

The Colonial Museum at Marseilles, opened in 1893, is remarkable for its valuable collection of tropical vegetable products. These are studied and analysed under the direction of Mr Heckel in the museum laboratory. Among recent acquisitions may be mentioned Dr Buisson's collection of the mollusca of Tahiti, botanical collections from New Caledonia, presented by Messrs Heckel, Jeanneney, and Col. Pelletier, and from the Antilles by R. P. Duss.

THE following are among the lectures to be delivered at the Royal Institution before Easter. On Fridays, at 9 P.M. :—Jan. 21, Sir John Lubbock on Buds and Stipules ; Jan. 28, Prof. Lloyd Morgan on Instinct and Intelligence in Animals ; Feb. 18, Prof L. C. Miall, A Yorkshire Moor ; March 18, Mr Jas. Mansergh, The bringing of Water to Birmingham from the Welsh Mountains : a series on "The Simplest Living Things" will be delivered by Prof. E. Ray Lankester on the following Tuesdays at 3 P.M. :—Jan. 18, 25 ; Feb. 1, 8, 15, 22 ; March 1, 8, 15, 22, and 29.

THE State Museum, Vienna, received the following collections during 1896: — Eppelsheim collection of Coleoptera, including more than 2000 species or 26,000 specimens of Staphylinidae ; the Gustav Mayr collection of Hemiptera, including 1350 species or 5500 specimens ; the Bergenstamm collection of Diptera, including 3000 species or 45,000 specimens ; the Steindachner collection of fish, chiefly from the Red Sea, 3400 specimens representing 702 species. The collection of geological photographs in this Museum then numbered 1892, while the ethnographic photographs were 5477.

THE New York State Science Teachers' Association held its second annual meeting at Ithaca on December 30 and 31. The following papers were read and discussed :—"Physical Laboratory Work in Secondary Schools," by Miss Mary E. Dann of the Girls' High School, Brooklyn ; "The Use of the Lantern in Science Teaching," by the President, Professor Nichols ; "The Teaching of Botany in Secondary Schools," by Professor L. M. Underwood of Columbia University ; and "Out-door Science Work in Secondary Schools," by Principal F. A. Vogt of Buffalo. The afternoon discussions were held on science teaching in Schools and Colleges.

AN International Exhibition of Photographic Apparatus and Photographs, organised by the Royal Photographic Society, will open at the Crystal Palace on April 27th. In addition to the usual displays of pictures, the leading firms, manufacturers and dealers will be largely represented. There will also be extensive loan collections, illustrating not only the history of photography, but its scientific and commercial applications, photo-mechanical processes, photographs in colours, photographs by means of the X-rays, and kindred exhibits. The arrangements are in the hands of a joint committee of members of the Society and exhibitors.

THERE has, says *La Feuille des Jeunes Naturalistes*, lately been formed la Société pour la diffusion des Sciences physiques et naturelles et de leurs applications, specially intended for the cultivation of natural science in schools through the establishment of school museums by means of—1, Gratuitous determination of objects of natural history ; 2, publication of exchange-lists ; 3, the establishment of a library of scientific works to be lent gratuitously to its members ; 4, the sale of teaching collections at a cheap rate ; 5, the publication of a journal containing special communications, and questions, and correspondence from its members. The Secretary is J. Courjault, Saint-Genis (Charente Inf.).

THE Australasian Association for the Advancement of Science began its seventh session at Sydney on January 6. Prof. Liversidge combined the office of president and general secretary. The following are the names of some of the presidents of sections and the titles of their addresses :—Prof. F. W. Hutton, of Geology and Mineralogy, "Early Life on the Earth"; Sir James Hector, of Geography "Submarine Geography"; A. W. Howitt, of Ethnology and Anthropology, "On the Origin of the Aborigines of Tasmania and Australia"; R. M. Johnston, of Economic Science and Agriculture, "The comparative share of consumable wealth actually appropriated or absorbed by the various agencies engaged in its production"; and the Hon. Allan Campbell, of Sanitary Science and Hygiene, "Aspects of Public Health Legislation in Victoria." The late Prof. T. J. Parker was to have been president of the Biological Section. Numerous papers were read. Prof. W. Baldwin Spencer lectured on the "Centre of Australia," and Sir James Hector on "Antarctica and the Islands of the Far South." An "At Home" was given by Miss Macdonald at the University on January 11, and on January 13 an excursion started for Lord Howe Island. An address of congratulation to the Queen was forwarded during the meeting.

THE following prizes were among those awarded at the Annual Public Session of the Académie des Sciences at Paris, on January 10:—

Prix Bordin (3000 fr.), to Prof. G. Pruvot of Grenoble, for his researches on the bottom and fauna of the G. of Lyons and the entry to the English Channel; *Prix Delesse* (1400 fr.), to Daniel Oehlert of Laval, for his numerous and important studies on palaeozoic faunas; *Grand Prix* (3000 fr.), the subject of which was new investigations and experiments on high mountain regions, especially as regards meteorology and the conditions of life, to Joseph Vallot, founder of the observatory near the top of Mt. Blanc, and the scientific explorer of many of the highest summits in the Alps and Pyrenees; *Prix Desmazières* (1600 fr.), to Prof. Jacob Eriksson, of the Swedish R. Academy of Agriculture, for memoirs on the smut of grain; *Prix Montagne* (1500 fr.), to Prof. Bourquelot of Paris, for works on the physiology of mushrooms; *Prix Thore* (200 fr.), divided between Louis Bordas for researches on the poison-glands of Hymenoptera, and Sappin-Trouffy for observations on the part played by the nucleus in the development of Uredineae; *Prix Barbier* (2000 fr.), to A. T. de Rochebrune for his work "Toxicologie Africaine"; *Prix Godard* (1000 fr.), to Messrs Beauregard and Boulart for their researches on the genito-urinary organs of Cetacea; *Prix Bellion*, 1000 fr. to Aug. Pettit for work on the supra-renal capsules, and 400 fr. to Mr Péron, for researches on tuberculosis of the lungs; *Prix Montyon* (750 fr.), to Mr Delzenne for his works on the coagulation of blood; *Prix la Caze* (10,000 fr.), to Prof. Rontgen; *Prix Gay* (2500 fr.), to Charles Flahault for his study of the geographical distribution of the French Mediterranean flora; *Prix Petit d'Ormoy* (10,000 fr.), to Prof. J. Gosselet for his geological writings.

CORRESPONDENCE

WASPS

I AM grateful to Mr A. O. Walker for his letter (*Natural Science*, vol. xii., p. 72). I see no difficulty in the wet of the winter of 1896-7 affecting the hibernating queens. The wet period that I specified included the autumn months of 1896 and the early spring months of 1897—*i.e.* the months during which the queens are searching out winter quarters and those in which they are endeavouring to find resting sites. In both of these occupations they are fully exposed to atmospheric influences. I have previously (*Natural Science*, vol. vi., p. 178) mentioned that I have seen a wasp on the wing as early as February 7th, and I may now add that I saw several as late as November 18th, 1897. I am fully prepared to admit that while actually in hibernation the rain, probably, has no direct effect upon the queens. I have shown in the article last referred to that cold does not kill hibernating wasps, and this opinion is held by many farmers with whom I have conversed upon insect pests in general. The year 1889, quoted by Mr Walker, does not appear to me in any way opposed to my theory, but rather to support it. Mr Walker's summary shows that the temperature and rainfall of March 1889 were not exceptional. April was 2·5° F. below average temperature, with a rainfall 2·61 inches above average. The low temperature would probably keep the queens hibernating and prevent them starting the risky business of nest-formation; in other words, probably the queens were not on the wing until the rainy period was over. The temperature of May was 1·7° F., and the rainfall 0·86 inch above average—not a very serious amount. There is no doubt that a high temperature acts favourably on wasp-colonies: Janet has proved this beyond dispute, and it is equally possible that a low summer temperature is unfavourable though it could hardly ever become actually destructive.

I can offer no explanation of the exceptional occurrence in Mr Walker's house early in September. It would be interesting to know if a nest was near the house, and also if there was anything going on, such as fruit preserving, which would strongly attract the wasps indoors. I fancy that in any year wasps may be found fairly plentiful in the windows of pastry cooks' shops and similar sugar-laden spots.

OSWALD H. LATTER.

CHARTERHOUSE, GODALMING.

REPRODUCTIVE DIVERGENCE

IN the last month's number of *Natural Science*, Dr Karl Jordan adversely criticises the further demonstration I gave of a theory which I had suggested in a former number of the Journal (vol. xi., p. 181). I do not propose, however, to again encroach on your space at any length as I have been assured by several zoologists of standing that on the one hand my demonstrations of the correctness of the theory appeared to them quite sound and convincing, whilst on the other hand they were unable to comprehend the objections raised by Dr Jordan. The arithmetical part of the proof has also been worked through again by myself and independently by a friend, and found correct. I therefore think that the question of the validity or otherwise of my arguments, and of those brought forward by Dr Jordan, may be safely left to the judgment of those readers of *Natural Science* who are sufficiently interested in the subject to read through carefully what has been written on both sides.

I wish to take the present opportunity of saying that if any mathematician should think it worth his while to attack the problem, I should be exceedingly obliged by his so doing. Thus it seems to me possible that, given the amount of variation originally present, and also the degree of infertility between the various individuals, one might be able to express by means of a general formula the amount of divergence taking place after a given number of generations.

H. M. VERNON.

THE ZOOLOGICAL STATION, NAPLES.

THE DOCTRINE OF UNIFORMITY

ALTHOUGH I quite agree in the main with Sir H. Howorth's strictures in the January number anent "the great fetish of the modern geologist, the doctrine of Uniformity," nevertheless I fail to see what scholasticism and metaphysics have got to do with the matter. Nor would I pronounce anathema on "the scholastic and ridiculous *à priori* prejudices of superior men," provided that they were the offspring of true scientific ideas and intelligence. That, indeed, is the most important point. In the country of

Shakespeare and Milton there is always room for doubt or suspicion with respect to the so-called scientific fads, fetishes, hypotheses, &c., which are broached and maintained with bold and aggressive rhetoric by superior ruling men. The principal question to be settled is, whether these fads, &c., are based on true science, or whether their origin may be assigned to a sort of philosophy of the universe or of things in general, which any superior man possessed of a certain order of mind or kind of talent is bound to hold and maintain? Thus, for example, the poet Goethe, in the course of his dabbling in scientific subjects, was known to seek by intuition the bond which unites the diverse parts of a natural object, and thuswise to imagine his 'ideal type,' which permitted him to re-unite all the animal kingdom into one harmonious whole, although infinitely varied. We learn also that Wordsworth hated science, because it loses the principle of unity and continuity. Tennyson also has given marked expression to the pantheistic conception of the unity of life, as, *e.g.*, in his 'flower in a crannied wall.' Certain French men of science aver that the doctrine of evolution is more studied in England than elsewhere. Why this is so it is not easy to comprehend, unless it be that the honest evolutionist is really a sort of poetical pantheist like Goethe, and seeks to introduce a unity into everything, and to solve the mysteries of the universe by means of synthesis, going perhaps as far as Lord Kelvin when he talks of 'failure,' because he had not succeeded in merging effects into causes and causes into effects, and making a unity wherein thought itself would disappear. Such being the pronounced views expounded by various poets and literates, is it to be wondered that many of our eminently rhetorical scientists have been infected therewith? The geological doctrine of Uniformity seems to be a case in point. The aphorism that "the forces of nature have always been in quality and quantity what they are now" chimes in very harmoniously with the poetic philosophy of the unity of life and substance and everything else. Persons who are afflicted with a baffled desire for unification, and make the unification of life and the physical forces a special feature in their scientific postulates, are not necessarily scholastic metaphysicians or sound *a priori* reasoners. A truly scientific metaphysician who holds to the doctrine of pantheism as a more scientific dogma, and does not practically bear it out in the region and area of actual investigation and research, is, of course, placed in a different position altogether. It is therefore the poets, and not the metaphysicians, who ought to bear the blame in what Sir H. Howorth condemns.

P. Q. KEEGAN.

PATTERDALE, WESTMORELAND,
January 14th, 1898.

A BRIGHTON correspondent sends us a copy of the *Church and Household* (Brighton) for November, containing an amusing specimen of Natural History as "she is wrote." A caricature of the argonaut and a drawing of a common lobster figure respectively as a 'pearly nautilus' and a 'scorpion,' while the accompanying letterpress is of about the same standard of trustworthiness. It is surely time that the rudiments of Natural History had penetrated the mind even of the uneducated penny-a-liner; but alas! this worried mortal is still in the dark ages.

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.

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In addition to Notes and Comments on the progress of Natural Science, brief Reviews of Current Literature, Obituary Notices, and News of Universities, Museums, and Societies, the last number of this Journal comprised the following specially contributed Articles:—

No. 72, February 1898.

I. The Classification of the Day Butterflies (*concluded, with Plate II.*). By A. Radcliffe Grote, M.A.

II. The Geographical Distribution of the Actiniaria of Jamaica. By J. E. Duerden, B.Sc.

III. The Authenticity of Plateau Implements (*with Plates IV.-VII.*). I. By Rev. R. Ashington Bullen, M.A., F.G.S. II. By W. J. Lewis Abbott, F.G.S.

IV. Suess's Theories of Geographical Evolution. By J. W. Gregory, D.Sc., F.G.S.

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NOTES AND COMMENTS

Governments and Fisheries

THE American Society of Naturalists has presented to President McKinley a strongly worded resolution, intended as a protest against the dreaded appointment to the Commissionership of Fish and Fisheries of a politician without scientific attainments. The matter has been warmly taken up by the scientific press of America, and scientific men all the world over will sympathise strongly with this protest. There is hardly a country nowadays that does not recognise to the full the importance of a truly scientific study of the sea and its inhabitants. If anything is to be done to improve the condition of the fisheries, all laws and practical suggestions are bound to have a scientific basis. But while we see every country of Europe investigating the conditions of its seas, or its lakes and rivers if it has no seas - while this course is being pursued by our colonies, and by the rising nation of Japan, and while America has hitherto been, and doubtless will continue to be, one of the leaders in this work, it is surprising as well as disappointing to learn that the Government of Egypt, in which our own country at present takes so large a share, should quite recently have thought it advisable to dispense with scientific control of their fisheries and to hand them over to an utterly inexperienced coast-guard department. We are in no way concerned with individuals, but we do not imagine that even the Egyptian Government will deny that in Dr J. C. Mitchell they had one who was, by his scientific training and practical experience, thoroughly qualified for the post of scientific and technical adviser to the fishery administration; and we are glad to be able to publish in this number a sketch of the fisheries of Egypt from the pen of that gentleman. ...t totally apart from the hardship to Dr Mitchell, in being ...prived of an important post at five weeks' notice, seeing all ...s plans for the improvement of the Egyptian fisheries suddenly cut short and his researches on the fish fauna of the Lower Nile put an end to, we must express our decided opinion, which we are quite sure will be the opinion of all our readers, that in taking this step the Government of Egypt is not behaving fairly to its subject population. With this population fish ranks higher than flesh, and an enormous saving to the scattered wealth of the country could easily be made in a very short time by a properly equipped department. Our position in Egypt will hardly be strengthened in the eyes of the world by such retrograde actions as that of which we now complain.

Two Views of Physiology

The Fullerian Professorship of Physiology at the Royal Institution was recently vacated by Professor Waller since he was unable to obtain a laboratory, without which he declared neither the investigation nor the teaching of physiology could be prosecuted. This has not prevented Professor E. Ray Lankester from accepting the same chair. Professor Lankester, to judge from the first of his lectures on "The Simplest Living Things," now being delivered at the Royal Institution, holds broader ideas as to the meaning of the term physiology. He eases his conscience by interpreting it in its old sense, for which we now generally use the term biology. He is more than right for there is no doubt that physiology to-day i becoming less the study of functions of specialised organs and mor a study of the properties of living matter itself, as exemplified the lowliest of organisms.

The view that we have just expressed was in fact strongly emphasised by Dr J. Loeb at the recent meeting of the American Society of Naturalists. His address is printed in the *Botanical Gazette* for January, and we hope our excellent contemporary will pardon us if we make copious extracts. "Living matter is a collective term for the quality common to all living organisms. Comparative physiology alone enables us to discriminate between the general properties of living matter and the functions of specific organs, such as the blood, the nerves, the sense organs, chlorophyll, etc. Nothing has retarded the progress of physiology and pathology more than the neglect of comparative physiology. Comparative physiology shows that secretion is a general function of all living organisms and occurs even where there is no circulation. Hence it was *a priori* false and waste of time to attempt to explain secretion from the experiments on blood pressure. Oxidations occur regardless of circulation, and it was *a priori* a waste of time to consider the blood as the seat of oxidation. Comparative physiology has shown that the reactions of animals to light are identical with the heliotropic phenomena in plants. Hence it is a mistake to ascribe such reactions as the flying of the moth into the flame to specific functions of the brain and the eyes. Sleep is a phenomenon which occurs in insects and plants, and it would be waste of time to attempt an explanation of sleep on the basis of phenomena of circulation. The best interests of physiology and pathology demand that the systematic development of comparative physiology be one of the physiological problems of to-day."

STEREOCHEMISTRY AND PHYSIOLOGY

THE chemical changes that take place in connection with life and the gradual organisation of living matter are phenomena that the physiologist of to-day is trying hard to connect. In his attempt he may be aided, says Dr Loeb, by two series of facts: "(1) The fact that phenomena of fermentation lead to an increase in the number of molecules and thus bring about an increase of osmotic pressure in the cells, this increase of osmotic pressure being the source of energy for the work of growth: (2) the facts of heteromorphosis, *i.e.*, the possibility of transforming in certain animals one organ into another or substituting one organ for another through external influences such as gravitation, contact, light, etc."

"The exact and definite determination of life phenomena which are common to plants and animals is only one side of the physiological problem of to-day. The other side is the construction of a mental picture of the constitution of living matter from these general qualities. In this portion of our work we need the aid of physiological chemistry and especially of three of its theories—stereo-chemistry, Van't Hoff's theory of osmotic pressures, and the theory of the dissociation of electrolytes. We know that the peculiar phenomena of oxidation in living matter are determined by fermentative processes, and we venture to say that fermentations form the basis of all life phenomena. It has been demonstrated that fermentability is a function of the geometrical configuration of the molecule. *Saccharomyces Cerevisiae* is a ferment for such sugars only as have three, or a multiple of three, atoms of carbon in the molecule. Among the hexaldoses only δ-glucose, δ-mannose, and δ-galactose are fermentable, while their stereo-isomers are not fermentable. But the influence of the geometrical configuration goes farther. Voit has suggested and Cramer has demonstrated that there is a far-reaching parallelism between the fermentability and assimilation of carbohydrates. Higher animals as well as yeast cells are able to form glycogen from such carbohydrates as are fermentable by yeast. The further development of these stereochemical relations and their extension to proteids and nucleins is another of the problems of physiology which will contribute to the main problem, the analysis of the constitution of living matter."

Physiologists who may desire to take up the study of this fascinating but intricate subject may be glad to know that Messrs Longmans, Green & Co. have just published a second edition of Dr Eiloart's translation of Van't Hoff's work, "The Arrangement of Atoms in Space." This deals with the stereochemistry of carbon and of nitrogen compounds, and contains an appendix by Professor Alfred Werner of Zurich, on the stereochemical isomerism of inorganic compounds. This edition has been thoroughly revised and brought up to date, and contains a full index. The price is six shillings and sixpence. Recommendation of such a classical work is superfluous.

Osmosis and Animal Electricity

The passage of fluids through thin membranes, known as osmosis, deserves even more attention than it has hitherto received, since it enables us to understand many vital processes that would otherwise seem to be inexplicable on purely physical or chemical grounds. Again, to quote Dr Loeb, "Van't Hoff's theory of osmotic pressure permits an application of the law of conservation of energy to a class of phenomena to which this law was hitherto inapplicable, namely, the phenomena of growth, functional adaptation, secretion, absorption, and even pathological processes, such as oedema. The physiologists who thought that the blood pressure determined secretion could not understand why secretion took place under a higher pressure than the blood pressure. Comparative physiology shows that secretion does not depend upon circulation, and the theory of osmotic pressure indicates that the osmotic pressure in the cells is more than twenty times as high as the blood pressure. The work of secretion is done by osmotic pressure and not by blood pressure. A prominent physiological chemist has become a vitalist because he could not explain why the secretions differ from the blood from which he thinks they are formed. He overlooks, among others, the fact that the protoplasm possesses the quality of semi-permeability, which means that it allows certain substances to pass through and others not."

"The theory of the dissociation of electrolytes is of fundamental importance in the analysis of the constitution of living matter. Pharmacology will feel its influence most directly. Everything seems to indicate that the specific physiological effects of inorganic acids are due to the number of positively charged hydrogen ions in the unit of solution, and the specific physiological effects of alkalies to the negatively charged hydroxyl ions. But the universal bearing of the theory of dissociation upon physiology will perhaps be best seen in the field of animal electricity. An active element of living matter in a state of rest is negatively electric to its surrounding parts. We may assume that an acid is formed in the active part, and that the passive parts are neutral. The positive hydrogen ions of the acid have a much greater velocity of migration than the anions. Hence the former will diffuse more rapidly into the passive tissue than the anions, and the active tissue will remain negatively charged."

Martian Morphology

WHAT relief, after wading through the tenth text-book of zoology or the twelfth primer of geology, to open a parcel from Mr Wm. Heinemann, and to find that it contains that interesting romance "The War of the Worlds," by H. G. Wells! Our best thanks to Mr Heinemann for a really pleasant evening. Considering that the staple of an up-to-date novel is criminal psychology or sexual pathology, it is strange that we receive so few. But there is little of the advanced novel in this thrilling tale by our English Jules Verne. Everyone by this time knows the story of the attack on London by the strange beings shot from Mars: this we need not recapitulate. We are more interested in the anatomy and physiology of the Martians themselves.

Some of our readers may remember that in September 1894 we were led by the strange light seen on the southern edge of Mars (which Mr Wells explains as due to the casting of the huge gun), to speculate on the kind of beings, and especially the reasoning beings, that might have been evolved did protoplasm exist on the red planet. We did not enter into such details as the trained scientific imagination of Mr Wells enables him to do, but perhaps we do not lay too flattering an unction to our soul in supposing that our hints formed the basis of the novelist's more vivid conception.

A big, greyish, rounded bulk, about 4 feet in diameter, with an integument glistening like wet leather, a face, or rather a facial area with immense dark-coloured eyes devoid of brow ridges, no nostrils, and below them a V-shaped mouth with pointed upper lip, its wedge-like brim, unsupported by a chin, incessantly quivering and dropping saliva. And then below this, sixteen slender, almost whip-like, tentacles arranged in two groups, "since named by that distinguished naturalist Prof. Howes, the hands." At the back of its head or body is a single tight tympanic surface serving as an ear, the internal viscera consisting chiefly of a brain sending enormous nerves to the eyes, ear, and tentacles. The mouth opens not into a stomach, but into lungs, the heart and its vessels being the only other internal organs. Such is Mr Wells' Martian, "a horror and a monstrous prodigy." The Martian, having no stomach, cannot eat, much less digest: instead he injects the living blood of other creatures into his own veins. He never sleeps, having no extensive muscular mechanism to recuperate. The Martians are without sex, and their young arise by budding. We are also interested to find that Mr Wells' Martians are, as we suggested, deaf "to what we hear, and realising their environment in ways inscrutable to us," although it must be admitted that in this point our author is not wholly consistent.

Another piece of inconsistency lies in the black smoke, which on page 145 is said to contain "an unknown element giving a group of four lines in the blue of the spectrum," whereas on page 298 the same element shows "a brilliant group of three lines in the green." This is a little fact that Mr Wells might have made certain of without any trouble, for, as many little details prove, he is well acquainted with various branches of science. It was, for instance, an ingenious idea to suppose the Martian machines constructed on the plan of human joints and muscles, while assuming the entire absence of that distinctively human invention—the wheel. There are no bicycles in Mr Wells' Mars.

Our Microscopic Allies

We believe it is not a new suggestion that the red colour of Mars is due to the red chlorophyll of the herbage, but the introduction of the Red Creeper and the Red Weed affords a detail not merely ghastly in itself, but, preparing the way in an artistic manner for the *dénouement* which, as all our readers are aware, is the succumbing of the Martians to the bacteria of this earth. This conclusion, both from an artistic and scientific point of view, strikes us as the best thing in the book. In the paragraph in which Mr Wells explains the end of the Martians, he expresses the place and functions of bacteria so admirably that we cannot forbear quoting it: "There were the Martians—dead!—slain by the putrefactive and disease bacteria against which their systems were unprepared; slain as the Red Weed was being slain; slain, after all man's devices had failed, by the humblest things that God, in His wisdom, has put upon this earth.

"For so it had come about, as, indeed, I and many men might have foreseen, had not terror and disaster blinded our minds. These germs of disease have taken toll of humanity since the beginning of things—taken toll of our pre-human ancestors since life began here. But by virtue of this natural selection of our kind, we have developed resisting-power. To no germs do we succumb without a struggle, and to many—those that cause putrefaction in dead matter, for instance—our living frames are altogether immune. But there are no bacteria in Mars, and directly these invaders arrived, directly they drank and fed, our microscopic allies began to work their overthrow. Already when I watched them, they were irrevocably doomed, dying and rotting even as they went to and fro. It was inevitable. By the toll of a billion deaths man has bought his birthright of the earth, and it is his against all-comers: it would still be his were the Martians ten times as mighty as they are. For neither do men live nor die in vain."

Life without a Stomach

Although the Martians, and perhaps our remote descendants, may be able to live without stomachs, yet the possession of that organ is usually reckoned a necessity for human life at the present day. It is therefore surprising to learn that the entire stomach of a living woman affected by a tumour was removed by Dr Carl Schlatter of Zurich at the beginning of September last, and the woman is still alive and well. Attempts made at St Louis and Milwaukee to repeat the operation on other subjects have not proved successful. At the same time the success of the original operation has shown that the stomach is a less essential organ than is usually imagined. Vomiting took place some time after the removal of the organ, in some instances to a considerable extent, suggesting that the remaining portion of the duodenum was becoming distended as a new receptacle for food. Dr E. C. Wendt, writing in the *Medical Record* (New York, Dec. 25), points out among the conclusions to be drawn from this operation that: (1) The fluids and solids constituting an ordinary mixed diet are capable of complete digestion and assimilation without the aid of the human stomach. (2) A gain in the weight of the body may take place in spite of the total absence of gastric activity. (3) The general health of a person need not immediately deteriorate on account of removal of the stomach. (4) The most important office of the human stomach is to act as a reservoir for the reception, preliminary preparation, and propulsion of food and fluids. It also fulfils a useful purpose in regulating the temperature of swallowed solids and liquids.

A modern Menenius can no longer show the folly of the strike of the members, since the members can readily reply that if the belly continues to give itself airs they will simply chuck it out.

The Ripening of Cheese

In our February number (p. 77) we had some remarks about ferment action which will enable our readers to appreciate the importance of a paper by Messrs S. M. Babcock and H. L. Russell, in the fourteenth *Annual Report* of the Wisconsin Agricultural Experiment Station (Dec. 1897). The ripening of cheese is usually supposed to be due to the action of bacteria present in the milk from which the cheese is made. The green or unripe cheese is hard, elastic, and insoluble, being opaque in thin sections. As it ripens it becomes softer, more soluble and translucent, while its nutritious constituents are rendered more soluble and therefore more readily digestible. The decomposition of the proteids produces in the mature cheese albumens, albumoses, peptones, amido products (tyrosin, leucin) and ammonia. The ascription of these changes to bacteria is supported by the great development of lactic acid bacteria in hard cheeses, while the experimental elimination of those organisms has seemed to confirm the idea that they were closely connected with the ripening. The American authors, however, have been investigating the enzymes or unorganized ferments of milk, and conclude that these alone are sufficient to produce a series of decomposition-changes similar to those found in ripening cheese, even when all bacterial ferments have been carefully excluded. The changes that occur were proved to be of a non-vital character and undoubtedly due to enzymes. These enzymes, however, may themselves have been produced by bacteria originally present in the milk, or they may be inherent in the milk itself. That the latter is the case was shown by securing milk drawn from the cow with great care, and treated immediately with antiseptics; for this freshly secured milk underwent identical changes. It was found possible to separate enzymes having a curdling action on the milk, as well as those with digestive functions. It is therefore the belief of the authors that the ripening of hard cheese under normal conditions is caused by the joint action of organised ferments (bacteria) and unorganised ferments (enzymes). The breaking down of the casein is due rather to the enzymes than to the lactic acid bacteria.

After Mr Wells' magnificent testimonial, quoted above, this is rather a come-down for the bacteria. At the same time, it does not seem probable that the lovers of Gorgonzola, Camembert and Stilton, least of all "Limburger käse," will be anxious to purchase their cheese sterilised: for our authors still consider that in the production of the characteristic flavours bacteria plays a somewhat important part.

Sleep Movements

The sleep movements of the leaves of certain plants noticed by Linnaeus form one of the many subjects of investigation by Charles Darwin, who gave an account of his interesting experiments and observations in his book on the "Movements of Plants." If we compare the day and night position of the leaves of clover, Canadian bean, or *Robinia*, we see that the result, effected by somewhat different movements, is the same, namely, to expose as little as possible horizontal leaf-surface. In the bean and *Robinia* the leaflets hang more or less vertically, in clover they are arranged in a neat little packet. The view taken by Darwin, and generally held since, is that the object of the movement is to prevent an excessive radiation of heat. The leaf is necessarily a delicate structure, functioning as the seat of the most important life-processes of the plant, and it is easy to conceive the danger of damage from excessive loss of heat by radiation to a clear sky if the leaf retained at night its horizontal day position.

In a recent paper (*Botanische Zeitung*, vol. lv., 1897, p. 71), Stahl criticises this view, and suggests that leaf-movements are associated with the giving up of water-vapour (transpiration). The object of transpiration is the maintenance of a current of water carrying in solution food-stuff absorbed from the soil by the root up to the leaves, and the sleep-movement, far from being a sign of repose, is an adaptation for enabling this process to go on by night as well as by day. In most plants the apertures from which the water-vapour escapes, or stomata, are closed all night and transpiration is reduced to a minimum. Stahl finds, however, that in cases where the leaves show the nyctitropic movement the stomata remain open. The object of the vertical position seems to be to prevent deposition of dew on the leaf, and merely to remove an effective obstacle to transpiration of water-vapour. In some plants the night position is also assumed in very strong sunlight. There the action has the opposite effect, namely, to check transpiration and remove the danger arising from excessive loss of moisture. It recalls the permanent vertical position of the leaves of many Australian Mimosas, Eucalyptus and Hakeas, which are exposed to intense sunlight. Stahl suggests that the spontaneous up and down movements of the leaflets of the American telegraph-plant (*Desmodium gyrans*), are to promote transpiration, the motion causing a continual removal of the saturated air near the leaf, and the shivering of the leaves of the aspen and many poplars may have the same meaning.

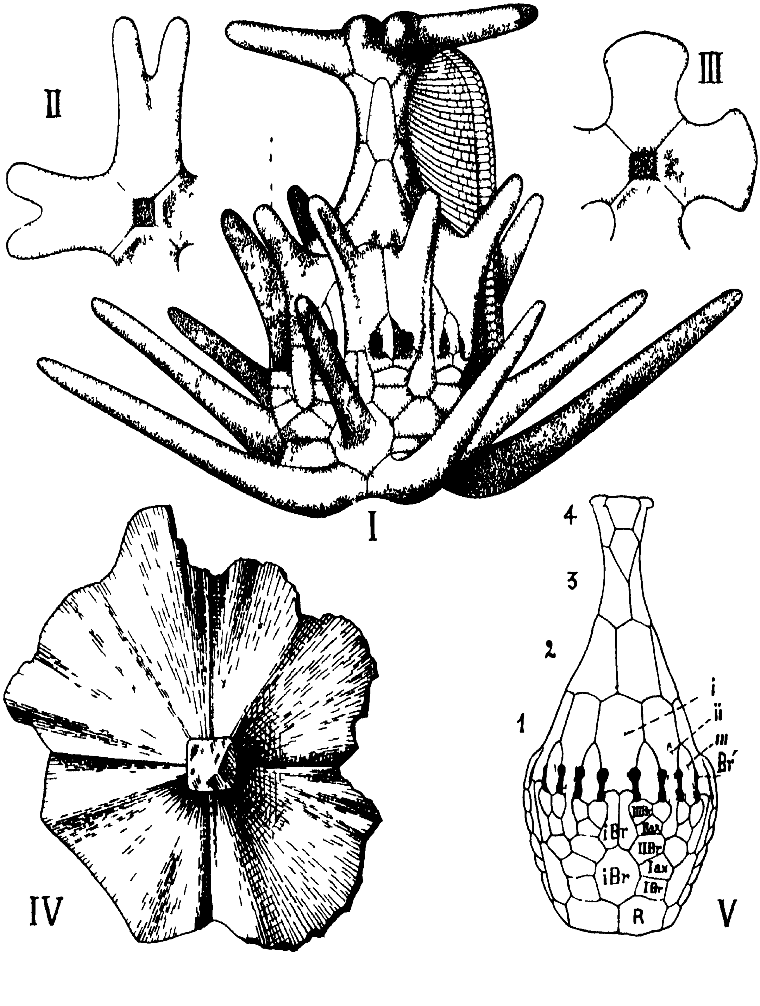
TURGIDITY

IN connection with the amount of water in plants, the experiments of Mr E. B. Copland on "the relation of nutrient salts to turgor," described in the December number of the *Botanical Gazette*, are of interest. Since the work of Pfeffer and others on the hydrostatics of the plant-cell, and the demonstration of the great internal pressure set up by osmosis, the turgidity of the cell has been an important factor in the explanation of plant-growth and movement. As in the case of other newly-discovered causes, it was at first made responsible for too much, and views of the relation between turgor and growth have materially changed within the last ten years. The experiments now before us were undertaken to ascertain the influence of various chemical elements on the turgor of the plant-cell. The results were however almost entirely negative. Potassium alone showed any direct influence, its removal as a food-substance being followed by a diminution of turgidity.

Potassium presented in solution to the roots of plants causes the cells of both root and stem to show a higher turgor than they do when it is replaced by sodium. From the analysis of the sap, the writer concludes that the influence is a direct one. When offered to the root, the potassium salt is taken up and stored in the cell-sap, where it becomes an important part of the osmotically active material which keeps the cell and plant turgid. It is difficult to understand this decided insistence of the plant for potassium, and the uselessness of the far more plentiful and physically lighter sodium.

The Riddle of Cryptodiscus

In 1864 Professor James Hall figured a peculiar fossil from the Niagara Limestone, apparently of Wisconsin, and gave it the name *Cryptodiscus* (Fig. IV). No notice was taken of this, and it was not so much as mentioned by Mr S. A. Miller in the various editions of his catalogue of North American fossils. Mr Miller himself, however, in 1892 figured a portion of a similar fossil from the Niagara Limestone of Indiana, indicating it as "? ? ? ? ? ? ?." In the *Journal of Geology* (Vol. V., No. vii., pp. 744-751), published at the close of 1897, Mr Stuart Weller, of the University of Chicago, describes and figures four different forms of the so-called *Cryptodiscus*, from the dolomitic Niagara Limestone of Joliet, Ill., and applies to them as many specific names. No diagnosis of *Cryptodiscus* is given, so that it is a little out of place for Mr Weller to say that Hall's name was "never properly published." The fossil may, however, be described as composed of four plates, each roughly forming the quadrant of a circle, opposed by their straight edges, meeting around the centre in a downward prolongation of square or sub-circular section, in which a central canal is left, and often forked or extended in several branches on their outer margins. Hall regarded *Cryptodiscus* as "the calyx of a Crinoidean ? of a new and peculiar type"; but Mr Weller's interesting article is largely devoted, as its title indicates, to proving that the fossils represent "the casts of the gastric cavities of medusae." We are spared the trouble of criticising this not very plausible suggestion, through a footnote added by Mr Weller on sending the paper to press. Specimens in the collection of Mr E. E. Teller, of Milwaukee, "seem to establish the fact that *Cryptodiscus*



A FOSSIL CRINOID

Fig. I.—Restoration of *Call[illegible]* [illegible], after Angelin. One arm remains on the right, the others having been removed. Fig. II.—The circlet of plates around the arms seen from above. Fig. III.—The same circlet in *C[illegible]*, after Angelin. Fig. IV.—*[illegible]*, after Hall's original figure. Fig. V.—*Callicrinus* [illegible], with the arms removed. Br′, the place of attachment of the arms; i, ii, iii, processes, some of which form the middle circlet of large spines in *C[illegible]*. The drawings are by Mr. G. C. Chubb, and are larger than natural size.

is a remarkable disk-like expansion of the four plates forming the terminal ring of the anal tube of some crinoid, probably *Callicrinus*." We have no doubt whatever that this is the correct interpretation. *Callicrinus* (Pl. IX., fig. V.) is a genus that does not differ in essentials from *Eucalyptocrinus*, and its presence in America has only recently been established by Wachsmuth and Springer. Its remains occur in the Niagara Limestone, associated with *Cryptodiscus*, and we would specially point to *C. cornutus*, Hall sp., as likely to be connected with "*Cryptodiscus*." At any rate, no one acquainted with the *Callicrini* of Europe and their strange anal expansions can doubt that *Cryptodiscus* represents the same structure; we may refer Mr Weller to Angelin's "Iconographia Crinoideorum" (Stockholm, 1878), plate xxiv., figs. 25, 26, *Callicrinus costatus*; plate xxviii., figs. 14, 15, 16, *C. murchisonianus*; and fig. 18, *C. koninckianus* (Figs. I., II., III.).

Since the above was written, the December number of the *Journal of Geology* has come to hand, and in it is Mr Weller's fuller description of Mr Teller's specimens, this time as *Callicrinus*. There is, however, nothing of importance to add to what has just been said. Mr Weller may be congratulated on the promptness and frankness with which he repairs an error, but it is a pity that his editor could not manage to eliminate his first paper; for, though the value of his figures and careful descriptions is undiminished, the specific names must eventually prove synonyms of those already given to the calyces. Our American friends like to be in a hurry, and Mr Weller has not even taken time to give either the measurements of all his species or a statement of the relative size of his figures, or the dates and page-numbers of the passages he quotes. Nor can one tell from his figures whether the objects are concave or convex. There is at this moment a wide opening in America for a new student of fossil echinoderms, but unless one appears who will turn out far more careful work than has hitherto been published, we shall not welcome him with any rapture.

Pettifogging Science

When we spoke of "our American friends," we did not mean to imply that there were no such offenders in other parts of the world, nor did we forget that America too has its earnest workers. We are urged to this explanation by an admirable address from a leading American biologist, published in *Science* for January 14. Dr C. O. Whitman, who, it is interesting to note, is connected with the same University as Mr Stuart Weller, writes as follows: "The avalanche of modern biological literature consists too largely of scrappy, fragmentary, disconnected products of a multitude of investigators, all working as so many independent individuals, each snatching whatever and wherever he can, and then dumping his heterogeneous contributions into the common hodge-podge. How are we ever to extricate ourselves from such appalling confusion? The ambition to be prolific rather than sound is a peril against which we seem to have no protection at present. And yet, if I mistake not, there is a growing sentiment against such traffic in science, which will eventually make it plain that ambition in that direction spends itself in vain. A dozen or more dumps a year, with as many or more retractions, corrections and supplements, is only a modest-sized ambition. Conclusions are palmed upon the unsuspecting reader, and then, without compunction or apology, reversed from day to day or from month to month, or, worse still, in an appendix subjoined, so that it may be seen how little it costs to be prolific when one day's work cancels another."

Dr Whitman casts about for a remedy. He sees that every worker cannot have Darwin's industry and reserve, but he thinks that something might be done by training students in laboratories to work on definite problems in co-ordinate groups, "each performing his mite in conjunction and relation with the others of his group." Such a method of work would, he points out, be of advantage not only to science but to the workers themselves, for instead of working in a cloistral and jealous seclusion, they would be brought into active and mutually helpful relations, and enabled to draw from one another the best that each could give. This happy result is to be brought about in great measure by combining instruction with investigation in the various scientific laboratories. We grant that teaching is as valuable to the teacher as to the taught, but we are a little doubtful whether this introduction of socialism into scientific investigation would not tend to check individual enterprise and to slacken actual advance by taking away the spur of competition. The remedy, it seems to us, is to encourage so far as possible the idea that one solid piece of work is worth more than a dozen driblets, to look with suspicion on the prolific pamphleteer, and to continue to cast ridicule on that worse than ridiculous nuisance—the preliminary noticer. Let us insist upon exactness of description, accuracy of drawing, correctness of reference, lucidity of style, in short, upon all those qualities that go to stamp a scientific monograph as classical.

THE PLACENTATION OF PERAMELES

THE full account of Mr Hill's discovery of a true allantoic placenta in the marsupials *Perameles obesula* and *nasuta*, published in the *Quart. Journ. Micro. Science* (vol. xl., pp. 385-446, plates 29-33), is one of the most important of recent contributions to zoology. The facts are startling in themselves; they are described luminously, illustrated by beautiful figures, and they raise a number of novel questions. Mr Hill has been able to collect material consisting of six stages—some from *Perameles obesula*, some from *P. nasuta*, but the processes in the two cases he declares to be so similar that he was able to take them consecutively without reference to specific difference. The uterine wall prepares itself for the attachment of the embryo in a fashion unknown in other mammals. In the normal way, the mucosa hypertrophies; the uterine glands enlarge; the interglandular connective tissue forms a loose network of cells permeated by lymph and the blood-vessels enlarge. But the superficial epithelium loses the outlines of its cells and becomes a syncytium, which increases in thickness by the multiplication of nuclei and protoplasm, and the blood capillaries penetrate the syncytial protoplasm until they form a network upon and just under the surface.

The embryo attaches itself to this syncytium by means of enlarged ectoderm cells over the discoidal area of true chorion, with which the allantois fuses. These ectoderm cells become resorbed, and the allantoic capillaries grow down deeply into the syncytium forming interdigitations with the maternal capillaries, the two sets of vessels being separated only by their endothelial walls and a little syncytial protoplasm. Thus a true, discoidal, allantoic placenta is formed. It is non-deciduate; at birth not only is there no loss of maternal tissue but an area of the allantois remains attached to the syncytium, and gets absorbed by maternal leucocytes. In addition to the allantoic placenta, there is a temporary yolk-sack placenta, somewhat annular in character, and functional before the allantoic placenta is ready.

Mr Hill discusses the bearing of his discovery on the relations between marsupials and Eutheria. He rejects the supposition that the allantoic placenta of Perameles is a convergent but independent structure. Like Semon and Hubrecht, he lays weight on the functionless degeneracy of the allantois in marsupials generally, and on the degenerate character of the milk dentition of marsupials. He inclines to the belief that Eutheria and Metatheria are parallel branches from a common stock, which possessed a discoid, allantoic placenta.

A Fresh Mammoth

We learn from the *Révue Scientifique* that Mr K. Nossilov, editor of the *Novoye Vremya*, has discovered an entire mammoth in Ia-Mala Peninsula in the country of the Samoyedes. In May 1897, he arrived at the mouth of the river Jouribey, and learned that two years previously the body of a mammoth had been found by the inhabitants exposed on one of the banks of the river. It still retained its skin and wool, and even the tusks, for they were so firmly fixed that the Samoyedes had been unable to withdraw them. Mr Nossilov did not himself remove the mammoth, but he suggests that here is an excellent opportunity for some museum or university of his country to procure a mammoth in a complete state of preservation. Even the famous mammoth skeleton in the Academy of Sciences at St Petersburg is very far from perfect. Many parts are restored in wood, and the tusks do not belong to the same animal as the other portion of the skeleton.

Geological Photographs

The Eighth Report of the British Association Committeee for the collection of photographs of geological interest in the United Kingdom, just issued, details those which had been received by the committee during 1896-97. The collection, which is stored at 28 Jermyn Street, and which is accessible to students on application, now numbers 1751 items. No less than 364 new pictures were received during the year, among them being a series from the Wealden area, from Nottingham, North Staffordshire, the Sgurr of Eigg, Yorkshire Caves, County Dublin, Yorkshire, the Isle of Man, Devonshire, Isle of Wight, Charnwood, Yorkshire Dales, and North Wales. The most valuable series came from that exquisite artist, Mr R. Welch, of Belfast, who contributed no less than 100 platinotypes of wonderful excellence of Irish geology. It is difficult to over-estimate the value and utility of such a collection, and the committee state that several of the features preserved have now disappeared. We would especially urge all those who follow geological nature with a camera to assist this committee by forwarding platinotypes unmounted to the secretary, Mr W. W. Watts, of the Mason College, Birmingham, and it is to be remembered that a photograph showing geological features, even if not specially taken from a geological point of view, may be of great value as a record for geologists and others.

Arrangement of Chambers in Foraminifera

A possible explanation of the quinqueloculine arrangement of the chambers in the young of the microspheric forms of *Triloculina* and *Biloculina* is offered by Mr J. J. Lister in a paper recently read before the Cambridge Philosophical Society. In the megalospheric forms of these genera the arrangement is very simple, is followed from the beginning, and is disposed on either side of an axis, the median plane which divides any single chamber symmetrically being the median plane of the whole series of chambers. In the microspheric form, as shown by Mr Schlumberger, the arrangement at first is that which characterises the genera *Quinqueloculina* during its whole growth. In the young stage of the *Biloculina* and *Triloculina* the plane dividing any chamber symmetrically is not identical with the corresponding plane of the preceding chamber, but directed at a definite angle to it. Mr Lister thinks that the megalospheric form is produced asexually, and has a direct development, while the microspheric form is produced sexually, and during its growth repeats the arrangement characteristic of allied forms before it attains the arrangement proper to its own genus. This is made much clearer by Mr Lister in his paper with the accompanying diagrams, and is on a par with the life history of *Clavellina* among the ascidians.

Missing Word Problem in "The Annals"

We announced some months ago that the trustees of the British Museum (Natural History) had commissioned one of the staff to prepare a catalogue of moths. As this is to be a standard book of reference for students and the public, we venture to hope that the quotations of previous authors may be a little more explicit than those in the twenty-sixth article of the February number of the *Annals Mag. Nat. Hist.* Entomologists may be quite familiar with their vast literature, but the unfortunate student who is endeavouring to acquire some knowledge of his subject may rack his brains over such quotations as :—

Wlk., xxx. 955.
Koll. Hug. Kasch, iv. p. 494.
Rag. Nouv. Gen., p. 44.
Feld. Reis. Nov., pl. cxxxvi., fig. 40.

One would also like to see uniformity in the abbreviation of author's names. Swinh., Schiff., Butl., and Fabr. convey some meaning, but one cannot say the same of Wlk., Wllgrn., Hmpsn.

Forthcoming Congresses

The International Congress of Physiologists is to be held in Cambridge during the week beginning 23rd August, that is to say, contemporaneously with the Zoological Congress. We are very glad to learn that the Executive Committee of the latter Congress has been taking more active steps than at first seemed probable to obtain the co-operation of all British zoologists, in which name we include not only Irish and Scottish, but those in India and the Colonies. To enable this country to keep up the standard of hospitality that has hitherto been maintained at this Congress, subscriptions are desired and may be sent to the treasurers, Dr. P. L. Sclater and Professor S. J. Hickson at 3 Hanover Square, London, W. The Zoological Society of London has itself contributed £100. In connection with this Congress, a very successful meeting of representatives from natural history and scientific societies in the north of England was held in Manchester on the 16th of February.

The British Association will meet in Bristol during the week beginning 7th September, under the presidency of Sir William Crookes. The local secretaries, Mr Arthur Lee and Dr Bertram Rogers, have already made several arrangements. The reception rooms are to be in the Victoria Rooms, Clifton; the public lectures will be given in the large Colston Hall; while the sections will meet in rooms provided by the Museum Committee, University College, the Society of Merchant Venturers, the Charity Trustees, the Bishop of Clifton, and others. There will be a picture exhibition, with a military band, in the Drill Hall. More scientific in its interest is a biological exhibit being organised by Mr E. J. Lowe, Dr Harrison, and Prof. Lloyd Morgan at the Zoological Gardens, Clifton. The excursions promise to be exceedingly interesting, since they will include Aust Cliff with its well-known section of Lias, Rhaetic, and Trias; Tortworth with its Silurian beds; Stanton Drew, with its megalithic remains; the Cheddar Cliffs and Caves, the sources of the Bristol water supply; the Severn Tunnel, Cadbury Camp, Swindon, Avonmouth, Wells, and Glastonbury, where the Mayor of Wells, the Dean and Chapter, and residents will entertain the visitors to lunch, and the Mayor of Glastonbury will provide tea in the old Abbot's Kitchen. Other excursions to Salisbury and Stonehenge, Nailsworth and Stroud, Longleat and Raglan Castle are under discussion.

The ninth International Congress of Hygiene and Demography will be held at Madrid from April 10th to 19th. The sections under which subjects will be discussed are as follows: Microbiology in Relation to Hygiene; Prophylaxis and Transmissible Disease; Medical Climatology and Topography; Urban Hygiene; Hygiene of Alimentation; Hygiene of Infancy and of Schools; Hygiene of Exercise and Labour; Military and Naval Hygiene; Veterinary Hygiene, Civil and Military; Sanitary Architecture and Engineering; Technics of Demographic Statistics; Statistical Results in Relation to Demography; Dynamical Demography (movements of population, etc.). The secretary is Dr Cabañas, the University, Madrid.

I

A New Scheme of Geological Arrangement and Nomenclature

PART III

I NOW propose to come to closer quarters with my subject, and to do so by calling in question Lyell's scheme of arrangement of the Tertiary beds, which still largely holds the field, as unscientific, mischievous and misleading.

I have not been able to find who first used the term Tertiary in its modern sense—namely, as equivalent to the greater part of the beds above the chalk—nor when the term was first used. Perhaps some reader of *Natural Science* could help me.

Cuvier and Brongniart in their famous memoir on the Paris basin; Webster in his memoirs on the beds of the Isle of Wight and the Thames basin; and Bonelli, Brocchi and other Italians in North Italy and Sicily, had explored with skill and acumen the Tertiary deposits in the localities referred to; but none of them had attempted any general scheme of general classification of the Tertiary beds as a whole. This was the work of the conchologist Deshayes, who, on the evidence of the fossil shells they contain, roughly arranged the beds above the chalk (excluding the beds being currently deposited) into three groups. Lyell, after his famous visit to Italy and France, brought back with him Deshayes' arrangement, and gave names to the latter's anonymous divisions. These names, he tells us, he adopted in consultation with Dr Whewell. Thus arose the terms Eocene, Miocene and Pliocene. These three divisions included everything which Lyell deemed to be Tertiary.

Above the Tertiary beds he placed the deposits which are now being made by river and sea and volcano, etc.—*i.e.*, the actually current beds we see accumulating everywhere now. These beds he called *Recent*, and it is important to remember that by 'Recent' Lyell distinctly meant Non-Tertiary. To him they belonged to an entirely different order of things to anything in the Tertiary beds, and they answered in fact to the beginning of another cycle of Geological history, such as we mark the importance of when we speak of the Primary, Secondary and Tertiary periods. This separation of the Recent beds was widely, almost universally, accepted; and presently—namely, in 1849—a new name was given to them by D'Archiac, namely, *Quaternary*, thus emphasizing the enormous importance which they held in the eyes of a popular school of Geology, since the very name Quaternary was merely the completion of the cycle of names already referred to—namely, Primary, Secondary and Tertiary; and this term Quaternary not only still lives, but is in continuous use, and especially by the geologists of the continent.

Now, some of us know well, or think we know, the vast difference that separates the Primary, Secondary and Tertiary beds. The huge gap which separates their fossil contents in our latitudes makes it necessary to deal with them in any systematic scheme of geology as ultimate and supreme factors.

In order to create a fourth division co-ordinate with them, we ought assuredly to have co-ordinate conditions separating the Tertiary beds from the 'Recent' beds of Lyell and the Quaternary of other writers. Let us turn then to Lyell's own words, and see how he defined his Recent beds, and by what *criteria* he separated them from the beds he called Tertiary.

I shall quote his own definition: "All sedimentary deposits, all volcanic rocks—in a word, every geological monument, whether belonging to the animate or inanimate world, which appertains to this epoch, may be termed recent. Some recent species, therefore, are found fossil in various Tertiary beds; and, on the other hand, others, like the Dodo, may be extinct, for it is sufficient that they should once have co-existed with man, to make them referable to this era." Again he says: "We may sometimes prove that certain strata belong to the recent period by aid of historical evidence, as parts of the delta of the Po, Rhone and Nile, for example; at other times, by discovering imbedded remains of man or his works; but when we have no evidence of this kind, and we hesitate to ascribe a particular deposit to the recent era, or that immediately preceding, we must generally incline to refer it to the latter, for it will appear in the sequel that the changes of the historical era are quite insignificant when contrasted with those of the newest Tertiary period."

Here then we have the ear-mark by which Lyell discriminated his Recent or Post Tertiary beds, namely, the presence in them of man and his works, and that alone. This may have been pardonable in 1830, but what is to be said of it now? What is to be said for a classification which is based professedly on biological evidence, and which treats the introduction not of a very specialised mammal or series of mammals differing essentially from their predecessors? but of one belonging to a large class of mammals, the species Bimanus, and that species alone, as the foundation, not of a new subsection of a particular series of beds, but as equivalent to the portentous changes involved when we compare the beds of the so-called Primary age with those of the Secondary, and those of the Secondary with those of the Tertiary. The position is too ridiculous for argument. Such a classification is an inversion of all logical system and method. It has frequently been protested against, and cannot, it seems to me, be justified in any way whatever. Far from being separable from the Tertiary series, the beds called Recent by Lyell, and Quaternary and Post Tertiary by others, ought to form in essence a very small and very intimate section of the beds of the Tertiary period itself. If the term Tertiary is to be logically used, it must include ourselves and our works. We are not living in Post Tertiary times; we are living in Tertiary times, and we shall continue so to live until a much mightier change has occurred in this world than the addition in a common cemetery of the bones of Shakespeare and Bill Sykes to those of many a buried Yorick, who once cracked nuts when hanging from a shady branch by his tail.

Let us proceed, however. This is not the only objection to Lyell's classification. When Lyell first wrote his Principles, the incoming of Man was supposed to be a comparatively very recent event. The fact that he had lived with such extinct beasts as the Mammoth in Europasia, the Mastodon and the Megatherium in America, was not generally suspected. It required a great many years of patient pleading and hammering by unknown but careful observers like Schmerling in Belgium, and MacEnery in Devonshire, and by others, to whom the world was deaf, before the fact was accepted by the Papal authorities of latter-day science. Be it remembered that no one was more to blame for this neglect and rejection of the facts and their teaching, for more than one decade, than Huxley himself when secretary of the Royal Society, and let us beware of surrendering ourselves to the science of "the Syllabus" in consequence.

We now know that not only was man contemporary with the extinct animals just referred to, but that in all probability he lived with the great Southern Elephant, *E. meridionalis* in Italy, and was also contemporary with the same animal in England, as attested by recent evidence from the forest bed in Norfolk. Inasmuch as (so far as we can tell) the forest bed in Norfolk was contemporary with the latest of the three divisions of the marine crag, the so-called Norfolk or Weybourne crag, it follows that Lyell's name "Recent beds," as defined by himself, and the names subsequently substituted for that phrase, are really equivalent to a great deal more than he supposed, and make a great invasion into his Pliocene horizon.

The names in question, therefore, *i.e.*, Post Tertiary, Quaternary, etc., have a double frailty. Need I press my case in this behalf further. I trow not, but, you will say, how can I presume to write in this way when I have been guilty of using this stupid nomenclature myself. Of course I have been guilty of it. I have nevertheless been ashamed of myself and am so still. My only excuse has been that in order to be understood we must use the common shibboleths of conversation, and in science these shibboleths are the technical phrases of the text books. It does not make the boat less leaky and unseaworthy because I happen to be in it along with the great guns of geology, but I do try to make amends by professing my discontent and shame. I challenge the authors of several recent text books of repute to do the same. Let them justify, if they can, the further use of such terms as Quaternary, Post Tertiary, or Recent (in the sense and with the connotation that Lyell used the word recent). If they cannot justify them let us all agree to disuse them as misleading and ridiculous and take a common stand on the platform that we are all Tertiary Beasts, just as much as that stiff-necked brute the Titanotherium, and that the Tertiary period comes right up to to-day.

Let us now turn to Lyell's division and classification of the Tertiary period. Lyell not only took over Deshayes' main divisions of the Tertiary beds, but he also took over the former's general criterion by which they were to be discriminated. This in essence did not depend upon stratigraphical considerations or upon natural breaks in the sequence, either by unconformability or otherwise, such as we accept as our criteria in the older rocks, but upon entirely different considerations, namely, the numerical proportion of extinct to living forms in any particular bed.

It must be also remembered that in Deshayes' scheme and also in Lyell's the classification proposed was entirely based on the proportion of extinct to living *marine testacea* only, and had application therefore to marine beds only and had no reference of any kind to the fauna of the land.

Deshayes' method was called in question when it was first published both in England and in France, *inter alia*, by Bakewell and Desnoyers, as being based on utterly arbitrary, uncertain, and shifting data. Who is to know until the ocean bed has been scoured from end to end what species are still living? Every fresh dredging expedition on a considerable scale adds to the number of molluscs once supposed to be extinct and which are found to be still alive, many of them (as in fact many of the fossil molluscs also) having very local and limited distribution.

The problem was further sophisticated by the fact that some of the beds point to conditions of deposition similar to those in the neighbouring seas; while others point to tropical or semi-tropical conditions now only existing in latitudes where the dredge has only been applied in most perfunctory ways. The method employed is also largely dependent on the personal equation of observers who, unfortunately, have been separated by our ridiculous theories into separate schools of students, one school dealing with recent and another with fossil molluscs, as if either could possibly be studied satisfactorily without continual reference to the other. The consequence has been the creation of a number of fossil species which had already been described and named as recent. Lyell was himself aware of some of his own difficulties. Thus, in 1836, he calls attention to the discrepancy in the results obtained by Deshayes and Dr Beck, when they examined the proportion of living to extinct forms among the crag shells, Dr Beck going the length of saying that, although a larger proportion of shells in the crag approach very near to others which now live in our northern seas, he regarded them as almost all of distinct species and unknown as living. Lyell adds the pertinent sentence. "In regard to the discordance in the results at which these eminent conchologists have arrived, it may arise, not only from the unequal opportunities which they have enjoyed of examining the necessary data, but also in part to the different estimate which they have formed of the amount of variation necessary to constitute a distinct species. One instance," he says, "will sufficiently illustrate my meaning. Those naturalists who agree with M. Deshayes in referring all the living varieties of *Lucina divaricata* brought from different countries to one and the same species, will identify many more fossils with recent shells than those who agree with Dr Beck in dividing the same recent individuals of *Lucina divaricata* into six or eight distinct species" (*Proc. Geol. Soc.* ii. 372-3). Again, according to our modern lights, the proportion of extinct to recent forms in a particular place may be largely accidental, a shift of the sea bottom might divert the Gulf Stream, or the corresponding cold Newfoundland current, and utterly change the molluscan fauna of a particular district, and yet be only a local and adventitious fact.

The only virtue of the criterion in question, if it has one at all, is that it is of universal and not of mere local application. We can, no doubt, count the shells in a New Zealand bed as we can in a crag bed, and we can then compare the proportions of living to extinct forms in them both, and thus establish a common measure, but the common measure is an utterly misleading and absurd one. The geological history and conditions of the two life-provinces are so different that no rational conclusion can be derived from the fact that two beds at the antipodes to each other happen at a particular moment to contain the same number of recent forms. The recent forms themselves may be, as in the Australian seas, very old and primitive types, while in our seas they are very young and specialised types. The *Lingula*, which has been extinct in European seas since remote geological time, still lives in the Australian seas. The conservation of the biological record is as remarkable in some latitudes as its tendency to radical change is in others, and we are utterly deluding the student with false notions of contemporaneity of deposition, of homotaxis or other similar homologies when we apply the same name to two widely separated beds, not because they contain the same or a similar fauna, but because the proportion of living to extinct forms is the same, or nearly the same, in each. We do not put negroes and Europeans in the same category because in some cases there is the same proportion of rogues to honest men among them, or because of any other purely adventitious circumstance.

A truly scientific classification of the Tertiary beds must, if it is to have any permanent value, be based on more stable data than the proportionate numbers of living forms to extinct ones which occur in a particular bed. The futile character of this criterion applies to it under all circumstances and in all kinds of beds, subaerial or otherwise.

In the case of the Tertiary beds, however, the classification in question became more especially inconvenient and misleading since, without any explanation or justification and in a measure surreptitiously, a mode of classifying beds dependent experimentally upon the proportion of the mollusca in the marine beds was transferred to a series of subaerial beds, where the same criterion was never in fact used as a touchstone. This was done apparently on the very superficial ground that De Beaumont had divided the extinct mammals into three series, respectively marked by the palaeotheria, the mastodons and elephants, and this threefold division of the mammalian fauna of the Tertiary beds was adopted and incorporated by Lyell into his scheme for arranging the marine beds without any inquiry as to whether the divisions in the one case corresponded with the divisions in the other, or could consistently be made to so apply. Names originally based on facts derived from the molluscs were quite arbitrarily applied to facts derived from land animals. This is a very good instance of what I have complained of, namely, the condensing of submarine and subaerial beds into a common series with common names.

On these and other grounds I venture to distrust and rebel against Deshayes' and Lyell's criterion for distinguishing the various divisions of the Tertiary period and the conclusions based on them.

Let us now advance again and approach more concrete ground.

The most recent of Deshayes' divisions was that to which Lyell gave the name of Pliocene. Like the other names he gave to the principal divisions of the Tertiary period, it was, as he tells us, a joint composition of his own and Whewell's, and in this case was a derivative from πλειων major and χαινος recens, and, as Lyell says, was meant to exclude all beds in which the majority of the species of testacea belong to living forms, and at the same time, as we have seen, it excluded all beds in which traces of man had been discovered. It was also co-extensive in its connotation with the latest of the three divisions of Deshayes'.

Now Deshayes' discrimination of his third or latest series of beds was entirely based upon the Italian beds, the beds of the Subapennines, as he first called them, and those of Sicily and the south of Italy respectively. So long as we restrict his definition to that area I see no great cause to quarrel with it.

Lyell's particular part in the business seems to me to be distinctly illogical and mischievous. In the first place he included within the definition of the series and within the connotation of the term Pliocene the English crag beds.

Now I venture to think, and I want to speak very emphatically upon this point, any scheme which includes the more recent beds in the Mediterranean area and those in the North Sea area in a common nomenclature must be of doubtful value. The recent geological history of those two areas is quite different. Their molluscs are different, and the mere fact that they may have been contemporary no more justifies our describing them under a common name than we should be justified in treating Brazil and Birmingham as parts of one zoological province because the parrots in the one country and the politicians in the other are contemporaries, or in speaking of the era of Shakespeare when describing the history of China at the beginning of the seventeenth century. If it be right to speak of the Sicilian and Subapennine beds and their contents as Pliocene, it seems to me to be utterly wrong and misleading to apply the same name to any beds whatever in the area of the North Sea, and *vice-versâ.*

This is not all. By "the crag was understood in 1830 all the shell-bearing beds of eastern England, from the base of the Coralline Crag upwards, including all the drift beds containing marine shells. Inasmuch as Lyell excluded from his Pliocene horizon all deposits contemporary with man, it is clear that he was mistaken in one respect or the other, for, as we now know, the upper crag in the sense in which he used the word was contemporary with the mammoth, which was contemporary with man. By using the term Pliocene, therefore, in the sense in which the inventor used it, we are landed in a complete quagmire. In that quagmire it seems to me every subsequent inquirer into this particular corner of the geological field has fallen. The only escape from it is either to drop the term as applied to the English beds altogether, and to let it stand (if thought right) for the Mediterranean beds alone, or to try in some way to entirely alter the definition and connotation of the term from what it originally meant, and from what its author and inventor meant. This seems very much to me like referring to some dull, sober, quiet, orthodox geologist by the name of Howorth. I cannot put the *reductio ad absurdum* more thoroughly.

Let us, however, advance again. Lyell in taking over Deshayes' division of the Tertiary beds altered it in one respect. He divided it into four series instead of three, and in my view altered the French writer's classification very much for the worse. He altered it by, in fact, separating the uppermost division which had been discriminated by Deshayes into two divisions, to which he gave the name of older and newer Pliocene respectively. This would have been acceptable and useful if applied to the Mediterranean series of Deshayes alone, and if it had been understood that it merely meant the breaking up of one of Deshayes' main divisions into two subordinate ones, but this is not at all what Lyell meant. In his scheme each of the two divisions of the Pliocene series is itself given co-ordinate rank with the other two great divisions of the Tertiary beds, namely, the Eocene and Miocene (see his table *Principles of Geology*, ed. ii., vol. iii., p. 61), and he emphasised this very strongly in the French translation of the *Principles* by introducing for the first time the term Pleistocene. which he made the equivalent of his "later Pliocene." Subsequently he dropped the two divisions of the Pliocene and divided the whole Tertiary series into four great sections, namely, the Eocene, Miocene, Pliocene, and Pleistocene.

Here again, it seems to me, we have another instance of an entire absence of proportion and of perspective in Lyell's method of classification.

The differences which separate Eocene from Miocene life are assuredly patent enough, and they involve real and substantial biological and other differences The same is the case when we distinguish between the beds classed as Miocene and those classed as Pliocene ; but what sort of differences at all comparable to these separate the Pliocene from the Pleistocene. Even if we limit ourselves to Italy and its marine beds, they are so slight that Deshayes condensed them both into one series; but as applied elsewhere by Lyell, namely, to the countries of the North Sea, they are, with our present knowledge, positively ridiculous. They were always quite inadequate, however, and at the very most justified the creating of subsections of the Pliocene just as we now have subsections of the Miocene, but nothing more, and to introduce a new term like *pleistocene*, with the particular intention of treating it as equivalent in weight and value to the other main divisions of the Tertiary series was intolerable, and would not, in fact, have been tolerated but for the glamour attaching to Lyell's name and the tyrannical influence which his reputation has exercised upon subsequent writers. Here again it seems to me we ought to eject the term pleistocene from our nomenclature altogether, unless we are to perform the indecent part of making it mean and include quite a different thing altogether from that which its original godfather meant it to include.

I spoke of this latter part of Lyell's classification and nomenclature as being ridiculous with our present lights. What I mean is this. When Lyell published his work in 1832 the term Crag was used as a common name for all the shelly, sandy. and gravelly surface beds of eastern England, from the base of the Coralline Crag upwards, inclusive of the so-called middle sands containing fossil molluscs. When the contents of all these beds were mixed together and treated as in fact one common fauna, chiefly noteworthy for the great number of tropical, sub-tropical, and of extinct forms which characterise the Coralline Crag, it was not unnatural that Lyell using his criterion should have put the Crag into his older Pliocene, and that he should have completely excluded Britain from the areas where his Newer Pliocene or Pleistocene were known to occur. Into that upper division, however, he put the well-known beds at Uddevalla. This exclusion of Britain from Lyell's original Pleistocene localities must have a strange look to those who know how frequently the term Pleistocene now occurs in more recent English geological literature as denoting a large series of English beds.

As originally defined, the term Pleistocene included among marine beds only the strata of Val de Noto in Sicily, those in the district about Naples, and in Calabria, some doubtful beds at Morea, and the well-known beds at Uddevalla, while the whole of the Crag was put into the older division of the Pliocene, namely, the Pliocene proper, together with the subapennine beds.

Let us again go on, however. The first person to properly discriminate the different elements in the English Crag was Charlesworth, one of those not too lucky men upon whom the sunlight did not always fall, and to whom those of us who care for these things ought to be grateful indeed, for he did more to elucidate the later English Tertiaries than anybody else. His teaching on this matter was sound. May we hope that he now has peace. He showed very plainly that the Crag consists of three distinct horizons, marked by very different life conditions and contents—namely, the White or Coralline Crag, the Red or Suffolk Crag, and the Mammaliferous or Norwich Crag.

Charlesworth's famous memoir on the Crag was published in the London and Edinburgh *Phil. Mag.*, vol. xxxviii. p. 81, August 1835.

The results obtained by Charlesworth were soon after examined by Lyell, who in a paper read before the Geological Society in 1839, accepted his views, and proceeded to apply his own numerical and proportionate method to them. This led him to perform curious somersaults in nomenclature. As we have seen in his first arrangement of the Tertiaries, he put the Crag into his "Older Pliocene," leaving the "Newer Pliocene" without an English representative.

He now entirely altered this view in a most revolutionary manner, and while he retained the Norwich Crag in his older Pliocene, he transferred the Red Crag and the Coralline Crag to "different parts of the Miocene," while he put the testacea of the fresh water or fluviatile deposits of Cromer and Mundesley in Norfolk, Sutton, Grays, Ilford, &c., into his Newer Pliocene (*Proc. Geol. Soc.*, vol. iii. pp. 129 and 130). In this paper Lyell co-ordinates the older crags of East Anglia with the Faluns of Touraine as Desnoyer had done. This he did again on what I deem the same irrational ground, that the proportion of extinct to recent forms in both was pretty nearly the same. How false and misleading such a comparison must be, is shown by the fact that Searles Wood only found 10 per cent. of the shells common to the Crag and the Faluns, thus proving that the two sets of beds really belong to two entirely different geological provinces, and ought to be classified under separate names.

Again, as we have seen, the shells in the diluvium or drift were originally treated as part of the Crag, a view to which some of us have returned. In November 1836, Mr James Smith of Jordan-hill, another of the excellent pioneers of geology, first definitely separated the shell-bearing drifts of Scotland and placed them among the 'Newer Pliocene' (*Proc. Geol. Soc.*, vol. ii. p. 428). In February of the following year Mr Clarke similarly separated the diluvium from the Crag of East Anglia.

In November 1839, Mr James Smith went further and separated the true shelly drift of Scotland, called glacial by some, and to which he now gave the name *pleistocene* (this being apparently the first time the name is used in England), from the submerged forests and sandbeaches there which he calls post-Tertiary (Ed. iii., 149 and 150).

In their paper on the geology of Russia by Murchison and Verneuil in 1841, they applied the same name *post-pleiocene* or *pleistocene* to the so-called diluvium or drift, inclusive of the marine surface shell-beds of Northern Russia, whose contents were correlated with the beds at Uddevalla, which Mr James Smith had affirmed to be identical with those in the older shelly drift beds round Scotland.

In 1846 Edward Forbes published his well-known memoir on the geological relations of the fauna and flora of the British Isles, and he also used the term pleistocene. "But," says Lyell, "he applied the term almost precisely in the sense in which I have hitherto used Post-Pliocene, and not as short for Newer Pliocene." "In order, therefore, to prevent confusion, I thought it best entirely to abstain from the use of Pleistocene in future; but in a note to my 'Elements of Geology' I advised such geologists as wished to retain Pleistocene to use it as strictly synonymous with Post-Pliocene" (*Antiquity of Man*, pp. 3 and 4). Thus we see Lyell destroying the connotation of his own name, and advising his friends to apply it to an entirely different geological period.

The next important step taken in discussing the later English Tertiaries was that consequent upon the labours of Searles Wood and Harmer, combined with those of S. P. Woodward. This led to another extraordinary somersault in nomenclature. Lyell himself and Sowerby had, after an examination of the earlier crag shells, decided that 26 per cent. of them only were recent. Now it was found that even in the oldest and earliest of the English crags the proportion of living exceeded that of extinct forms. Therefore, according to the absurd criterion of Deshayes and Lyell, which had been responsible for the various chameleon changes I have described, the older crag beds were again transferred from the Miocene horizon to the Pliocene, and made to represent the older Pliocene. This change was dependent on an examination of the Testacea.

Meanwhile Owen and Lankester pronounced the Mammalian remains in the Suffolk or Red Crag to be very distinctly Miocene in character, adding another element of confusion to the already interminable tangle.

Lyell, who in his original scheme had treated his Newer Pliocene as non-existent in England, having thus appropriated his older Pliocene to the Coralline and Red Crags, now proceeded to evict the Norwich Crag and associated beds, including in his scheme the Forest Bed from the Older Pliocene, and transferred it to the Newer Pliocene. To the same series he also transferred the whole of the Glacial beds, and reserved the term pleistocene apparently entirely for the beds containing palaeolithic implements, and those in which the Mammoth and its companions were found associated with human remains, which beds he supposed lay over the so-called drift, a view against which some of us have protested for years, not a word being said or suggested about any marine equivalents of these beds. He further reserved the term Recent for everything above the so-called palaeolithic beds (*Antiquity of Man*, p. 260).

This is not enough. When we turn to a still later authority, Mr Horace B. Woodward, we find him following the lead of Mr Searles Wood, junior, and transferring the whole of the so-called Glacial series to the Pleistocene horizon, and retaining the Forest Bed and the Norwich Crag in the Newer Pliocene. With these two he also placed the Suffolk Crag, which for the first time was divorced from the Coralline Crag, and placed in another main division of the Tertiaries, the latter being left alone in the Older Pliocene. In the same work we are told very distinctly, and I think rather rashly, that Miocene beds do not occur in England and Wales at all. I am weary and tired of my imperfect analysis of this most extraordinary story. Nothing surely in all the annals of Science, nothing in the prodigious memory of my good kind friend Sherborn (may the sun long shine brightly on so industrious a worker) can equal this record, for it is not a question of naming a species, but of arranging and naming a great series of geological horizons. The chameleon has adopted every possible tint. The names I have quoted have done service in every possible variety of way, and have had as many meanings as there are whims in women. It is simply astounding that such a mass of absurdity as is involved in the story I have told should have been tolerated. It has only been so tolerated, I take it, because men have been afraid to say that Lyell, great genius as he was, has in this matter, as in many others, led the geological rabble into the wilderness; has led not merely the geological lambs, but the old horny rams and hornless ewes as well. Anyone who has favoured me by reading what I have here written is at liberty, if he can, to find some rational clue to the whole mess and maze, and thus to justify it. To me the names I have quoted have been used in so many different senses, apparently at the whim of fancy of every new writer, and have such an absolutely confused connotation that they are completely worthless for all serious purposes; and much as I dislike changes of nomenclature, I see no possible hope save to sweep them away into the limbo of oblivion.

But the names are a small matter. I protest also, as a heretic should, against the whole scheme and method of Lyell's systematic arrangement of the Tertiary and Recent beds as utterly irrational, and based upon entirely misleading and mischievous criteria, namely, the proportion of recent to fossil forms in any bed, and I will quote, in again doing so, a former president of the Geological Society, a big man with a very wide grasp of logic and knowledge. *Apropos* of this very scheme, Whewell, in his address to the

Geological Society, said: "I conceive that in a matter of arrangement any arbitrary numerical character must lead to violations of nature's classification, and can only be considered as an artificial method to be used provisionally till some more genuine principle of order is discovered" (*Proc. Geol. Soc.*, ii. 637).

Thirdly, I protest again against attempting to force false analogies and inferences into our science by giving a common name to beds so far separated as are the Tertiary beds of the Mediterranean, those of Touraine, and those of the borders of the North Sea The equivalents of our crag are not to be sought in Sicily, or in Piedmont, or on the Loire, but in Belgium and other places on the North Sea, where common features will justify a common nomenclature. The different geological basins or provinces here named should be studied separately, and their beds should have a separate nomenclature; so should the subaerial and submarine beds in each. My present paper is critical. My next one, if you will have it, shall attempt something constructive.

HENRY H. HOWORTH.

II

The Broadening of Atoll-islets

A POETIC simile of Dana's compared an atoll to a garland thrown upon the waters. But few are the atolls that answer to this ideal; in most the wreath encircling the lagoon is twisted and torn to fragments, forming a more or less complete chain of atoll-islets.

That the shore line of these islets is not stationary has long been recognised. By an extension of their length they may unite into a complete ring, and further growth upon the inner sides may result in the obliteration of the lagoon and the conversion of the atoll into an island.

Murray[1] allows that in small atolls the islets may thus by ingrowth fill up the lagoon, yet he considers that in atolls over two miles in diameter the lagoon would enlarge by solution, which implies that the lagoon shores of the islets would be eaten back.

This idea has not received support from those who have studied coral reefs on the spot in either the Indian, Atlantic, or Pacific Oceans. Guppy[2] has described in admirable detail the way in which the large lagoon of Keeling Island is gradually disappearing, Heilprin[3] has pertinently remarked of the Bermudas that whereas we should expect an exposure of bare rock to occur in a basin of solution, his dredgings invariably proved the entire floor to be covered with a thick deposit of ooze. To me[4] the wide lagoon of Funafuti seemed on every side to be encroached upon by the land. On the other hand, Bourne observed in Diego Garcia that the lagoon made inroads on the land.

While attention has thus been paid to the increase or decrease of the land upon the lagoon shore of an atoll, less notice seems to have been bestowed on the possibility of fluctuations on the outer or peripheral coast. Some features of the Funafuti beach suggested to me a seaward growth.

Crossing the largest or windward islet from east to west, that is from the ocean to the lagoon, there appears first a steep and

[1] *Nature*, vol. xlviii., 1893, p. 576.
[2] Guppy. *Scottish Geogr. Mag.*, vol. v., 1889, p. 573.
[3] Heilprin, "The Bermudas Islands," 1889, p. 41.
[4] Hedley, "The Atoll of Funafuti," 1896, p. 17.

high bank of coral shingle facing the waves, then a gentler, longer slope inland, after this another shingle bank, less steep and less high, followed by a similar slope, then another and perhaps still another bank, each lower than its predecessor, the last scarcely perceptible. The coral blocks of which these banks are built vary according to their situation; on the seaward side they are pebbles and boulders, mostly so ground and worn as to be scarcely recognisable as corals, but on the banks further inland they are rough and much etched by the weather, there they are also brittle from decay.

The history of these successive banks of coral debris seems obviously to be, that each was piled up by the ocean waves and was afterwards shut in by its eastern neighbour, built up to windward at a later date. The lower present level of the inner banks must be the result of collapse of its material, comparable to the collapse that occurs in an unused heap of road metal, while the more advanced state of decay of that material, previously noted, likewise points to its superior antiquity. While the upper parts of the hurricane beaches thus change, their lowest stratum may by solution and deposition be welded into breccia, as a heap of hail may be turned by slight melting and freezing into a solid mass.

These wave-like rows of shingle appear to be an ordinary phenomenon of atoll structure and have been noted by many observers. Guppy[1] saw on the windward angle of Keeling Island a massive looking slope of large blocks of coral forming a kind of glacis. On North Keeling Island he was informed that the shore on the landing place on the west coast had advanced some fifteen or twenty paces during the previous ten or fifteen years, the old line being indicated by the overhanging cocoanut palms removed that distance from the sea; it was largely the work of a single night, a huge pile of coral blocks being piled on the beach during a cyclone.

Speaking of the atoll of Peru, one of the Gilbert or Kingsmill Archipelago, Whitmee[2] says, "The Island itself is formed of successive ridges of sand, broken coral, and shells. These ridges are most of them from thirty to fifty feet across, and the hollows formed between them are generally from four to six feet in depth. For some distance, at that end of the island which I examined, they run across, and in the middle they run parallel with the sides of the island. The whole extent examined presented the same appearance, and the ridges were so regular that they gave one the idea of being artificially formed. The waves must exert a mighty force during heavy weather to form these extensive ridges.

y, *Scot. Geogr. Mag.*, vol. v., 1889, pp. 296 and 468.

[2] Whitmee, "A Missionary Cruise in the South Pacific," 1871, p. 35.

the reef flat; that it is not so buried suggests that the reef flat as well as the dry land advances seawards. The reef flat is typically seamed with channels; if these are not excavated by the sea (which is incredible) then they must be enclosed by coral growth. That they are so enclosed is offered as proof that the reef flat itself grows seawards, built out probably on a foundation of its own waste.

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III

Second Contribution on Numerical Variation of parts in Ranunculus repens (L.)

THE present investigation was undertaken in continuation of a paper which appeared in *Natural Science* for May 1897, p. 323, as it was thought that the conclusions drawn and results obtained from a series of 500 specimens, might be influenced by the small number examined. Accordingly (as noted in *Natural Science*, June 1897, p. 429), another series of 500 was examined, and in the following paper the results of this second series, separately, and combined with the first, are discussed. It will, I think, be admitted that conclusions based on observations of 1000 specimens are fairly well founded. The specimens were again most kindly supplied by Miss K. M. Hall, Curator of the Whitechapel Museum, from practically the same locality as last year, my occupation preventing me from collecting them myself.

The number of specimens examined was again 500. The method employed in recording the results obtained was slightly different from that used last year. Sheets of paper ruled in five columns were taken, and the columns were marked successively—first column, consecutive numbers from 1 to 500; second column, calyx; third, fourth, and fifth, corolla, stamens, and carpels respectively, each specimen having one line to itself on one sheet.

Example—

1896.

Number.	Calyx.	Corolla.	Stamens.	Carpels.
1	5	5	68	46
2	5	9	55	43

With this method the results are more accessible than with the former plan, which, however, possessed certain advantages.

The total number of parts in the 1000 specimens examined was—

	1896.	1897.	Total.
Calyx	2,502	2,496	4,998
Corolla	2,836	2,717	5,553
Stamens . .	27,267	26,808	54,075
Carpels . .	18,037	17,354	35,391
Total .	50,642	49,375	100,017

It will be seen that these figures show an average of rather more than one hundred parts (organs) to each specimen.

The time occupied in the actual dissection of the specimens was in the aggregate nearly 56 hours, this giving each specimen, on an average, about 3⅓ minutes.

The floral formula obtained from last year's work was—

Calyx, 5 ; Corolla, 5 ; Stamens, 54 ; Carpels, 36.

This year's results are—

Calyx, 5 ; Corolla, 5 ; Stamens, 54 ; Carpels, 35 :

and the mean of the two—that is to say, the floral formula obtained by calculating the mean of 1000 specimens, is—

Calyx, 5 ; Corolla, 5 ; Stamens, 54 ; Carpels, 35.

The results are given exactly, in tabulated form, below.

	Mean of Calyx	Mean of Corolla.	Mean of Stamens.	Mean of Carpels.
1896 .	5·004	5·672	54·534	36·074
1897 .	4·992	5·434	53·616	34·708
Mean . .	4·998	5·553	54·075	35·391

It will be seen that the corolla shows a marked tendency to increase the number of its members, more so in the first series than in the second. In fact, in view of the large fraction, the corolla might almost be given as 6. Four specimens in the 1000 conform to the formula given—two in the first series (1896) and two in the second series (1897).

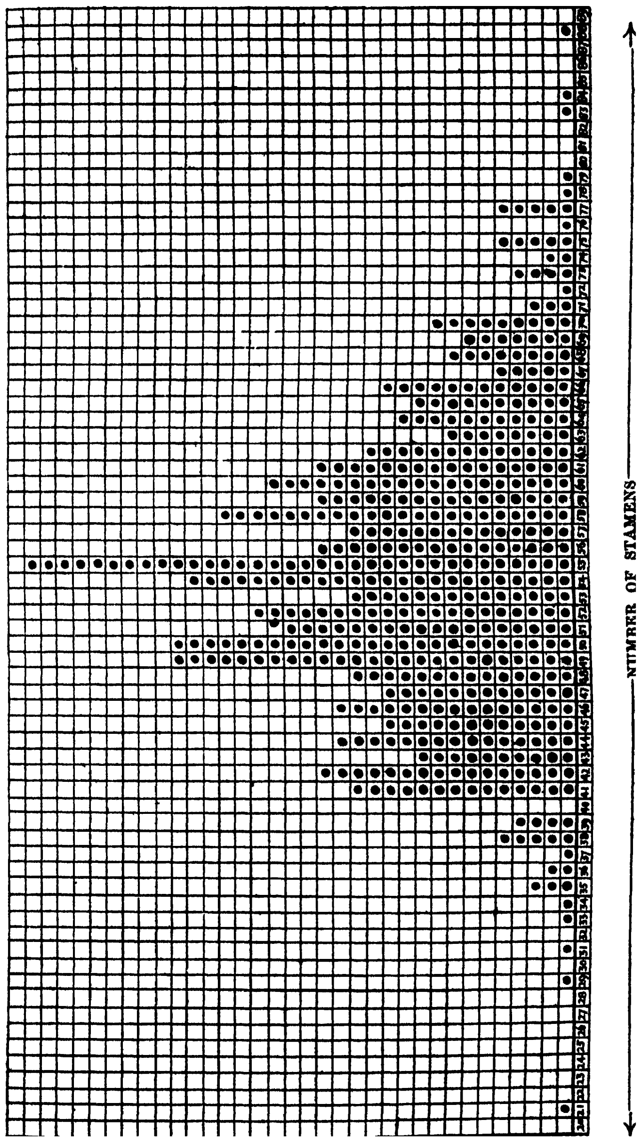
Calyx.

	1896.	1897.	Total.
Number of specimens with 3 sepals	—	1	1
" " 4 "	9	11	20
" " 5 "	481	478	959
" " 6 "	9	9	18
" " 7 "	1	1	2

In the one specimen having but three members to the calyx the sepals were equidistant from each other.

In all 20 cases of 4 sepals there was a re-adjustment of

FIG. 1.—VARIATION IN STAMENS OF 500 SPECIMENS OF RANUNCULUS REPENS (L.) *First Series.*



symmetry, and in 16 of the 18 cases of 6 sepals there was also a re-adjustment of symmetry.

It will be observed that the results of the second 500 show, as in the first series, that very little variation occurs in this (calyx) whorl, the two series agreeing most remarkably.

The total amount of variation from the mean being so small (4·1 per cent.), it may be stated that there is practically no varying in the number of parts.

Corolla.

	1896.	1897.	Total.
Number of specimens with 4 petals	7	1	8
" " 5 "	345	361	706
" " 6 "	60	85	145
" " 7 "	36	36	72
" " 8 "	24	14	38
" " 9 "	13	2	15
" " 10 "	6	1	7
" " 11 "	7	—	7
" " 12 "	1		1
" " 13 "	1	—	1

In the second series there is, on the whole, rather less tendency in the petal whorl to vary from the mean than was shown in the first series—72·2 per cent of the second 500 having the average number of petals, against 69 per cent. last year. The number of specimens having 6 petals is, however, considerably greater than in the first series. By a curious coincidence the number with 7 petals is the same in both series (36). The greatest difference between the two, lies in the specimens having more than 7 petals, the first series having 52, the second only 17.

Stamens.

On reference to fig. 1, which represents the variation in the first series of 500 specimens (those examined in 1896), it will be seen that the curve is fairly regular, there being, however, a decided hump at 49-50. Fig. 2 gives the corresponding curve for the second 500 (1897). This is very much less satisfactory, there being no decided maximum, as in fig. 1, and, also, the recorded maximum 47 does not agree with the calculated mean 54.

The range of variation is—

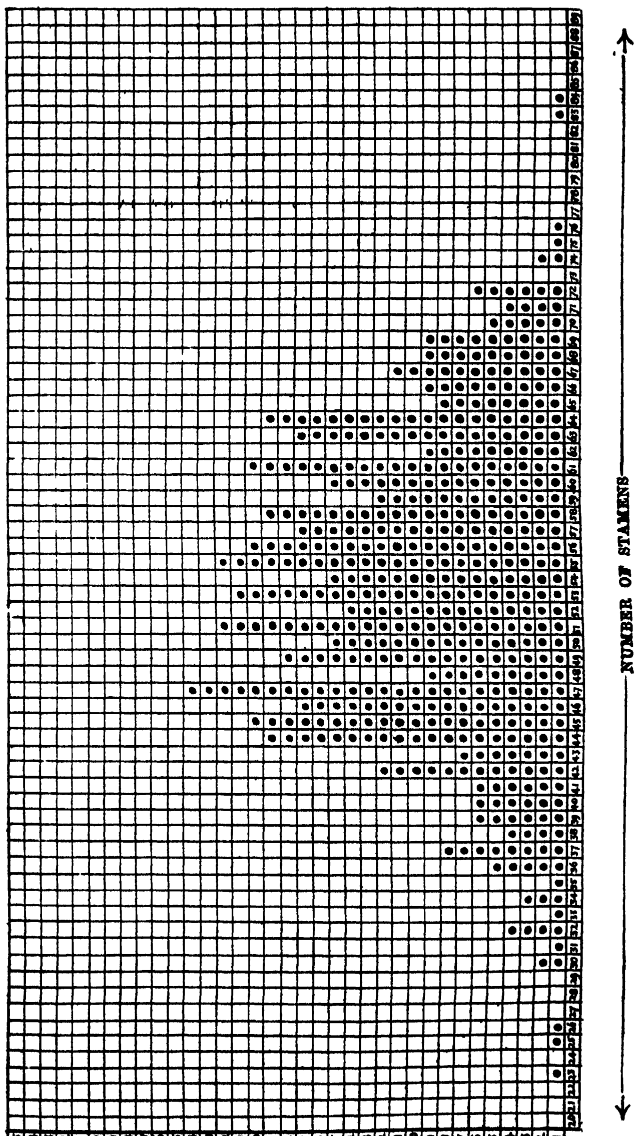
Series I.—Maximum 88, minimum 21.
Series II.—Maximum 84, minimum 23.

Fig. 3 gives the curve obtained by combining the two series. It must, I think, on the whole, be considered very satisfactory. The recorded maximum is 55, the calculated mean 54. The hump at 49, remarked in fig. 1, is still noticeable, although not traceable in fig. 2.

Carpels.

The extreme irregularity of this curve given by the first series (fig. 4) was noticed in my first paper, the recorded maximum being at 25, while the calculated mean (for that series) is 36,

FIG. 2.—VARIATION IN STAMENS OF 500 SPECIMENS OF RANUNCULUS REPENS (L.) *Second Series.*



a marked secondary and also a tertiary maximum being noticeable.

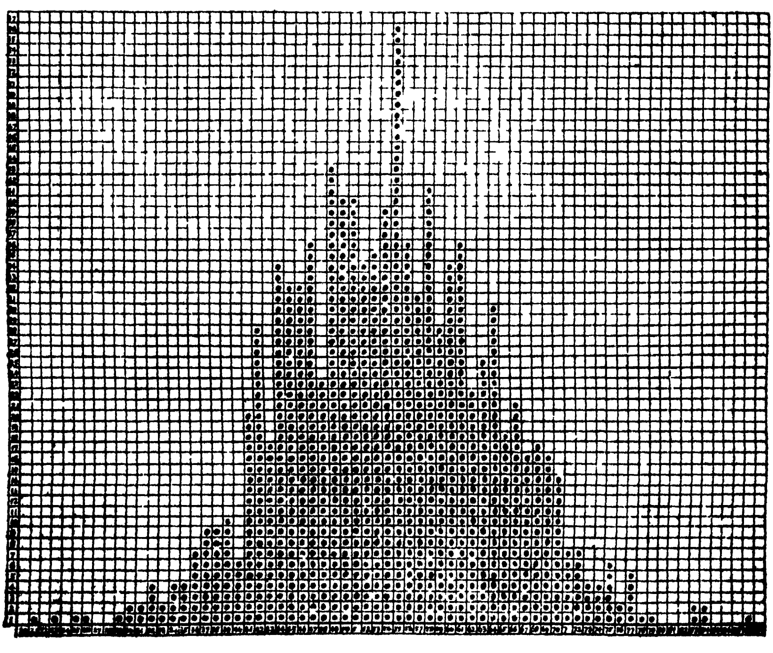
The curve of the second series of carpels (fig. 5) shows a regularity which, with 500 specimens, I think would be hard to surpass. The recorded maxima are 34 and 36—the calculated mean, 35.

The range of variation is very much less in the second series than in the first.

Series I.—Maximum 65, minimum 15.
Series II.—Maximum 52, minimum 17.

Fig. 6 is the curve of the two series combined. It is a very fair example of a continuous variation curve. The hump in fig. 4 at 25 still, however, makes its presence felt, and destroys the symmetry of otherwise an exceedingly good curve.

FIG. 3.—VARIATION IN STAMENS OF 1000 SPECIMENS OF RANUNCULUS REPENS (L.)



On examination of the appended charts I think it will be conceded that there is here an undoubted case of undisturbed continuous variation. The addition to the first, of the second series, simply rounds off and does not alter the curve in general. Any irregularities in the resultant curves are only apparent, and are due to the comparatively small number of specimens under notice. I consider, from these results, that the concluding remark of my first paper must be answered in the affirmative.[1]

[1] I suggested (*Natural Science*, May 1897, p. 328) that the recorded irregularities in the stamen and carpel curves were only due to the small number of specimens (500) then under discussion, and that they would disappear if a larger number were examined.

Correlation of Variation

Calyx

In every case of calyx variation, whether above or below the mean, all the other whorls are above the average. As there are, however, only 41 specimens out of the 1000 to prove this, I do not think that much importance should be attached to the results.

Formulae obtained from specimens having variation in calyx—

Minus variation

	Petals.	Stamens.	Carpels.
Specimens with 3 sepals	5·0	59·0	41·0
" " 4 "	6·0	50·5	34·5
Mean	5·5	54·7	37·7

Plus variation

	Petals.	Stamens.	Carpels.
Specimens with 6 sepals	6·3	52·3	36·0
" " 7 "	6·0	62·5	40·5
Mean	6·1	57 4	38·2

Collective variation

	Petals.	Stamens.	Carpels.
Mean of specimens with 3 sepals .	5·0	59·0	41·0
" " 4 " .	6·0	50·5	34·5
" " 6 " .	6·3	52·3	36·0
" " 7 "	6·0	62·5	40·5
Mean .	5·8	56·0	38·0

Calyx and Corolla

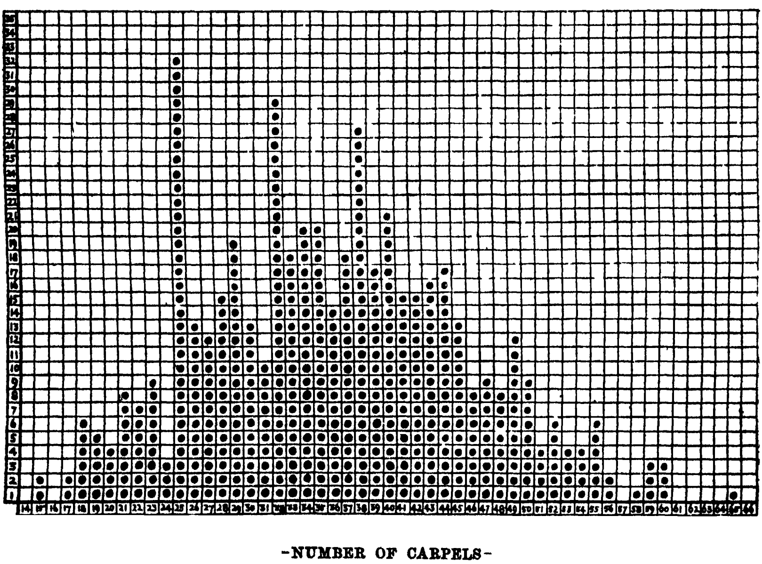
Analysis of variation in calyx and corolla—

Number of specimens having in			1896.	1897.	Total	P cent
calyx	5 sepals, and in corolla	5 petals	338	351	689	68·9
	5 " "	less than 5 "	7	1	8	·8
	5 " "	more than 5 "	136	126	262	26·2
Less than	5 " "	5 "	4	4	8	·8
"	5 " "	less than 5 "	...	..	...	...
"	5 " "	more than 5 "	5	8	13	1·3
More than	5 " "	5 "	3	6	9	·9
"	5 " "	less than 5 "	...	..	...	...
"	5 " "	more than 5 "	7	4	11	1·1

The calyx having practically no variation, it appears from the above that if there is any superabundant calyx- or corolla-material in the embryonic flower, rather than increase the two whorls alike, the sepals are left unaffected, and the superabundant matter goes to form more petals—third line of above table. Actually, the calyx and corolla whorls of the specimens represented in that line have a combined average of 12 parts, but instead of each having 6, as we should assume from the formula being normally 5 and 5, the calyx remains the same, 5, and the corolla rises to 7. In further analysis of the last statement, the mean of the 262 specimens in which the calyx had 5 sepals and the corolla more than 5 petals, is—petals, 6·9; stamens, 52·4; carpels, 37·4. The extra material available has increased the corolla by 1·9 members (or more strictly by 1·4), and has also added to the carpels; but in these specimens the stamens are decidedly below the general mean.

We may assume therefore, from the foregoing, that if there is any plus variation in any specimen, the chances are that the calyx will remain normal, the corolla and carpels be increased, while the stamens will be below the mean.

FIG. 4.—VARIATION IN CARPELS OF 500 SPECIMENS OF RANUNCULUS REPENS (L.)
First Series.



-NUMBER OF CARPELS-

(The formula given by the 8 specimens with corolla below the mean is—calyx, 5; corolla, 4; stamens, 55·3; carpels, 40·5.)

CORRELATION OF VARIATION IN STAMENS AND CARPELS

Number of specimens having				1896.	1897.	Total.	P. cent.
stamens	54	and carpels	35 .	2	2	4	·4
	54	„ less than	35 .	9	5	14	1·4
	54	„ more than	35 .	13	8	21	2·1
Less than	54	„	35 .	10	15	25	2·5
„	54	„ less than	35 .	136	183	319	31·9
„	54	„ more than	35 .	84	50	134	13·4
More than	54	„	35 .	8	12	20	2·0
„	54	„ less than	35 .	82	59	141	14·1
„	54	„ more than	35 .	156	166	322	32·2

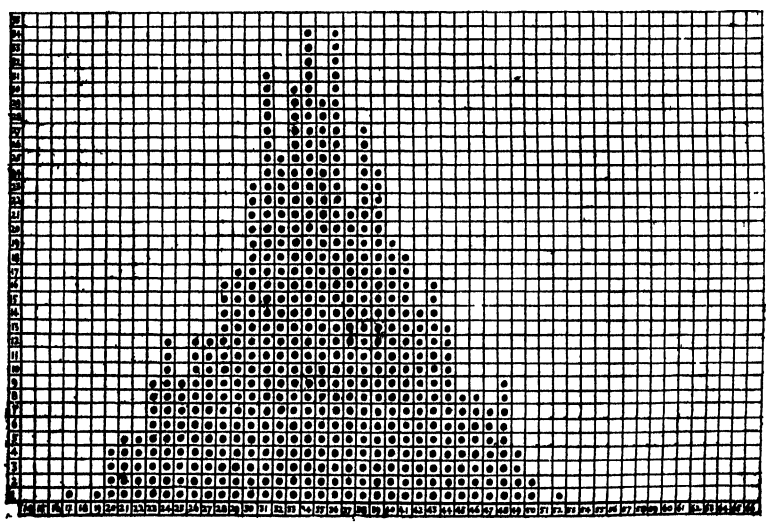
From these figures I assume that there is only a certain amount of sexual material to begin with, and if this is insufficient to form the average number of stamens, the number of carpels will also be below the mean. The reverse also holds good—that is, if there is a superabundance of sexual matter, so that the number of stamens is above the mean, the number of carpels will also be above the mean. We do not find, at least not to any extent (1·4 per cent.), that the number of stamens comes up to the mean at the expense of the carpel whorl, or that the carpels increase at the expense of the stamens (2·5 per cent.).

CORRELATION OF VARIATION IN ALL FOUR WHORLS COLLECTIVELY

There are 81 possibilities of such variation (3^4)—'the mean,' 'below the mean,' 'above the mean'—in each of four whorls.

In the combined series (1000 specimens) of these 81 possibilities, only 35 (43 per cent.) occur.

FIG. 5.—VARIATION IN CARPELS OF 500 SPECIMENS OF RANUNCULUS REPENS (L.) *Second Series.*



NUMBER OF CARPELS

It may, therefore, be definitely stated that the whorls do not vary independently. Particulars of the 35 are here given.

(1. signifies the mean, 2. = below the mean, 3. = above the mean, so that 1.1.1.1 means that the specimen has in the calyx, corolla, and stamen and carpel whorls, each respectively, the mean number of members. Similarly 1.1.1.2, for instance, signifies that calyx, corolla, and stamens are at the mean, but the number of carpels is below the mean, and so on.)

1.1.1.1.	2.1.2.2.	3.1.2.2.
1.1.1.2.	2.1.2.3.	3.1.2.3.
1.1.1.3.	2.1.3.2.	3.1.3.3.
1.1.2.1.	2.1.3.3	3.3.2.2.
1.1.2.2.	2.3.2.2.	3.3.2.3.
1.1.2.3.	2.3.2.3.	3.3.3.1.
1.1.3.1.	2.3.3.3.	3.3.3.2.
1.1.3.2.		3.3.3.3.
1.1.3.3.		
1.2.2.2.		
1.2.2.3		
1.2.3.2.		
1.2.3.3.		
1.3.1.3.		
1.3.2.1.		
1.3.2.2.		
1.3.2.3.		
1.3.3.1.		
1.3.3.2.		
1.3.3.3.		

It will be noted that the greater number of the observed possibilities, 20 out of 35 (57 per cent.), occur when the calyx whorl is at the mean. Of course, recollecting that in (practically) 96 per cent. of the specimens there was no variation from the mean in the calyx, such would naturally follow. Of the remaining 4 per cent. —41 specimens—15 have purely chance variations, a rather less proportion than that of the whole series—36 per cent. of 41 specimens against 43 per cent. of 1000.

I have now shortly discussed correlation of variation in calyx,—calyx and corolla,—stamens and carpels,—and lastly,—correlation of variation in all four whorls collectively. To summarise the results, we found in the analysis of the *calyx* that increase in corolla was in every case accompanied by increase in the stamens and carpels independently of the calyx. Therefore, as regards the calyx on the one hand, and corolla, stamens, and carpels collectively on the other, the data obtained seem to show that the number of members of the calyx has no effect whatever on the other three whorls. In this instance at least, any variation is pure chance. It may be, however, that this conclusion should be modified on account of the small number of specimens affecting it.

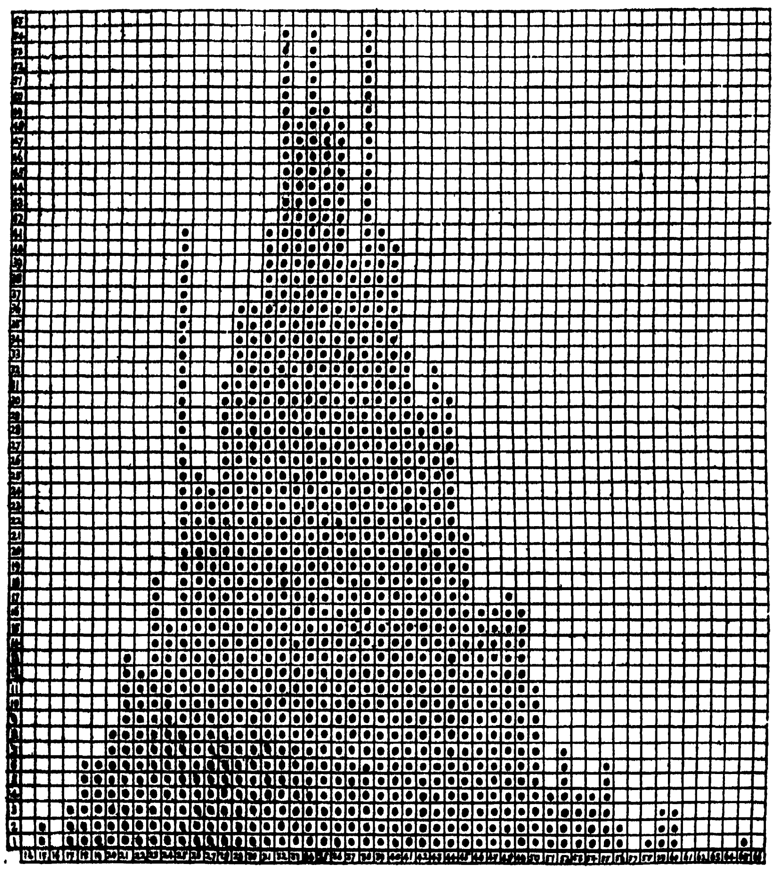
Calyx and Corolla

The results obtained here are rather perplexing, but in view of the number of specimens discussed (262), I presume, must be accepted.

Stamens and Carpels

No further remarks are needed, as the figures given are sufficiently convincing. The data given of the *collective correlation* are equally, or perhaps more, conclusive, and do not require further discussion here.

FIG. 6.—VARIATION IN CARPELS OF 1000 SPECIMENS OF RANUNCULUS REPENS (L.)



NUMBER OF CARPELS

In concluding this paper I must express my thanks to Miss K. M. Hall for supplying the specimens, and to Mr P. Chalmers Mitchell for making various suggestions.

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IV

Egyptian Fisheries

IN Egypt as in other Mohamedan countries fish forms an important part of the food of the people. Fish diet is not enjoined upon the followers of Islam by their religion, but, from firmly established ideas as to its virtues, its use is universal amongst the rich and poor of the country, and it follows that the fishing industry is one of considerable importance.

The Nile itself and the network of irrigation canals throughout the cultivable parts of the Delta furnishes the adjacent population with a supply of food. In the case of the larger towns and more populous districts this supply is augmented by fish from Birket El-Keroun, the supposed residue of the ancient Lake Moeris, from the brackish lakes on the northern coast line and from the Mediterranean and the Red Sea. The local supply from the Nile and its canals, in fact all fresh-water fish, is as a rule eaten fresh, and the same may be said for the marine supply coming from Suez and Alexandria; that from the brackish lakes consists mainly of salted fish, a very small proportion of the lake fish being sold for fresh consumption in the immediate neighbourhood. The reason for this is due partly to the difficulty of transport, there being no quick service of trains nor means for keeping fish fresh over a long railway journey, but the chief reason is the strong demand there is for a form of salt fish called *fessikh*, made from grey mullet, the predominating fish of these localities. Of the Nile species all are eaten fresh with the exception of *Tetrodon fahaka* and *Malapterurus*, the poorer natives being the chief consumers. The flesh of all is more or less muddy tasted or insipid. Serranidae and Carangidae furnish the bulk of the edible species from the Red Sea, and all the larger sized forms of the littoral fish fauna are to be found in markets at Alexandria and Port Said. The fresh water fishermen are, all of them, natives, who form no distinctive class of the population. The sea-fisheries are pursued chiefly by Neapolitan fishermen, who make Alexandria, Port Said and Suez their headquarters. Native fishermen are to be found at these places, but they seldom venture far from the shore. At Lake Menzaleh, the largest and most important of the brackish lakes, the fishing population is quite distinct from the *fellaheen* or agricultural labourers. They live on two islands in the lake, and with few exceptions they marry among themselves and they hold native outsiders in great contempt. Their physique is fine and their cast of countenance is distinctly Caucasian, indeed, some authorities have it that they are descendants of the Hyksos or shepherd kings of Ancient Egypt.

The brackish water fisheries, as being most characteristic of the country and being those with which I have been most intimately associated, I shall describe at some detail. The brackish lakes, beginning at the extreme eastern boundary of Egypt and going westwards are Lakes Birdaweel and Umm Farag between El Arich and Port Said, Lake Menzaleh between Port Said and Damietta, lakes Bouroullos and Edkou are between Damietta and Alexandria, and Lake Mareotis behind and to the west of the latter place. The two first mentioned receive no fresh water feeders, and on this account their sea entrances, on which they depend for their existence, often become silted up and require to be kept open by the tenant of the fishings. The fauna is entirely marine, so they can scarcely be placed under the present category. On the other hand, Lake Mareotis, although it receives fresh water from numerous canals or drains, does not communicate directly with the sea, the pumping apparatus required to keep the level of its waters low preventing the entrance of marine forms. The three other lakes each possesses a sea entrance, and is connected with the Nile system through receiving the drainage water and flood discharge of several irrigation canals. Lake Edkou was, till it was closed last year, fished in a manner similar to the *valli* of the Italian Adriatic. The sea entrances of Lakes Bouroullos and Menzaleh remain unobstructedly open and are centres of much activity when the spawning instincts of the lake fish cause them to move seaward. Taking Lake Menzaleh as the type, it is shallow, its depth on an average being about 140 c.m., but in the neighbourhood of the sea entrance, there are several converging channels of a depth varying from 3 to 6 metres. A current flows constantly through the entrance with a direction and strength governed by the state of the tide, the direction of the wind, and the inflow of fresh water from the canals.

The fish fauna consists of a mixture of fresh-water and marine forms, of which the latter predominate, and their relative distribution is regulated by the density of the water, which varies in the several parts of the lake at different seasons. With the exception of the Mormyridae, all the families of Nile fish are represented and two species at least breed in the lake. These are *Chromis niloticus* and *C. menzalensis*, and while the former spawns in the neighbourhood of the fresh-water inlets, the latter breeds in water as salt as, if not salter than, the sea itself. These two forms and the *Silurid Clarias*, if we exclude *Anguilla*, are the constant members of the fresh-water species, the occurrence of others being more or less accidental. At the period of high Nile the lake receives numerous additions to the numbers of its fresh-water species. Of the marine fauna, the Mugils are the most abundant. Five species are found in the lake, and two of these—viz., *M. cephalus* and *M. capito*—are of the highest economic importance. They are taken at all seasons of the year, though at certain periods more abundantly than in others, due either to their local occurrence during the spawning season or to certain conditions of the water. *M. capito* is more generally diffused throughout the lake than the larger species, and it shows a greater preference for fresh water than the latter, which in the adult stage is always found near the deeper and salter localities. Neither of them attain the maximum recorded size of their species. *M. capito* is rarely found to exceed 40 c.m. in length, and 58 c.m. is about the maximum for *cephalus.* This would appear partly to be the result of overfishing, but observations show that in the case of *cephalus* few spent individuals return to the lake after spawning is over, and that nearly all the fish of the spawning shoals which leave the lake are virgin fish. Older fish are to be found in the Suez Canal, which has yielded individuals of over 84 c.m. The spawning season of *M. cephalus* lasts from May till the beginning of November, a period which seems to be interrupted for from six to eight weeks in the months of August and September when only a few scattered spawners leave the lake for the sea. *M. capito* spawns from the first week of November till about the end of December. They leave the lake in shoals, which return empty after one day or two, then after an interval, during which their reproductive organs fill again, they migrate seawards a second time. At least two crops of ova are ripened in a spawning season.

As before mentioned, the grey mullets are used for the manufacture of fessikh, and they form on an average nine-tenths of the total catch landed at the markets.

Next in importance come *Morone labrax* and *M. orientalis.* They spawn from about the end of December till March, the season of the smaller fish lasting the longer of the two. *M. labrax* is in little evidence and *orientalis* not at all during the months of summer, but from October till the end of the spawning season they are caught in large numbers.

Sciena aquila and *Umbrina cirrhosa* are found in the lake throughout the year. They would appear to spawn sometime in May or June, but no ripe specimens have been collected.

Chrysophyrs aurata, which is found in the salter parts of the lake, leaves to spawn during the months of November, December and January.

The annual spawning migration of the common eel takes place in December, January and February, when the moon is in her last quarter. They leave after sunset or in the late afternoon should the sky be overcast.

Other forms occur less commonly as *Serranus cabrilla*, *Hemiramphi*, and some Carangidae, while among the more accidental may be mentioned *Balistes*, which has been taken in good condition ten miles from the sea entrance.

Fishing is usually prosecuted from boats which are manned, on an average, by a crew of four men not including boys. They fish either singly or in companies of as many as fifteen boats. Nets and hooks, baited and naked, are used. Of nets, the mullet seine is that in most general use. It is carried round a shoal of fish, which has been driven towards it by men who support the upper edge of the net about three feet above the surface of the water by means of poles to prevent the escape of the fish. In the Suez Canal this system cannot be employed on account of the depth of the water, but a trammel net supported horizontally on reeds is attached to the outer edge of the seine and receives the fish as they jump the corked line. Fixed trammels, trammel seines, and drag nets are also used, while the circular casting net is a great favourite with the fishermen, who are very adept in its use.

Much damage has been done to the fisheries by denying to them the protection of properly constituted bye-laws. Till two years ago no attempt was made to regulate the capture of fish, and even now the size of mesh used is that which suits the convenience of the fishermen. Lately it has been forbidden to fish on grounds which were ascertained to be the nurseries of young fish; formerly forty or more boats' crews would combine to sweep the fish, chiefly young grey mullet of an average size of 17 c.m., from the lagoons on the margin of the lake which fish of that size frequent. As much as fifteen tons of these fish have been taken in a single haul. A close time of eight days has been enforced during the last two spawning seasons of *M. cephalus* for the protection of this fish, and the indications of success following the institution of this and other protectionary measures promise, under proper control, a great future for the industries in question.

J. C. MITCHELL.

V

The Red and Blue Colouring Matters of Flowers

IT is rather difficult to refer to an exact date the sober beginnings of our present knowledge regarding the chromogens or the visible colouring matters of flowers and leaves. Schubler and Decandolle endeavoured to prove the existence of two essentially different series of flower colours, viz., the *xanthic* producing the yellow tints with their transitions into red, and the *cyanic* producing the blue tints with their modifications, and that the colours of both series are formed from chlorophyll, the xanthic by oxidation and the cyanic by deoxidation. This view may, however, be considered as the result of mere conjectural surmise rather than that of a definite chemical investigation. The researches of Filhol, Cloez, and Fremy served to elicit the distinction between the blue and red pigments which are soluble in water, and many of the yellow which are of a resinous nature and dissolve only in alcohol and ether. The blue pigment (cyanin or anthocyan) with acids makes red flowers, alkalies turn it green ; and hence it was concluded that the blue colours of flowers are not produced from the red by the action of alkalies. They found that blue, violet, red, brown, and orange flowers have only one colouring matter, while yellow flowers have two, viz., xanthin insoluble in water, and xanthein soluble in water. According to Cloez and Fremy all red and rose flowers have an acid cell sap, and the colour of this sap would be due to a modification, under the influence of an acid, of a colouring matter (cyanin), which is found likewise, but in the state of greater purity, in blue flowers whose sap is neutral. Finally, it was recognised by Filhol that the flowers of poppy, *Pelargonium*, *Camellia*, and *Salvia* contain a pigment which is more stable than that of most other flowers, that they contain no resin or xanthogen, and that when treated with alkalies they assume a blue or violet colour without any green. Such were some of the earliest researches on this intricate and most interesting subject, and the fundamental error or defect which characterised them all was that the rose, blue, and violet flowers owe their tints to one and the same substance, influenced by the reactions of the vegetable juices, this substance (cyanin) itself being a b l u e uncrystallisable mass soluble in water and alcohol, but insoluble in ether.

This mistake is one which, in the absence of any definite knowledge anent the differences in kind and the sources of the various tannic chromogens, might have been easily made by anyone who experimented on flower colours with alkalies and acids as his only reagents. By the employment of neutral reagents, however, such as acetates of lead and magnesium, the results are such as to afford ground for distinction between the various hues and tints. For instance, the fine green precipitate yielded by acetate of lead with the colouring matter of the rose tribe is turned red again by acetic acid, but the similar precipitate given by the blue pigment of harebell corolla is turned blue by this acid; and with acetate of magnesium and acid the effects are similar. The blue of scabious when treated with acetate of lead yields a similar green colour and precipitate, but on adding acetic acid we have a red liquid and a still green precipitate, the portion lying under the acid liquid gradually turns blue, while the portion outside the liquid, though still bathed therewith, remains green. This interesting experiment seems to clearly suggest a radical difference in the composition or constitution of the normal red and blue pigments of flowers, but likewise proves the fallacy of the early view that 'rose, blue, and violet flowers owe their tints to one and the same substance influenced by the reactions of the vegetable juices.' Similar experiments even seem to show that certain distinctively red flowers, such as camellia, owe their tint to a substance which is not the same as that of other red flowers, such as rose; but in this case there is more than a suspicion that the real and original colour of the former kind is blue and not red, and that artificial means could practically effect the change. Even acetate of lead when used alone to react on various red colouring matters of vegetable origin produces results which argue an essential diversity therein. Thus, with genuine red wine it yields a greyish-blue or greenish colour, with the colouring matter of bilberries it gives a blue precipitate, with that of mallow or elderberries a green, and with that of phytolacca berries a red-violet precipitate. V. Vogel, experimenting later, found that by using a concentrated solution of sulphate of copper the colour of new or old red wines was decolourised, although the fresh pellicle of the grape itself is coloured violet by this reagent, as is also the extract of cherry juice; while that of bilberry remained unchanged. By the action of sub-acetate of lead bilberry juice is completely decolourised, while that of cherry is not decolourised in the presence of alcohol or of red wine. The spectroscope has also been employed to distinguish between these diverse pigments, but its revelations need not be referred to here, especially as I am disposed to consider them to be not so very valuable owing to the extreme difficulty of isolating completely the different colouring matters.

A decided advance was made towards a veritable chemical comprehension of the subject when Morren declared that the red and blue colouring matters are not formed from chlorophyll, and when Marquart observed that cyanin appeared by its characters and its little stability to approach the paracarthamin obtained by the action of sodium amalgam on quercitrin and quercetin, a discovery that immediately originated a crop of surmises, the ground whereof was, that paracarthamin, similarly as cyanin, was also turned green by weak alkalies and by acetate of lead. Thus, for example, Stein, in a short and pleasant paper to the *Jahrb. f. praktische Chemie* for 1863, distinctly avers, that in most red flowers which he examined the red colouring matter appears to be paracarthamin, and this also is the same body which is found in blue flowers; the blue pigment is none other than the saline (calcium) compound of the red matter. Nachtrag, too, in a note on the 'Natural Occurrence of Paracarthamin,' concludes, from the behaviour of the red bark of dogwood (*Cornus sanguinea*) towards alcoholic potash solution agreeing with that of the pigments of dahlia towards the same reagent, that it likewise contains paracarthamin, and he adds, "the red that can be artificially produced from plant-yellow appears, therefore, to be repeatedly present ready formed in the vegetable kingdom." A certain weight of probability was imparted to these views by the fact alluded to by Stein, viz., that quercetin had previously been shown by Rochleder to exist in the flowers of the horse-chestnut, and by the circumstance that in the flowers of *Aesculus pavia*, the origin of the red pigment from the yellow can be clearly followed, as this flower when it comes forth from the bud is yellow and only by little and little turns red. If this be really the *modus operandi* as existing in nature, it would seem to follow that the bright pigments are really reduction-products and not oxidation-products—a consequence which is not usually met with in analogous cases in other departments of biology. It is now pretty clearly ascertained that the yellow rutin or quercetin present in flowers does not, on account of the acid sap, impart a yellow colour thereto in the living condition at least, and that the tannic chromogen of the red flowers is really a development product of this or other intermediate tannoid glucoside. Moreover, it is a fact that certain tannic acids do yield on reduction with HCl and sodium-amalgam a red substance with reactions towards acetate of lead and alkalies similar to those of paracarthamin, but which nevertheless is certainly not the same as the latter body, although these same tannic acids appear to minister all the while as chromogens to the production of extremely vivid and brilliant scarlet and pure blue flower colouring matters.

The seemingly intimate relationship between paracarthamin and the red and blue pigments having thus by more explicit researches been dissolved, it became necessary for chemists to search for some other body which could possibly be isolated in sufficient purity so as to yield the mysterious green reaction with alkalies and acetate of lead. Everybody who has commenced the study of this subject is both pleased, puzzled, and surprised, by this reaction. There is a peculiarly vivid emerald-green brilliancy about it which is almost unmatchable. I have frequently prepared paracarthamin from rutin and quercetin extracted from plants, but the reactions thereof are incomparable with those yielded by the nature-formed cyanin, whether in petal or leaf. According to Wiesner the colour of the lead salt ought to be really blue, the actual green being caused by an intermixture of tannin in the solution; but experiments with carefully purified tannin show that the colour is actually a dark bluish-green. However, to resume, it would appear that the researches of certain micro-chemists such as Nägeli, Wiesner, etc., revealed for the first time the important fact, that certain astringent matters were found in parts of plants which become red or blue, nay even the actual colouring matters seemed to spring up as it were grasped into the very substance of the tannin itself. It was natural, therefore, that Wigand should point out that the chromogen of anthocyan might be regarded as perhaps not tannin, but some modification thereof, which he denoted as cyanogen, 'the transformation depending on a process of oxidation.' This extremely valuable declaration was, however, in a most marked manner left unnoticed in later works, until Lindt detected the relationship anew, and subjoined the conjecture that the production of the red pigment might depend on phloroglucin—a decomposition product of tannin, similarly as the action of vanillin on this phenol takes place, an action which results from the combination of two molecules phenol with one molecule aldehyde with elimination of water and production of a red resinous body. Waage, however, in 1890 recognised the fact that the presence of phloroglucin in a plant was a by no means absolute requirement for the coming forth of a soluble red pigment (anthocyan), since, for instance many Chenopodiaceae are tinged strongly red, though no phloroglucin can be detected therein. It might possibly, of course, happen that on the production of anthocyan a consumption of phloroglucin took place, or perhaps it required a longer time to form in the light than the red matter which was suddenly created thereby in a colourless tissue, and hence in either case it would not be detected; but then again it may be objected that the metabolism would be so much increased by the action of light that even a wholesale formation or substitution of phloroglucin in the briefest time would be accounted for. Thereupon Waage performed an experiment forthwith on the seeds of buckwheat, which indicated the probability of some though only a slight consumption of phloroglucin accompanying the rapid formation of the red colouration in etiolated seedlings which are suddenly brought into the light. It was also observed that in the same cells where phloroglucin can be detected, tannin-reactions also take place; but the reverse was not the case, as many cells contain tannin without phloroglucin being present therein. On the whole, Waage concluded that phloroglucin takes part in the formation of phlobaphenes, tannin-reds, and, besides the tannin acid, it has a share also in the brown colouration of dead or dying autumn leaves.

Although the researches just mentioned were conducted mostly after the micro-chemical method, nevertheless their vast range and extraordinary minuteness of detail have supplied results which have proved of the utmost value to all practical students of this interesting theme. Nothing could be more suggestive than to be apprised regarding the precise distribution of phloroglucin, to know that it exists in one plant and does not exist in another, and sowise to lean gradually to the understanding that, although its function in the formation of certain red colouring matters stands pre-eminent, yet there are certain other red pigments with the origin of which it has got nothing whatever to do. It does not require a specially delicate eye for colour to see that the red of the rose is different from that of the foxglove, that of the rhododendron from that of the daisy, etc., and such being the case, it is reasonable to surmise or conclude that they have a different origin. What then does there remain for us to state or summarise with respect to the precise chemical causes which bring about the outcome of these brilliant red and blue phenomena of the vegetable kingdom? In a general way it may be admitted as established that, in Dicotyledons at least, where the red colour of flower or leaf verges towards bluish, as for instance in the rose and rosaceae generally, there the phloroglucin group enters into the molecule of the chromogen; where, on the other hand, the red is more scarlet, the chromogen seems to be of the nature of an acid, and the actual visible pigment is only a saline compound modified by exposure to the air and by certain substances gums, mucilages which accompany it and even possibly give origin to it. Both forms of tannic chromogen arise from a carbohydrate which, according to its chemical component and constitution, gives origin to certain intermediate tannoid compounds which seem to form, on the one hand, phloroglucin-tannin, and on the other, protocatechuic acid-tannins. The former class of tannins when oxidised or dehydrated produce the actual crimson colouration, and for this purpose they do not require to enter into combination with a base; the change seems to depend on what in chemical parlance is termed condensation, *i.e.*, the formation of an oxide or simple ether by the elimination of H_2O between two HO groups in different molecules. It has been surmised that the action is analogous to the action of aldehydes on phenols in the presence of dehydrating agents, as Lindt had originally stated (see above), but experiments conducted with phloroglucin, various aldehydes, and HCl produced in every case a crimson resinous matter which was not turned green by alkalies and acetate of lead. This green reaction is however yielded by a substance which is obtained by heating the lead or zinc compound of the pure tannin of the horse-chestnut, etc., with dilute HCl, filtering off the phlobaphene and shaking the red filtrate with amyl alcohol, which dissolves a red substance which is washed with cold water and dissolved in ethyl alcohol. This seems to be the most successful attempt that has yet been made in the way of producing by synthesis the anthocyan and erythrophyll of flowers and leaves; and there seems little doubt that some such process as this is brought about in nature by the concentration of the cell sap through increased transpiration, etc., from these organs. It is as well to mention that, according to Jodin, light does not exert a photo-chemical action on tannin, although it does so on chlorophyll and carotin. Also that MM. Gautier and Girard experimenting on the regulated oxidation of the tannins came to the conclusion in 1877 that the red colouring matter of, *e.g.*, wine is only one of the transitory oxidation products of the oeno-tannin contained either in the skin or pulp of the grape. Tschirch likewise has pointed out that in the cell sap the tannins are oxidised directly (or, if they are glucosides, after resolution into their constituents) to red-brown phlobaphenes, some of which are formed very slowly, while others are formed very rapidly.

The latter class, viz., the protocatechuic acid-tannins, may be regarded as chromogens which evolve colouring matters not out of their own molecules by condensation and dehydration, but possess the property of forming coloured salts which turn blue, red, or even green when exposed to the air. It is possible, indeed, that they contain a salifiable group (chromophor) in their chemical structure; they are more entitled to be called tannic acids than the aforesaid class are. Their most important peculiarity, however, patent to everybody, is that they can embody a true blue flower. No one has ever seen, or ever shall see, a blue rose; but a blue dahlia or even a blue daisy is quite possible. The very fact of this possibility emanating, so to speak, from a source which is distinctly acid argues a power of fixation which is something stronger than that shown on the formation of a mere 'lake.' It was astutely observed by Stein that decidedly blue flowers, such as corn-flowers, contain a large quantity of calcium, phosphoric acid, and pectin. From experiments which I have recently made, I am much disposed to conclude that the presence of a certain kind of pectin associated with certain inorganic bodies explains the production of blue from the tannic chromogen, and likewise the fact that even in an acid medium the combination between these bodies is so complete that no separation or reddening occurs.

P. Q. Keegan.

SOME NEW BOOKS

"PARKER AND HASWELL"

A TEXTBOOK OF ZOOLOGY. By Professors T. J. Parker and W. H. Haswell. 2 vols. 8vo. Pp. 1400, with 1173 figs London: Macmillan & Co., 1897. Price, 36s. net.

THERE appears to be no end to the making of Textbooks of Zoology, of one kind or another, to suit all sorts of readers. At one period Germany was manufacturing them one after another; at the present time the French zoologists are bringing out three or four textbooks, each on its own peculiar lines; and now it seems that we are to have a succession of similar productions by English authors. The generality of these last have been translations, or adaptations, or compilations, and if we except the "Cambridge Natural History"—which appeals, perhaps, to a different public—we have had no really original English textbook dealing with the entire animal kingdom in an adequate manner since Huxley's two small volumes on the anatomy of the vertebrata and of the invertebrata were published in 1871 and 1877 respectively.

It is, therefore, not surprising that "Parker and Haswell" has been looked forward to with expectation for some time. We welcome it gladly now that it appears: it should be in the hands of all students, and even teachers will find it of value. But a feeling of sadness and regret cannot fail to come over us, as we open the book, and remember that one of the two authors did not live to see the issue of his work.

The first volume deals with the invertebrata, the second with the vertebrata—in all there are 1400 pages and 1173 figures; and the craniata alone occupy 523 pages and are illustrated by 450 figures—which is sufficient to show the thoroughness with which the group is dealt with.

There is a good index at the end of each volume, and the whole book is attractively got up; the printing is clear, and the figures are mostly good, many original and some excellent. We are warned, in the preface, that we must not expect a very "up-to-date" book, for it is two years since the proofs were finally corrected by the authors, who lived 1200 miles apart, and were separated from their publisher by half the circumference of the globe.

The plan of the work reminds one of Huxley's once much-used Manuals. A 'type,' or, as our authors prefer to call it, "an example," of each class is described in detail; this is followed by the "characters" of the class, and of the orders and suborders into which the class is divided; the systematic position of the 'example' is then pointed out (too insufficiently to be of use, we think); and the general structure of the entire class is discussed; together with habits, distribution, and the relations of the orders and classes of the phylum.

There may be two opinions about this plan, but as the book is specially intended for a beginner, who has no previous knowledge of zoology, it is probably the best suited for the purpose. After all, it is the plan followed in a general course of lectures on zoology, and the book becomes to some extent useful in the laboratory. But we wish that a greater uniformity had prevailed in the way in which the plan is carried out; for whilst in most cases, such as the Vertebrata, Arthropoda, Mollusca, a few representative genera are mentioned under each of the small divisions of the class, in other groups, *e.g.*, Annulata, Echinoderma, Coelentera, these are omitted, and the student will find it impossible to locate the various genera mentioned in the subsequent account of the general structure of the group as exhibiting modifications in this or that organ, as compared with the type.

Again, it would have added very considerably to the value and interest of the book, both to English and to Australasian students, if there had been uniformity in the plan of stating whether the 'example' is a Northern or a Southern form.

For instance, *Petromyzon* is taken as the type of the Cyclostomi; but we are told, in the opening sentence, that it is a Northern form, and is represented in the Southern hemisphere by *Geotria* and *Mordacia*. In the case of *Astacus*, however, we are merely informed that it is represented in the South by allied genera; but the fact that *Paranephrops* is the New Zealand genus is only mentioned in the chapter on geographical distribution at the end of vol. ii. [It may be noted that *Paranephrops* and *Asthenosoma*, and some other genera mentioned in the text, are not to be found in the index.] But in the Annulata and Echinoderma no indication is given as to whether the type occurs in Australasia or not; there is nothing in the immediate text to show that *Lumbricus*, the type, is represented by *Acanthodrilus* in New Zealand.

The classification adopted will, perhaps, scarcely satisfy specialists in the various groups; nevertheless, there is much to be said against the tendency, so frequent at the present day, of introducing new 'systems' into books which are, after all, intended as a guide to students. These new classifications are really stumbling-blocks to a beginner, for in reading, as he should do, other text-books or zoological classics, he will meet with different systems which he has not sufficient knowledge to co-ordinate with the more modern one which he has recently read. Moreover, the beginner is unaware of the instability of 'systems,' and naturally thinks that the facts of the science being firmly established (as he believes), the deductions from these facts should be stable also. Nevertheless, there comes a time when a rearrangement must be made, and such a sub-division as that of the Crinoidea into Neo- and Palaeo-crinoidea should be given up. So, too, the groups 'Myriapoda' and 'Gephyrea' ought to be split up, and the forms hitherto included distributed to their proper places.

We are glad that the authors refuse to see in the Polyclad Turbellarians any close relationship with the Ctenophora, but rightly place the Rhabdocoelida at the base of the class, and recognise that *Ctenoplana* and *Coeloplana* are merely slightly modified Ctenophores.

Dinophilus is placed with the Rotifera and Gastrotricha, as a phylum

Trochelminthes. In the account of the Nemertines no reference is made to the important difference in the position of the nerve cords with respect to the musculature in the various orders, that was pointed out long ago by Hubrecht. *Balanoglossus*, *Cephalodiscus*, and *Rhabdopleura* are placed, but with some hesitation, with the Chordata.

Amongst the novelties of the book may be mentioned the number of fossil vertebrates described and figured, the amount of embryology of each type, and the phylogenetic trees at the end of each Phylum. No doubt the last have their use, but one or two of them do not appear to agree with the text. Thus, on p. 483 of vol. i., the tree representing the Annulata, shows a 'chaetopod' stem rising from the Archiannelida, and ultimately bifurcating into Oligochaeta and Polychaeta. This is all right, but the Gephyrea and Hirudinea are seen issuing, one on each side, from this common stem. This does not indicate the, probably, true course of events, nor, indeed, does it agree with the statement that "the Hirudinea diverge somewhat widely from the Chaetopoda, but a study of their earlier developmental stages shows unmistakably their close connection with the latter group, more particularly with the Oligochaeta."

There can be little doubt but that the Leeches are degenerate Oligochaeta—for have we not a Leech with bristles ?—and they should therefore have been represented as springing from that class and not from the common stem. The armed 'Gephyrea,' too, can be traced step by step to the Polychaeta.

The description of many of the examples will be found very useful, some because of their novelty as types, others as being local forms. Thus, *Triton nodiferus* represents the Gastropoda, instead of the snail; *Anthenea* stands for *Asterias*; *Chiloscyllium fuscum* replaces *Scyllium canicula*.

The anatomical account of *Apus* and of *Hormiphora* will be welcome, and especially interesting is that of *Callorhynchus*. But some of these descriptions lose in value owing to errors, both of commission and omission. The figure of the transverse section of *Nereis* is wrong in the limitation of the coelom; there is no mention of the 'ciliated organ' which was discovered and described by Goodrich in 1893, and is now known to be of considerable morphological importance. The authors clearly recognise that the Polychaete nephridium, or 'diplo-nephridium,' differs from that of the Oligochaete, which is an 'ectonephridium.' Goodrich's observations have a direct bearing on this distinction.

Is it true that "in *Nereis dumerilii* there is only a single pair of testes, situated in one of the segments between the 19th and 25th ?" If it is (and we doubt it) it is so exceptional a limitation amongst the Polychaeta that it should be specially mentioned as such.

In the account of *Cucumerina*, we meet with the astounding statement (p. 372) that in the respiratory trees, "each of the terminal branches ends in a ciliated funnel opening into the coelome." Is there, here, a confusion with the sessile, ciliated cups (without external outlet) of *Chirodota* and *Synapta?* or with the posterior nephridia of Echinids? On p. 439, as well as in the account of the earthworm, the nephridial funnel is said to open into the coelune in the segment "corresponding to" the external pore. Surely everyone knows the fact, peculiar as it is, that the nephrostome of Chaetopods is always in the segment preceding that of the pore—though the significance of this arrangement is unknown. These are samples of sundry errors; and others of less note occur in the descriptions of some of the types. If definite examples are taken to illustrate the structure of the group, it appears to us that every statement in connection with them should be as accurate as dissection and careful observation can make them. Some of the errors will be discovered by any student who dissects the type with any care at all. Also, we fail to see why the nephridium of the type (*Lumbricus*) is illustrated by a figure of that organ of *Tubifex*; it would have been quite as easy to borrow the block of Gegenbauer's figure of the earthworm nephridium, as that of Lang's.

On p. 569, the statement occurs that "in *Scutigera* one of the stigmata is unpaired and dorsal": the fact is that in this Chilopod, they all have this position; no mention is made of the peculiar organ into which they lead. On p. 612, the abdomen of *Limulus* is said to consist of "seven firmly united segments" On p. 627, it contains "only eight segments", but Lankester showed years ago that the abdomen consists of twelve fused segments.

In vol. ii., pp. 137 and 158, we read that the vertebrae of Selachians contain "bone"; this is a bad error, since calcified fibro-cartilage is meant, which is not the same thing as bone, either histologically, chemically or developmentally.

We have referred to the abundance and general excellence of the illustrations, but it seems strange that it is necessary to illustrate *Apteryx* by a photograph of a stuffed specimen in the Museum of the Royal College of Surgeons, London. The picture of the egg of *Callorhynchus*, with the embryo within, is especially good, and we are glad to see those of foetal marsupials with the embryonic membranes; there are good ones of Aurelia, Sponge anatomy; but one featue of the pictures is the diagrammatic dissection or solid section, *e.g* of Amphioxus; the ideal vertebrate is also a striking picture.

But some err in the attempt to show too much; two or more less elaborate figures would have been clearer. Thus, the dissection of the Leech, fig. 373, does not give a true idea of the alimentary canal, since among other things the coeca are represented both in a state of distension and of contraction on the same side, and there is a figure (338) of seven bristles of different Polychaeta without any indication, anywhere, as to what genus each bristle represents. Some figures are attributed to wrong authors, or the author is not mentioned; thus fig. 269 is by Van Bemmelen, and fig. 259 by Allmann, fig. 165 is by Julin—these names are not mentioned.

An inexplicable error occurs on p. 188, vol. i. Fig. 137 is Lacaze Duthiers' well-known picture of the Red Coral, but it is described as "*Cirripathes anguinea*" (after Bronn). A curious transposition of the legends of two phylogenies occurs on pp. 580, 582 of vol. ii.

It is a notoriously difficult task for one, or even two, men to write a Text-Book which shall be free from errors, and satisfy the various specialists who may have to review it, for each is apt to consider his particular group as the group that should be properly done. The present book is no exception. Nevertheless, in spite of several errors and omissions, the book will be found very good of its kind; it is so well illustrated, so clearly printed and generally good, that it will be found a useful addition to the student's and teacher's shelves.

Apart from the descriptive matter, special reference should be made to the chapters at the end of the second volume on the Philosophy of Zoology, the History of Zoology, and on Geographical Distribution, with especial reference to Australasian forms. These subjects are rarely read by beginners, and we commend these chapters to their consideration, and hope that, bound up as they are with the more 'paying' matter, they will spare some hours from their 'schools'' work to study the history of these subjects. There is one noticeable difference from most modern books, and that is an absence of literature in the body of the book. It may be true that it is impossible to give a complete or sufficient bibliography within reasonable limits, but it is possible to give a carefully chosen and representative bibliography, and it is advisable, we think, that a student, even a beginner, should be induced to read original memoirs. It is more stimulating than a Text-Book, and gives him a fuller knowledge of how the facts of the science are discovered and verified and correlated; and we ourselves have always found that beginners take a great deal more interest in zoology, if they can be induced to read occasionally special papers and memoirs.

In the "Guide to Literature" at the end of the present book the majority of references are to students' Text-Books of one kind or another; only a few monographs are referred to, and many important ones could have easily and appropriately been added. It is, however, a good idea to add a list of such periodicals as the *Zoologischer Anzeiger*, the *Centralblatts*, &c., wherever current literature may be found.

THE RICHES OF DRESDEN

GUIDE TO THE ROYAL COLLECTIONS AT DRESDEN. Translated by C. S. Fox. Published by authority. 8vo, 290 pp. Dresden, 1897. Price, 1 Mark.

THE number of English speakers, especially Americans, who visit Dresden every year, many of them living there for some time, has caused the authorities of the museums and picture galleries in that town to issue an English edition of their handbooks. We find herein an account of the Zoological, Anthropological, and Ethnographical Museum, which, although the successor of a cabinet of art and natural curiosities formed by the Elector Augustus, 1553-1582, practically dates from 1849, in which year the museum was burnt by the revolutionaries. Following this is a short description of the Mineralogical, Geological, and Prehistoric museum, which has had a similar history to that of the zoological collections, and is now placed in the west wing of the Zwinger building, the zoological collections being in the south wing. These guides are intended for *das grosse Publicum*, not for the scientific student: they do not, as do the guides of our British Museum, give any connected account of the objects contained, but content themselves with drawing attention to the more striking among them. They are probably no less useful to the visitors for whom they are intended, but do not form such interesting reading at home.

G. Fischer, of Jena, announces the publication of a "Fauna Chilenis," based upon the collections made by Prof. L. Plate. The separate sections of the work are undertaken by some twenty-seven specialists, whose results will be published as supplementary volumes to Spengel's *Zoologischen Jahrbücher*, but will be obtainable separately at a somewhat higher price.

Serials

The Royal Botanic Gardens, Ceylon, have recently begun to issue a circular intended to deal with the agricultural, horticultural and botanical work there carried on.

The *Science Teacher* is a new monthly started in New York by Mr A. T. Seymour, instructor in science and mathematics in Westminster School, Dobbs Ferry, N.Y.

The *Philadelphia Medical Journal* commenced publication in January. The editor is Dr. Geo. M. Gould, and he is helped by many leading physicians of Philadelphia.

The New York weekly, *Garden and Forest*, is dead. It was founded about ten years ago by Prof. Sargeant of the Arnold Arboretum, and was edited by the late Mr W. A. Stiles.

The *American Archaeologist* (formerly *The Antiquarian*), published at Columbus, Ohio, contains useful and valuable matter dealing with the primitive peoples of North America in particular. This year it commences volume ii., and the subscription is $1.50.

The February number of *The Photogram* contains two articles of interest to our readers. One begins a series on Anatomy in Portraiture; the other is a short practical note by A. E. Livermore on a means of producing stereoscopic effects with Rontgen photographs.

The section of Archaeology and Palaeontology of the University of Pennsylvania has just begun the publication of a review entitled *The Free Museum of Science and Art*. It is intended to give an account of accessions to the museum, notes on the collections, and short scientific papers.

The following entry occurs in the contents on the wrapper of the *Journal* of the Society of Arts for 4th February, "Correspondence:—Recreations of an Indian Official—Dry Rot"! The "Recreations" contain, however, an interesting list of the native names of Indian plants by no less a person than Sir George Birdwood.

L'Intermédiaire des Biologistes (see *Natural Science*, January, p. 63) continues to fulfil the promise of its earlier numbers. The following questions are pretty certain to find answerers among our readers: **121**, What museums contain remains of either skeletal or soft parts of *Rhytina borealis?* **130**, Where and when did Mr F. Galton publish his last researches on heredity? Replies sent to us will be forwarded.

Messrs Munn & Co., 361 Broadway, New York, have kindly sent us a cloth-bound copy of their index to the more important papers contained in the *Scientific American Supplement.* We often refer to the *Scientific American*, but we are not favoured with the *Supplement*. Messrs Munn tell us that it is "a unique journal, containing no advertisements." The issue of this index is a unique and useful way of advertising it. If every periodical followed the example of the *Scientific American* we then should accumulate a fine subject index of contemporary literature, as complete as that which Mr Cotgreave is trying to compile.

The *Westminster Review* for February has a charming bit of sarcasm in a review of a book on birds by a Dr W. T. Greene. It appears that he follows the classification adopted in the Gardens of the Zoological Society, not because he approves of it, but for the convenience of his readers. On this our contemporary remarks: "That classification is very unsatisfactory, and it would be a good thing if the society were to consult such an authority as Dr Greene, instead of leaving the subject in the hands of mere amateurs." Our eminent ornithologist, Dr P. L. Sclater, and that skilled writer on bird anatomy, Mr F. E. Beddard, will chuckle over this phrase. Unfortunately, people more ignorant than the Westminster reviewer are apt to take irony so very seriously.

We have received the first three livraisons of volume vii. of the *Actes de la Société Scientifique du Chili.* Dr. C. Pérez Canto has an article on the reproduction of certain bacteria from the point of view of their classification. Gustavo Undurraga contributes some detailed anatomical investigations of the pneumo-gastric nerve. E. Riggenbach describes a new species *Bothriotaenia Chilensis*, found in the intestine of a carnivorous gadoid fish living on the coasts of Chili, known as *Genypterus Chilensis.* F. Gautier writes on the presence of antimony in certain tin minerals of Bolivia. F. Lataste discusses several remarkable cases of teratology, and contributes what he calls a study of rational biology, "Symmetry in Living Beings." There are also minor papers by F. Lataste and A. Dugès, and notes by Dr Otto Nordenskjöld on Patagonia.

FURTHER LITERATURE RECEIVED

Traité de Zoologie Concrète. v. Vermidiens, Delage et Herouard: Schleicher, Paris. Elementary Botany, Groom: G. Bell. Elementary Physics, Kerr: Blackie. Die Gattung Cyclamen, Hildebrand; Lehrbuch der Entwickelungsgeschichte des Menschen, Kollmann; Organographie der Pflanzen, Goebel: Fischer, Jena. Andrée and his Balloon, Lachambre and Machuron: Constable.

The Gipsy Moth in America, Howard: *Bull. U. S. Dept. Agriculture.* On an Index to Literature, Cotgreave. Die Resultate der Tiefseeforschung, Chun. Catalogue of Zoological and Palaeontological Works, No xlii. Echinodermata., xliii. Vermes: Dulau. Catalogue of the Hadfield Collection of Shells: Manchester Museum. Anthribidae from the Islands of Engano, &c., Jordan: *Ann. Mus. Genova.* Studies from the Yale Psychological Laboratory, vol. iv.

Actes Soc. Sci. Chili, vi. 4, 5, vii. 1, 2, 3; Amer. Journ. Sci., Feb.; L'Anthropologie, viii. (6); Bol. Mus. Paraense, xi., No. 2; Botan. Gazette, Jan.; Feuille des Jeunes Nat., Feb., and Bibl. Cat.; Irish Nat., Feb.; Journ. Malac., vi. 304; Knowledge, Feb.; Literary Digest, Jan. 15, Feb. 5; Mem. Soc. Cient. Mexico, x., Nos. 5-12; Naturae Novit., Dec. No. 24, Jan., No. 1; Naturalist, Feb.; Nature, Jan. 20, 27, Feb. 3, 10, 17; Chivers' New Book List, Jan.; Photogram, Feb.; Proc. Biol. Soc. Washington, vol. xii. pp. 5-30; Revue Scient., Jan. 22, 29, Feb. 5, 12, 19; Science, Jan. 14, 21, 28, Feb. 4; Scientific Amer., Jan. 8 and 15, 22, 29, Feb. 5; Journ. School. Geogr., Jan.; Intermediaire des Biol., i. 2 and 5; Journ. Conchol., ix. No. 1; Rivista Psichologia, fasc. 18; Revue Sci. Nat. Ouest., April 1897; Scot. Med. and Surg. Journ., Feb.; Scot. Geogr. Mag., Feb.; Westminster Rev., Feb.

OBITUARIES

LIEUT.-COL. CHARLES COOPER-KING, of Kingsclear, Camberley, who died, aged fifty-four, on January 16th, was an enthusiastic exponent of the natural sciences at the Staff College, Sandhurst. From 1886 to the time of his death he was lecturer on applied science there, and drew a large class of officer-students to geology, both in the lecture-room and in the field, for being a military expert, his explanations of the science in relation to military tactics and battlefields were well appreciated. He also added to our knowledge of the geology of Berkshire and of the local prehistoric man.

DR ALBRECHT SCHRAUF, professor of mineralogy at Vienna, and for many years custos in the Imperial Museum of that city, died in December 1897, aged 60. His "Lehrbuch der physikalischen Mineralogie" (1868) and his "Atlas der Krystallformen" (1865-1877) are the most important among many important books and papers on mineralogical subjects.

THE death of Prof. RUDOLF LEUCKART, of Leipzig, is announced. We hope to give some account of his life and work next month.

The following deaths are also announced:—Dr WALDEMAR V. SCHROEDER, professor of pharmacology in the University of Heidelberg and author of a number of treatises on physiological chemistry; CESARE CRETY, professor of zoology and comparative anatomy in Sessari, Sardinia, on Sept. 14, 1897; Dr MAX ZEPPELIN, the zoologist of Stuttgart, on Dec. 3, 1897, aged 41; Rev. WILLIAM HOUGHTON, ichthyologist, at Wellington, Somerset; JAMES THOMSON, the entomologist, well known as the author of the "Systema Cerambycidarum," and other monographs on beetles; ALESSANDRO LANZILOTTI, professor of veterinary anatomy at Milan, aged 40; F. ALEX. SLUDSKI, professor of geodesy in the University of Moscow, on Nov. 25, 1897; FRIEDRICH ADOLF HOFFMANN, the geologist, in Mexico; Dr EUGEN ZINTGRAFF, the African traveller, on Dec. 4, 1897, aged 39; ALFRED MONOD, the cryptogamic botanist, aged 61; HENRY N. BOLENDER, botanist, at Portland, Oregon; and KASIMIR VON PIETROWSKI, killed during the botanical expedition of Tatra, aged 20.

NEWS

THE following Appointments are announced:—Prof. Michael Foster to be Gifford lecturer in Glasgow University for the sessions of 1898-99 and 1899-1900; F. C. Kempson, of Caius College, Cambridge, to be demonstrator of anatomy at the University; Dr W. B. Benham, Aldrichian demonstrator in comparative anatomy at Oxford, to be professor of biology in the University of Otago, N.Z.; W. G. van Name, to be assistant in biology, and G. L. Bunnell, assistant in zoology, at the Sheffield Scientific School of Yale University; Dr Rhumbler to be professor of zoology in the University of Gottingen; Dr Sigmund Fuchs to be professor extraordinarius of physiology in the University of Vienna; Dr Abelous to be professor of physiology at the University of Toulouse; R. H. Biffen, of Caius College, Cambridge, to be demonstrator of botany at the University; Dr Julius von Istvánffy, of Budapest, to be professor of botany in the University of Klausenburg; Dr Alexander Mágocsy-Dietz to be associate professor of botany in the University of Budapest; Dr Richard Klebs, of Konigsberg, to be professor; Prof. O. Mattirolo, of Bologna, to be professor of botany and director of the museum and botanical garden at Florence; Prof. F. Morini to take his place at Bologna; Francis Ramaley, instructor in pharmaceutical botany at the University of Minnesota, to be assistant professor of botany in Colorado University, Boulder, Col.; Dr P. Zenetti, of the Pharmaceutical Institute of Strassburg University, to be professor of chemistry and natural history at the Dillingen Lyceum; Romul Alex. Prendel, of Odessa, to be professor of geology and mineralogy at Klausenberg; H. W. Pearson, of Christ's College, Cambridge, to be assistant curator of the herbarium of the University; Dr Otto Finsch, of Delmanhorst, to be director of the ornithological department in the Rijks Museum of Leyden; William J. Moenkhaus, of Indiana University, to be custodian of the Paulista Museum at San Paulo, Brazil; Frederic Philippi, professor of natural history and director of the Botanical Gardens at Santiago (Chili), has succeeded his father, the veteran Prof. Rudolph Armand Philippi, as director of the National Museum of Santiago, Prof. R. A. Philippi having retired at the age of ninety, after forty-three years' service.

DR ARTHUR WILLEY has been re-elected for one year to the Balfour Studentship in Biology at Cambridge University.

WE regret to learn that Sir J. William Dawson of Montreal is suffering from a stroke of paralysis and in a critical state of health.

MAJOR QUINCY of Boston suggests that £13,000 be appropriated immediately for the establishment of a marine aquarium in Boston.

PROF. J. O. EDMOND PERRIER has been elected *membre libre* of the Paris Academy of Medicine in place of the late Dr Magitot.

THE Hoagland Biological Laboratory of Brooklyn, N.Y., has received from its founder, Mr C. N. Hoagland, a mortgage for 24,000 dollars.

SIR NATHANIEL LINDLEY, Master of the Rolls, has been elected a Fellow of the Royal Society in virtue of his membership of the Privy Council.

THE Ragonot collection of Microlepidoptera and the Berthelin fossil Foraminifera are among recent additions to the Museum of Natural History at Paris.

THE Field-Columbian Museum of Chicago has purchased the complete herbarium of the late M. S. Bebb, which is specially rich in specimens of *Salix*.

THE Société d'Acclimatation of Paris intends to issue, in addition to its *Bulletin*, a monthly journal which will be largely devoted to discussion and correspondence.

THE U.S. Government has sent Mr B. E. Fernow, chief of the Division of Forestry, to Hawaii to make preliminary explorations and a report on desirable forestry legislation.

MR GEO. K. CHERRIE, assistant curator of ornithology in the Field Columbian Museum, has resigned his position in order to explore the region of the Upper Orinoco.

PROF. WM. LIBBEY, of Princeton University, intends to celebrate the annexation of Hawaii by taking four of his students there on a scientific expedition during the coming summer.

MR CLEMENT WRAGGE, the founder of Ben Nevis Observatory, and now Government meteorologist of Queensland, proposes to establish an observatory on the summit of Mt. Kosciusko.

PROF. O. C. MARSH has presented his unique palaeontological and osteological collections to Yale University, in which he has been honorary professor of palaeontology for thirty years.

PROF. H. A. MIERS has been granted the sum of £50 a year for five years to assist in the purchase of specimens and apparatus for the mineralogical department, Oxford University Museum.

ON December 18, 1897, a hall was opened at Bologna for the reception of the herbaria, preparations, and sections of the botanist Aldrovandi. It has been erected at the cost of the city and province.

THE Cambridge Archaeological and Ethnological Museum has received from Mr W. W. Skeat, District Magistrate of Larnt, Perak, a large collection of Malay native objects of great interest and importance.

THE Romanes lecture this year will be delivered in the Sheldonian Theatre, Oxford, on June 1, by Sir Archibald Geikie, who has announced as his subject, "Types of Scenery and their Influence on Literature."

IN accordance with the wish of the late Prof. John Tyndall, Mrs Tyndall has forwarded to the Royal Institution a cheque for £1000 to be disposed of as the board of managers may see fit for the promotion of science.

THE Rev. George Henslow, who has been appointed professor of botany to the Royal Horticultural Society, has undertaken to give demonstrations on the plants exhibited at a number of the meetings during the present year.

MR GEORGE SHARMAN, palaeontologist to the Geological Survey, retired at the end of last year, having served since 1855. We understand that he will be succeeded by Mr F. L. Kitchin, a graduate of Cambridge and Munich.

GOVERNOR BLACK of New York proposes the purchase by the State of a forest tract for the purpose of scientific cultivation, the administration to be in the hands of the Regents of the University or the Trustees of Cornell University.

MR JONATHAN HUTCHINSON, whose museum at Haslemere, Surrey, is well known, intends to establish an educational museum of a similar nature in his native town of Selby, Yorkshire, using for it the overplus material from Haslemere.

PROF. F. NOLL of Brünn, the literary executor of the late Julius Sachs, Professor of Botany at Würzburg, is now working over the unpublished manuscripts of the latter. An admirable account of Sachs by Prof. Noll, with a portrait, is given in *The Botanist Gazette* for January.

AT its meeting on 12th January, the American Academy of Arts and Sciences elected John M. Coulter, of Chicago, and Douglas H. Campbell, of Palo Alto, as Associate Fellows in the Section of Botany, and Elias Metschnikoff, of Paris, as Foreign Honorary Member in the Section of Zoology and Physiology.

ON the 29th January the Horniman Museum at Forest Hill ceased to be open to the public for a time. Mr Horniman intends to pull down the present building and erect a more suitable one as a memorial to Her Majesty's Jubilee. It is hoped that the foundation stone of the new building may be laid in June.

THE Museum of Rouen is being reorganised, and we learn from *La Feuille des Jeunes Naturalistes* that it is spreading into the building previously occupied by the School of Design. It will now be possible to display in a worthy manner the fine collection illustrative of Normandy geology formed by Mr Bucaille.

DURING 1897, says the same journal, all the specimens of the Noury ornithological collection in the museum at Elbeuf have been revised by Mr L. Coulon, who has published a catalogue of them through the Société d'Etudes des Sciences Naturelles of that town. In the same museum the Lepidoptera, especially those of Normandy, have been completed by Mr Dupont, while Mr Lancelevée has classified the Formicidae. Mr Raoul Fortin has been classifying the Quaternary rocks and fossils, while the Gastropoda have been revised and labelled by Mr Lhomme.

PROF. GUIDO CORA has resigned the professorship of geography at the Royal University of Turin, held by him for the last sixteen years, that he may devote himself to scientific research in geography and the allied sciences. His address, and that of his periodical *Cosmos*, will in future be 2 Via Goito, Rome.

THE Director of the U.S. Geological Survey has been instructed by Congress to prepare a map of Alaska, showing all known topographical and geological features, including gold-bearing rocks. The text, which will state the best known routes to the gold-fields, will also be issued. 40,000 copies are to be printed.

IT is interesting to learn, as we do from *Science*, that there is a scientific position in the U.S. Department of Agriculture, for which only women are eligible. It is the post of assistant microscopist to the Department. The microscopical inspection service has of late been greatly extended, and vacancies are to be filled in sixteen different cities.

PROF. K. MITSUKURI, the eminent zoologist of Tokyo, who has recently been in Washington as Japanese Plenipotentiary to the congress on the seal question, passed through England in February. He intends visiting Paris, Naples, and various German centres of learning, and hopes to be able to return for the Zoological Congress at Cambridge.

WE learn that the Fossil Plants at the British Museum, which have for years been divided between the Geological and the Botanical Departments, have now been merged into one collection. This is only one of many enlightened movements which have taken place recently in the Museum, and the new arrangement will be of especial benefit to botanical students.

EXPERIENCE has not accustomed us to associate the Roman Church with Science in any other position than that of antagonism. We are the more pleased to learn that, on the initiative of the Marquis de Maroy, of Wassy, a natural history museum is being established at the Vatican. Geological and mineralogical collections are already displayed in a large gallery.

It is stated that Captain Otto Sverdrup intends to take the *Fram* northwards up Smith's Sound, and then to make sledge expeditions over the northern part of Greenland. £1111 (20,000 kroner) has been voted by the Norwegian Storthing for the purpose of making such alterations in the *Fram* as will give more room for scientific work and render her more seaworthy.

A temporary post of observation erected on the peninsula of Rase Tarfa, opposite Massowah, by the Austro-Hungarian warship "Pola," engaged on a scientific exploration in the Red Sea, under the direction of Dr Franz Steindachner, was suddenly attacked by Bedouins on January 11. The attack was repulsed without loss by the personnel of the post, aided by the Turkish camp guard. The station was then broken up.

L'Anthropologie informs us that Count H. de la Vaulx has returned to Paris from Patagonia with valuable anthropological and ethnographic collections made throughout the whole of that vast country, and comprising both ancient and modern specimens. Among other things may be mentioned more than a hundred human skulls and skeletons. These collections have been offered to the French Government, and will be studied by the officers of the Museum of Anthropology.

At the recent meeting of the American Society of Naturalists, there was founded a new Society for Plant Morphology and Physiology. Abstracts of the papers read are given in *Science* for January 28. The officers for the ensuing year are:—President, W. G. Farlow; vice-presidents, J. M. Macfarlane and G. F. Atkinson; secretary-treasurer, W. F. Ganong. The next meeting of this Society will be held in December 1898, in conjunction with the American Society of Naturalists and the Affiliated Societies.

Among the sums given by the Swedish Government in aid of scientific research, we note the following:—To Dr C. A. Westerlund, to aid the publication of his work, "Synopsis molluscorum extramarium regionis palaearcticae," 700 kronor; Mr N. O. G. Nordenskjöld of Upsala, to aid in the publication of the scientific results of the Swedish expedition to Magellan Straits of 1895-97, 1000 kronor; to Baron A. E. Nordenskiöld in return for 30 copies of his work, "Periplus," described by us in our last number, 4500 kronor.

At the annual meeting of the Geological Society of London on February 18th, Mr William Whitaker succeeded Dr Henry Hicks as president, and Mr W. W. Watts succeeded Mr Marr as secretary. The following awards were made:—The Wollaston Medal to Prof. Ferdinand Zirkel, and the Fund to Mr E. J. Garwood; the Murchison Medal to Mr T. F. Jamieson, and the Fund to Miss Jane Donald; the Lyell Medal to Prof. W. Waagen, and the Fund divided between Messrs H. Woods and W. H. Shrubsole; and the Barlow-Jameson Fund to Mr E. Greenly. At the annual meeting of the Geologists' Association on February 4th, Mr J. J. H. Teall succeeded Mr E. T. Newton as president.

A scientific congress is to be held at Buenos-Aires on April 10-20 of this year, to celebrate the 25th anniversary of the foundation of the Société Scientifique Argentine. Among the sections are those of Biology, Anthropology, and Geology, including Mineralogy and Palaeontology. Subscriptions, 5 pesos (about £1), and announcement of one's wish to attend, should be sent to the President of the Committee of organisation at Cevallos, 269, Buenos-Aires. We have given some account of the vast riches of the La Plata Museum in the pages of *Natural Science*, and we can add from personal experience the assurance that any of our European colleagues who attend the meeting will meet with a hearty welcome.

THE *Scottish Geographical Magazine* quotes *Globus* to the effect that the Danish Commission for Geographical and Geological Researches in Greenland has received 150,000 kroner from the Karlsbergstiftung, to be devoted to the exploration of the east coast from Angmagsalik to Scoresby Sound. A party will be carried from Denmark in the autumn by a trading vessel to Angmagsalik, and will travel northward as far as it can reach before the vessel returns in the following year. On its report will depend the plan for the main expedition which in the summer of 1900 will be landed at Scoresby Sound, and having passed the winter there, will make its way southwards. Lieutenant Amdrup of the Danish Navy is the appointed leader.

THE Secretary of State for War has replied to the petition of the Guildford Natural History Society in reference to the preservation of Wolmer Forest as a sanctuary for wild birds, thus constituting a national memorial of Gilbert White of Selborne. In his reply, the Marquis of Lansdowne states that, while he is fully in sympathy with the aims of the memorialists, he considers the continuance of the existing system would, so far as it is in the power of the War Department, best promote the objects of the Society. Enclosed with the reply was a copy of a report by Captain A. H. Cowie, hon. secretary of the Aldershot Game Preserving Association, who has had the Government ground in question under his management since 1895. This report stated that since that date all birds had been strictly protected.

THE trustees of the Elizabeth Thompson Science Fund met in Boston, Mass., on January 13, and made the following grants among others :—$150 to Prof. J. M'K. Cattell, for the study of fatigue in relation to mental conditions. $250 to Prof. J. von Kennell, for a Monograph of the palaearctic Tarteicidae. $25 to Prof. Wm. Z. Ripley, for a Bibliography of the Anthropology and Ethnology of Europe. $100 to Prof. C. H. Eigenmann, for the study of Blind Fishes. $250 to Prof. P. Francotte, for the investigating of the fecundation and segmentation of the eggs of Polyclada. New applications will be considered in January 1899, provided they are received by the secretary before December 1, 1898. Circulars announcing the terms of the trust for the guidance of applicants may be obtained by application to Prof. C. S. Minot, Harvard Medical School, Boston, Mass.

ALTHOUGH local natural history societies abound in the north of our country, are organised, combined, and do good work for science, yet they do not seem to flourish with such vigour in the south. For this reason we are the more pleased to learn of the healthy condition of the Portsmouth and Gosport Natural Science Society, especially as, only four years ago, it was almost given over for dead. At the twelfth annual meeting on January 12, it was announced that the number of members was fifty-three, and it is hoped that the present year will see a material increase, and with this some more practical scheme of organised work. At present the Society confines itself to hearing lectures on the first and third Wednesdays of each winter month, and to making outdoor excursions during the summer. The president for the current year is Dr Charles Foran, a leading Southsea dentist and ardent naturalist, and the secretary is Mr Edgar L. Curtis, 57 Victoria Road S., Southsea.

CORRESPONDENCE

MR COSTE ON DARWINISM AND DESIGN

THE problem of keeping up the proper intercommunication between the various branches of human knowledge is one of such increasing intrinsic difficulty as the sciences grow more specialised, that we should be disposed to welcome rather than to disparage any effort in this direction. For nothing (except the work itself) contributes more essentially to the soundness of work in each science than that it should keep in touch with the rest, and from time to time stop to contemplate its work from the standpoint of another science. Yet any attempt to determine the general value for knowledge of a special branch of research is apt to be regarded by the specialist as an intrusion upon his domain rather than as a demonstration of intra-scientific amity. This is a pity, but I might have anticipated that my attempt to throw some light on the logical status of the Darwinian Theory, in an article on "Darwinism and Design" published in the *Contemporary Review* for June 1897, would expose me to misrepresentation and hostility, and I was prepared to bear a certain amount of this with philosophic composure. But I cannot help thinking that Mr F. H. Perry Coste would have done better service to his own cause if his criticism of my paper in the December number of *Natural Science* (vol. xi., p. 408), had not been so largely composed of irrelevancies interspersed with the refrain "Yah, metaphysician!" The more so, that this appeal to an obsolescent prejudice is as little a proof of Mr Coste's perspicacity as of his good temper. For even if the whole tenor of my article did not show it to be intended as a piece of philosophical criticism, he had my own word for it (*l. c.* p. 871) that I only claimed to be a philosopher.

I take the more pleasure in drawing attention to a passage where Mr Coste has read me aright because in so many other cases he gives me much ground to complain of his ostentatious disregard of my stated purpose.

Mr Coste criticises—or perhaps rather abuses—me severely for not doing a variety of things which it lay beyond the scope of my article to deal with. I do not discuss, he urges, (*a*) "the evolution of the solar system" (p. 409); (*b*) I do not prove that variations are inherited or in fact definite (p. 411, 413): (*c*) I ignore Weismannism (p. 414); (*d*) I do not disprove all the mechanical alternatives to teleology (p. 413); and (*e*) I do not prove a God (!) (p. 413). It is very terrible that all these things should have been left undone in an article of eighteen pages! But I may perhaps be permitted to point out that I very distinctly disclaimed the intention of doing any one of these things, and that it is scarcely candid on Mr Coste's part to ignore these disclaimers. I rigidly limited myself to the subject indicated in my title, *viz.*, Darwinism and Design, for the very good reason that no one can profitably discuss everything at once. Moreover I had it in mind to follow up my criticism of Darwinism by papers dealing with the other evolution theories, although the pressure of other work has necessarily postponed the execution of this project. And even Mr Coste will hardly deny that a criticism of modern biological conceptions must start with Darwinism, and not with the latest lucubration of Prof. Weismann. Still, if it is any consolation to Mr Coste, I am quite willing to state with respect to the several omissions laid to my charge, as to (*a*) that I have yet to learn that Darwinism has any thing to say about "the evolution of the solar system"; as to (*b*) that I regard this as a proper question for the biologist and said so (*l. c.*, p. 871, 879); as to (*c*) that it was not incumbent on me in dealing with Darwinism to discuss Prof. Weismann's latest modification of a theory which, as Mr Romanes showed (in his "Examination of Weismannism"), has long ago been forced by the pressure of the facts to give up the pretensions to logical consistency with which it at first impressed even philosophers; as to (*d*) and (*e*) that my purpose was only the humbler one of showing that "properly understood Darwinism was not necessarily hostile to teleology" (*l. c.*, p. 882). Obviously the *proof* of such contentions would require one or more volumes. But I thought it valuable to clear the ground for such proof by showing that Darwinism was not inconsistent with teleology. If I had gone on to show the same thing in the case of the other theories of Evolution, and succeeded, I should then have been in a position to make felt the positive argument for teleology. This argument is philosophic in character, and, in my opinion, irrefragable and sufficient to make teleological explanation the ideal of human science, even though our science should not now be (nor ever be) in the position to realise it in practice. But my article manifestly attempted nothing so gigantic, though I trust it did not exhibit the deplorable blindness to the complexity and scope of the questions at issue which appears in Mr Coste's criticism.

In addition, however, to condemning me for not putting into my discussion what it would have been mere folly to include in it, Mr Coste accuses me of various confusions of language and so charges me with 'metaphysical sophisms' and 'fallacious trifling with words.' This is, of course, a particularly discouraging reply to an honest effort to insist on the distinction of ideas ordinarily confounded. It can, however, I think, be easily shown that in this case the confusion of terms (and of thoughts) is Mr Coste's and not mine. I am said to "write as though evolutionism and Darwinism were the same thing." I challenge Mr Coste to find a passage in which I do anything of the sort; while I could show him many where the two are implicitly and explicitly distinguished (*cp.* especially the first and last paragraphs of my article). Such a confusion would indeed be strange in one who has long been conscious of being an evolutionist without thinking Darwinism the Alpha and Omega of Evolution.

Mr Coste, on the other hand, clearly succumbs to the popular fallacy of taking the most prominent species as coextensive with the genus in at least two passages. (1) After quoting a passage in which I had spoken of "Darwinism *qua* Darwinism," he proceeds to say (p. 413) that if I had "possessed any acquaintance with Weismann's work," &c. This surely involves a fusion of Darwinism with Weismannism, which, in spite of the dependence of Weismann on Darwin, must be regarded as a very serious confusion. (2) A little later Mr Coste complains that "metaphysicians who want to write about evolution will not take the trouble to find out what evolutionism connotes at the present day." Now I was writing about Darwinism, which (in my ignorance of "what evolutionism *connotes* at the present day"!) I had imagined to be only *one* out of the many possible theories about Evolution; whereas Mr Coste here equates Darwinism with evolutionism in a manner which seems to me destructive of all clear thinking. But perhaps 'what evolutionism connotes' in Mr Coste's eyes is equivalent to 'what it is popularly and inaccurately confused with.'

Again, I am accused of paltering with a double sense of 'adaptation.' The word apparently = 'inherited structural adaptations' (p. 411). Well, if that were true, one could only say that (like the 'elliptical' sense of denial of design mentioned on p. 410) it is not a good specimen of scientific precision of language. But in reality my 'fallacy' consisted only in proving that in a general sense the possibility of 'active adapting' (*l. c.*, p. 869) could not be disputed. This was established by adducing the active adaptings of conscious beings, and whether these result in inherited structural adaptations or not is totally irrelevant. That is, Mr Coste fails to see that the point at issue is whether 'adaptation' is wholly mechanical or also purposive, and confuses it with the wholly irrelevant question whether purposive adaptations are inherited or only functional! If any one mixes up distinct senses of words, it is not myself.

Lastly, I must allude to a very puzzling exhibition of Mr Coste's logic. On p. 410 he seems to consider it absurd that I should have been at pains to show that a completely logical working-out of Darwinian assumptions might deny the efficacy of intelligence as such, and reduce all animals to automata. This simply shows his ignorance of the length to which materialistic explanation can go and has gone, but does not make it less necessary for me to prove at the outset that "intelligence, *i.e.*, action directed to a purpose, has been at work." Which, accordingly, I did (*l. c.*, p. 871). Yet on the next page I am told that I have 'not even attempted' the proof of this proposition! And a little later we hear that 'we all know' that this intelligent adaptation is a fact (in which case surely it would have been superfluous to 'attempt' a proof!).

These specimens of Mr Coste's ratiocination go far to re-establish my conviction that there is still room for philosophic criticism in science, even more than his admission (p. 412-3) of the value and novelty of my contention as to the methodological character of the Darwinian assumptions. I had, indeed, anticipated that this would prove to be the most interesting part of my argument for the biologist; but even here Mr Coste fails to state its full import, which lies in the inevitable corollary (*l. c.*, p. 880-1) that the real inconsistency is not between Darwinism and design, still less between teleology and the facts of organic history, but between design and an abstract application of the calculus of probabilities. F. C. S. SCHILLER.

MATHEMATICAL BIOLOGY

THE mathematical treatment of biological problems involves a certain danger; we have seen that about a year ago (see *Nature*, vol. lv. p. 155), and we see it again in the case of Mr Vernon's theory of "Reproductive Divergence."

The first premise of Mr Vernon's theory (see *Natural Science*, vol. xi. p. 405) expressed in an algebraic formula is as follows :—

Parents :		Small.	Medium.	Tall.
100 SS	give offspring	$a. \times$	$b. \times$	$c. \times$
100 MM	,,	$b. \times$	$d. \times$	$b. \times$
100 LL	,,	$c. \times$	$b. \times$	$a. \times$

$$\text{I.—}(a+b+c).\times=(2b+d).\times=(a+b+c).\times\ ;$$

a, b, c and d being the percentage numbers of offspring produced by the intermarriages between S with S, M with M, and L with L under normal conditions ; $\times=\frac{100+m}{100}$ being the measure of fertility under those new conditions as assumed by Mr Vernon.

The second premise of the theory (*l.c.*) is that the numbers of offspring produced by the intermarriages of short and medium, and of medium and tall parents, "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." Hence we have :—

Parents :		Small.	Medium.	Tall.
100 SM + 100 MS	give offspring	$(a+b).y$	$(b+d).y$	$(c+b).y$
100 ML + 100 LM	,,	$(b+c).y$	$(d+b).y$	$(b+a).y$
100 SL + 100 LS	,,	$2b.z$	$2d.z$	$2b.z$

$$\text{II.—}(a+2b+c).y+2b.z \mid (2b+2d).y+2d.z \mid (a+2b+c).y+2.b.z\ ;$$

$y=\frac{100-n}{100}$, and $z=\frac{100-p}{100}$, being the measure of fertility of the intermarriages between different parents.

The number of medium offspring will be smaller or larger than that of small and tall ones, or equal to this number, if we have—

$$(2b+2d).y+2d.z \gtreqless (a+2b+c).y+2b.z\ ;\ \text{or, as in (I.)}\ a+b+c=2b+d,$$

$$d \gtreqless b\ ;$$

that means, the answer to the question whether there are more medium than small individuals under II. is entirely independent of x, y, z, or of the degree of fertility resp. sterility, but depends solely on the percentage number of small (b), medium (a), and tall (b) offspring produced by M intermarrying with M under ordinary circumstances. Mr Vernon must so alter his premise I., that of the offspring of M marrying M less than one-third are medium individuals ($d<b$) ! This, however, cannot be a premise of "Reproductive Divergence," as it would mean putting the cart before the horse.

If this simple mathematical demonstration should not be intelligible enough, Mr Vernon will perhaps see the fallacy in his theory, if he takes that degree of sterility between different parents, which should be the most favourable one for the theory, namely, absolute sterility not only between the extremes S and L, but also between S and M, and between M and L. In this case we have to do only with the outcome of the marriages of S with S, M with M, and L with L ; *i.e.*, only with the numbers under I., which are equal, as Mr Vernon says himself (*l.c.*). KARL JORDAN.

ZOOLOGICAL MUSEUM, TRING,
February 12, 1898.

DIPELTIS AN INSECT LARVA

I AM much obliged to Mr C. J. Gahan for directing attention to the fact that Dipeltis bears close relationship with certain coleopterous larvae. A larva almost identical with the one figured in the January number of this review was recently shown me by Mr E. A. Schwarz of the U.S. National Museum. Since seeing this specimen I agree with Mr Gahan's conclusions that *Dipeltis* is the larva of some insect, and not one of the Apodidae. The "two small shallow pits, which are interpreted as ocelli," and the "two faintly preserved eye spots," need be no longer explained as ocelli and eyes, since in the above-mentioned larva very similar modes and depressions are present. Mr Schwarz thinks the smaller specimen figured by me (figs. 4 and 5) may be related with the larva of Lampyridae or Dascyllidae. It is more natural to interpret the three anterior large segments of *Dipeltis* as divisions of the thorax of a Lampyrid larva than that they are parts of the cephalon of an *Apus*-like crustacean.

CHARLES SCHUCHERT.

U.S. NATIONAL MUSEUM.

THE AUTHENTICITY OF PLATEAU MAN

The cudgels on behalf of the Authenticity of Plateau Man have been taken up by so many scientists that I only find it necessary to call attention to one paragraph of Mr Cunnington's article in the November number of *Natural Science* (vol. xi., p. 327). He states: "The vast number of the flint implements from the plateau gravel is another difficulty. . . . We are told that two pits dug in 1891 into a bed of gravel one foot in thickness yielded thousands of artificial flakes and some hundreds of scrapers."

Mr Cunnington is under a serious misapprehension here. It was not from the pits, but from the floor on the face of the Chalk Escarpment, nearly 80 yards long, that these numerous flakes were discovered. Moreover, these flakes were not those of Plateau man at all, but of the late Palaeolithic man, and are of undoubted authenticity, being of the ordinary form and bearing that hall mark of man's handiwork, the bulb of percussion. Many have been sent to and acknowledged by Sir John Evans himself.

If Mr Cunnington had carefully read the report to the British Association, Ipswich, 1895, he would have seen that in reference to the plateau gravel of one foot in thickness, my words were: "This gravel was hard and compact. From it I secured very many worked implements." Specimens forwarded at the time to Sir Joseph Prestwich brought the reply: "Interesting, rude but true." BENJAMIN HARRISON.

IGHTHAM, *Feby.* 6, 1898.

SOCIÉTÉ PHILOMATHIQUE DE PARIS

In a copy of the publications of this Society, preserved in the British Museum (Natural History), there is a portion of vol. i entitled *Bulletin de la Société Philomathique à ses correspondens*, which is paged 1'-119'. As I have had a good deal of trouble about the date of this portion of the work, I may mention that in vol. iii. of *Bulletin des Sciences de la Société Philomathique*, p. 192, under the heading "Avis," there is a note which enables us to fix the date of publication of these pages as 1803.

C. DAVIES SHERBORN
(*Index Animalium*).

NOTICE

TO CONTRIBUTORS.—All Communications to be addressed to the EDITOR of NATURAL SCIENCE, at 29 and 30 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.

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NATURAL HISTORY DEPARTMENT, MUSEUM OF SCIENCE AND ART, EDINBURGH

THIS Magazine — a continuation of "The Scottish Naturalist, founded in 1871—was established under the present editorship in January 1892, for the purpose of extending the knowledge of and interest in the Zoology and Botany of Scotland. The Annals is entirely devoted to the publication of Original Matter relating to the Natural History of Scotland, and includes papers contributing to the elucidation of the Fauna and Flora recent and fossil, Observations on Life Histories, etc., and Notes recording the occurrence of uncommon species and other useful and interesting facts.

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APRIL 1898

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NATURAL SCIENCE

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NOTES AND COMMENTS

"WHAT'S IN A NAME?"

SOME apolog[illegible]ll doubtless be demanded by our readers for that we present them, [illegible]hin a single number, with two lengthy articles on special points [illegible]ological nomenclature. We venture to think, however, that th[illegible]icles appeal to wider circles than the select few who pose as speci[illegible] on crinoids and crayfish. The animals whose names are in quest[illegible] are among the best known of invertebrates; of some it might be said that they are in everybody's mouth (for which remark we offer further apology). Is it not a little humiliating to zoologists that they are still undecided what to call a lobster? It is not even safe to call it a Crustacean; for, if some priority-fanatics had their way, the name Crustacea would be the heritage of what are now known as echinoderms. It is doubtful whether obedience to any code of rules could effect the automatic solution of all these puzzles; certainly their settlement needs a far wider and deeper knowledge of ancient literature than is possessed by any living zoologist. Each day almost brings its discovery and the second-hand bookstall reverses a time-honoured belief. Let us pray that the speedy adjustment of these disputes may set free brains and energy to find out a little more about the facts of nature, and that the present reign of topsy-turvydom may be remembered only in some modern "Stultifera Navis," towards which our parodist makes his humble contribution:—

> The lobster and the crayfish
>   Were walking claw in claw,
> They wept like anything to hear
>   Such quantities of jaw.
> If only this were cleared away,
>   They said, we should guffaw.

being done by the Telegraph Department of New Zealand, who, on the occurrence of any earthquake shock, fill up certain forms stating the exact time, duration, and other details of such shocks as occur in their own region. This information is of much value, and has been collected in New Zealand since 1889, and though some of the other colonies have instituted similar observing stations, Mr Hogben asks for a more uniform and developed system. He asks also for the immediate establishment of stations at Sydney, Melbourne, and Timaru, in order that a chain of stations might be established which would then be continuous around the world.

The Origin of the Australian and Tasmanian Aborigines

Mr Howitt, the President for Ethnology, in dealing with the origin of the aborigines, said he was of opinion that, in spite of the contention of many writers to the effect that the primitive Australians and Tasmanians had come from other lands in ships or canoes, there were but few evidences to show that they had any knowledge of navigation or of sea-going vessels. Whatever evidence there was in the customs or in language of the aborigines, from the time that the first voyagérs visited these shores, led to the conclusion that their ancestors knew nothing better than the catamarans of the Tasmanians or the bark canoes of the Australians. In any theory as to the origin of the natives of Australia, one fundamental element must be that the ancestors of these savages reached Australia or Tasmania by land; or if the land connection was not continuous, the intervening channels were such as could be traversed by vessels no better than the catamarans or canoes above mentioned. He quoted many geological facts, all of which tended to show that an immense period of time was one of the elements of any solution of the problem, and that during that period the Australians had been isolated from outside influence, having at the same time a continental area in which to develop their institutions. The level of culture of the Tasmanian had been termed the eolithic, and that of the Australians might fairly be termed neolithic, or even as regards some of the tribes of Central Australia bordering on the palaeolithic. The social organisation of the Tasmanians was also below the level of that of the Australians. Mr Howitt came to the conclusion that the Australians reached their continent by a land bridge connected with the Indo-Asiatic continent, or by a land extension of the Austral continents to the north-west, or over some shallow channels separating Australia from these lands.

ARTESIAN WATER IN AUSTRALIA

THE construction of Artesian wells all over the arid districts of Australia, to which we have often referred in these pages, bids fair to develop the resources of the continent in a remarkable manner. The Rev. J. Milne Curran read an interesting paper on the subject before the geological section of the Australian Association, in which he briefly sketched the history of Artesian exploration. Referring to the Ballimore, Trangie, Coonamble, and Dubbo bores and the cores brought up from the 1896 bore at Salisbury Downs, the author of the paper concluded that the Artesian water of the north-west and west of New South Wales was derived from Triassic strata, and that there was no evidence to show that a single Artesian well in New South Wales derived its waters from Cretaceous rocks. Mr R. L. Jack, in the discussion which followed, stated that Artesian water had just been struck in Queensland, on the central line of railway at a place called Maria Creek, west of Dawson River, in rocks of Permo-carboniferous age. These are the oldest rocks in which Artesian water has been struck in Australia.

THE ABORIGINES OF JAMAICA.

VOL. II. of the *Journal of the Institute of Jamaica* (July 1897) contains a paper by Mr J. E. Duerden, curator of the Museum of the Institute, on the "Aboriginal Indian remains in Jamaica." From this we may gather that the antiquities of the island are being investigated with zeal and studied in a systematic manner, and that endeavours are being made to collect material for the formation of a really good museum of local antiquities. Chapter I. deals with the kitchen-middens, refuse and shell-heaps, of which a large number have already been discovered and partially explored. From them much may be learnt as to the life of the Indian inhabitants in pre-Spanish times, a culture which waned rapidly under Spanish domination. A brief sketch is given of the individual finds of this class and of the objects found therein; and, while no great detail is as yet forthcoming, the preliminary investigation will, no doubt, lead to more systematic and exhaustive research. The Burial Caves are treated of in Chapter II. The prevalence of cave-burial in Jamaica was due to presence of very numerous natural caves and sink-holes in the 'White and Yellow Limestone' formation which covers a large proportion of the island of Jamaica. The human crania found in these caves shew in all, or nearly all, instances traces of artificial deformation, very marked in some cases. Examples of pottery vessels, which in the kitchen-middens are, as might be expected from the nature of the deposits, always in a fragmentary condition, have been found in a group have been elevated, and since their elevation have, like the northern part of Queensland, remained nearly stationary and exposed to great and prolonged denudation and erosion, which has reduced the islands to their present height; the platforms upon which the barrier-reef corals have grown being merely the flats left by the denudation and erosion of a central island of greater size than that now left; while the atolls are similar flats from the interior of which the islands have been eroded and the lagoons of which have been continually scoured by the action of the sea, the incessant rollers pouring a huge mass of water into the lagoon, which finds its way out through the passages leading into it." In short, the Fijian area is one of elevation and not of subsidence, though what the age of the elevated reef may be is uncertain. Since it attains a thickness of 800 feet, it was very probably deposited originally during the period of subsidence, but this has nothing to do with the present shape of the atolls. Prof. Agassiz, who is informed by Prof. David that the evidence of the Funafuti boring was not so simple as was at first supposed, suggests that the boring merely penetrated the base of an ancient reef, and therefore in no way corroborates the theory of a subsidence. Prof. Agassiz is also inclined to ascribe the formation of atolls more generally than has hitherto been done to the erosion of volcanic summits or of extinct craters. He describes one or two undoubted cases in the Fiji group, and points out that many of the smaller atolls may have been formed in the same way. The great depth of the lagoons of some of the atolls is quite as intelligible upon this view as on the theory of subsidence. All interested in coral reefs will look forward to the fully illustrated report of his cruise which is promised by Prof. Agassiz.

A Note on Plant Distribution

We recently received a number of a bi-monthly French journal, probably unknown to most of our readers, the *Revue des Sciences Naturelles de l'Ouest* (vol. vii., No. 2), which contains the second and concluding part of an account of the genus *Acaena* by M. P. Citerne of Nantes. *Acaena* is a genus of Rosaceae, very closely allied to *Poterium*, two species of which, *P. Sanguisorba* (Salad Burnet) and *P. officinalis* (Great Burnet), are well-known members of our British flora. It resembles *Poterium* in habit, containing about forty species of small low-growing herbs or under-shrubs. *Poterium* is a north temperate plant; *Acaena*, on the other hand, is widely spread over the southern hemisphere, advancing in America as far north as Mexico. Associated with its occurrence over so wide an area we find an interesting means for distribution in the structure of the fruit. This is small, and, as frequently happens in Rosaceae, is a pseudocarp (like the apple) composed of the somewhat swollen floral axis, enclosing the true fruit with a single seed. In shape it is rounded, oval, or pyramidal, generally with a narrowed base, and bears outgrowths in the form of spines, or more rarely wings. The spines appear before flowering, and develop and often increase in number as the fruit ripens; they vary considerably in number, size, and shape, and often end in a barb, like a fish-hook. They are thus eminently adapted for clinging to the fur of animals or plumage of birds, and the wide distribution of the genus and its presence on isolated Oceanic islands can be accounted for by the transport of the fruits by widely-ranging sea-birds. It is of interest to notice that M. Citerne, in his division of the genus into seven sections, relies to a great extent for sectional characters on the number and form of these outgrowths and their distribution over the surface of the fruit. The chief centre of distribution, where, that is, the genus is most richly developed, is in extra-tropical South America. Nine of the thirty-eight species are endemic in Chili, four spread southwards through Patagonia to the Straits of Magellan and Tierra del Fuego, one is confined to Patagonia, one to Tierra del Fuego. One species occurs only in the Straits of Magellan, Tierra del Fuego, and the Falkland Islands, another in these three localities, and is reported from Mexico; a third in the same three, and also in New Zealand. The Falkland Islands, and the Islands of St Paul and Amsterdam in the South Atlantic, have each a peculiar species, while one, the most widely distributed of the genus (*A. sanguisorbae*), is found in Tasmania, New Zealand, at the Cape of Good Hope, and in the Island of Tristan da Cunha and the Campbell Islands. One species is confined to the Cape. A small section of three species, characterised by fruits without spines, is confined to New Zealand, and three others occur only in Australia (with Tasmania). Tracing its path northwards we find a few species in the Andes of Peru, Bolivia, Columbia, and New Grenada, two of which reach as far north as Mexico, while one is peculiar to the Sandwich Islands.

Japanese Explorers in Formosa

In our January number, we alluded to the geological exploration of Formosa now being undertaken by the Japanese. We learn from the *Révue Scientifique* that the highest peak of that island, Mount Morrison, was ascended for the first time in 1896 by Sevioku Honda, Professor of Forestry at Tokio, accompanied by various people, amongst whom were a geologist and a topographer. After landing at Kelung, the explorers gained Ling-ki-ho, the last Chinese village, thenceforward they met with only the aboriginal population. After the luxuriant cultivation of the low country, the travellers traversed a very wooded region, in which fig-trees, camphors, palms and tree-ferns predominated. The camphor-trees are particularly magnificent there, but already they are being destroyed by the dealers. Higher up, *Chamaecyparis* and *Cryptomeria* predominate, and yet higher are other conifers. No part of the ascent appears to be difficult; the slopes are not abrupt, and the soil is covered with trees or verdure to within a short distance of the summit, which is at an altitude of 4305 métres (14,000 feet). The mountain is not volcanic; it is composed of slate and quartz. There was no snow, the time of the ascent being autumn. The aboriginal population is agricultural, and affords an interesting study.

The Gipsy Moth

THE latest bulletin (No. 11 N.S.) which we have received from the Entomological Division of the U.S.A. Department of Agriculture, deals with the history of the 'Gipsy' Moth (*Porthetria dispar*) in North America. Mr L. O. Howard tells us that the insect was introduced in 1869 at Medford by Prof. Trouvelot, who wished to experiment with various European silk-spinning caterpillars. Some of his 'Gipsy' caterpillars escaped out of the window and established themselves in a neighbouring wood. After twelve or fifteen years' struggle with a hard climate, insectivorous birds, and occasional fires, the moths began to multiply and to spread about the surrounding parts of Massachusetts. In 1889 the caterpillars became a perfect plague at Medford, stripping trees completely of their leaves, falling about the road in thousands, invading houses, and getting into the food and the beds. During recent years the species has spread through a great part of eastern Massachusetts, but stringent efforts have been adopted to keep it in check, and it is hoped that the undesirable immigrant will ultimately be altogether exterminated.

The excessive multiplication of this moth in its new country will be of interest to British entomologists. Always with us a local insect, it is now probably extinct as a wild species in England though it is still kept up by moth-breeders in a 'domesticated' state.

The Muscle Scars of Fossil Cephalopod Shells

A PAPER of much interest and importance was read before the Linnean Society at their meeting on 3rd February last, "On the Muscular attachment of the Animal to its Shell in some Fossil Cephalopoda (Ammonoidea)," by G. C. Crick, of the British Museum (Natural

History). Having first briefly noticed previous descriptions and figures of what was believed to be the impression of the muscular attachment of the Ammonoid animal to its shell, the author pointed out the form and position of the 'shell-muscles' and of the 'annulus' in the recent *Nautilus*, and indicated the form of the impression of these structures as seen upon an artificial internal cast of the body-chamber for comparison with the fossil forms, in nearly all of which any indication of the muscular attachment there may be is preserved upon the natural internal cast of the body-chamber. After describing the character of the 'muscular scars' in an example of *Crioceras*, in which they were both very perfectly preserved, and the position of the 'annulus' as clearly shown in an Ammonite from the Oxford Clay, Mr Crick pointed out in a series of diagrams the impression of these structures in the various forms assumed by the Ammonoids, viz. — *Baculites, Hamites, Ancyloceras, Crioceras, Macroscaphites, Scaphites, Turrilites*, and *Heteroceras*, and several Ammonites, as well in *Clymenia*, and in some of the Goniatites.

The scars of the muscles are to be found in almost every instance on the inner or dorsal side of the body-chamber, near to the last septum, and are generally oval patches with the longer axis directed either antero-posteriorly or transversely; in spiral forms like *Turrilites* or *Heteroceras* they are curved with the shell, the one nearer the inner side of the coil being in either case the shorter. The annulus runs from one to the other of these scars round by the ventral side, not far from the septum, and in those forms where the latter is much folded it dips slightly with each lobe between the saddles. There is also evidence of muscular attachment to the shell between the two big scars and on the dorsal sides, so that the animal was connected with the shell all round the chamber. This, Mr Crick is inclined to infer, is evidence that the chambers of the whorls were filled with gas and not with water, since this complete attachment would be unnecessary in the latter case.

Mr Crick's researches effectually dispose of the notion, which was once held by many, that the animal was, so to speak, a tenant at will of its shell, like *Argonauta*, and show it to have been a tenant for life, like the Nautiloidea; but further in the table of affinities we cannot yet go.

Science in Lincolnshire

A note and comment in our February number announcing the formation of a Lincolnshire Science Society has brought us a protest from the Lincolnshire Naturalists' Union, which includes among its officers some eminent men, some well-known naturalists, and some whom we are glad to recognise as our own contributors. The Union has, we understand, existed for some time and publishes an account of its work and of the investigations of its members in the *Naturalist* and in *Lincolnshire Notes and Queries.* This Society also is advocating the establishment of a Museum in Lincoln, or rather the acquisition of a better building, in which to keep the collections which have already been brought together. It has therefore the same objects as the Lincolnshire Science Society. There is no doubt an unfortunate distinction between 'Science' and 'Natural History,' but it does not seem to us that there should be room in the county of Lincoln for two Societies intended to cover almost the same ground. We hope that some way will be found to fuse the energy of the upstart Science Society with the respectability of the older Naturalists' Union.

Feathers and Females

We are very glad to see, from various articles in the daily press, in *Nature Notes*, and elsewhere, that the crusade against the wearing of feathers by the harder-hearted sex is continued with vigour. We only regret that the necessity for this crusade is as great as ever it was. We know very well (what many of the crusaders seem to forget) that these feather-headed ladies read neither *Natural Science*, nor *Nature Notes*, nor even Sir William Flower's articles in the *Times*. As before, we can only appeal to our readers to do all that is in their power to influence those with whom they are naturally brought in contact. The apparent cruelty is probably due in most cases to mere ignorance, although we must confess that it does not seem to us that the woman who can commit the vulgarity of wearing the unnaturally dyed feathers that have been fashionable of late, would care how much suffering she caused to satisfy her unnatural and ridiculous taste.

A Card-Index to Current Zoological Literature

The Concilium Bibliographicum of Zürich-Oberstrass is now prepared to furnish sets of cards including references to articles on any zoological topic the subscriber likes to suggest. One can now order references to all papers published throughout the year on such subjects as the reptiles of Celebes, the salivary glands of molluscs, the instincts of bees, or the teleostean lake fauna of Westmoreland. Charges are now made according to the number of cards sent—the larger the number of cards the smaller the relative price. Cards are distributed every month, and if no papers on the given subject appear, a printed statement to that effect is sent to the subscriber.

The prices are undoubtedly cheap; even if one's subject necessitates less than 100 references *per annum*, the cards cost only 5 centimes apiece. 3000 references can be had for 39 francs. This reduction in price is rendered possible by the office having established its own printing press, and this will, it is hoped, enable the cards to be sent out with greater punctuality than has hitherto been possible. It is exceedingly unfortunate that the skilled typographer who was engaged should have been confined to the hospital for three weeks; but even this did not prevent the energetic Dr Field and his assistants from issuing no less than 200,000 cards from the press during December last. Some day it is possible that the international committee, convoked by the Royal Society, may provide zoologists with a complete and up-to-date subject catalogue; but even then, so far as we are acquainted with the plans that have been put forward, they do not propose to enter into one-hundredth part of the detail that is already possible to Dr Field. For the present, therefore, zoologists may be warmly recommended to consult their own advantage, and to assist a practical and useful work by subscribing to the Concilium Bibliographicum on the principle of 'no results, no pay.'

British Naturalists

We have received from Mr L. Upcott Gill the "Naturalists' Directory" for 1898. We are glad to see that a few more naturalists whose names are known to us have found their way into this issue, and since a complete work of this character would undoubtedly be of great service, we hope that in future Mr Gill will be even more successful in drawing up his lists. We notice his statement that *Natural Science* can be obtained post free for twelve shillings *per annum*. This, as we remarked last year, is not the case. A feature that might be made very useful is the list of works on natural science published during the previous year in the British Isles. This is said to be complete, but we have failed to find several works by well-known British authors which were reviewed in our own pages last year. There is also a list of Societies, Field-Clubs, Museums, etc., but this, like the other section, seems to us very incomplete. In short, the "Naturalists' Directory" deserves the support of our readers not for what it is, but for what it might be.

For Bibliographers

Those who have used Dr H. Carrington Bolton's "Catalogue of Scientific and Technical Periodicals" will be glad to hear that a edition is soon to be published by the Smithsonian Institution. It is not surprising, considering the growth of science, that it has been found necessary to add 3500 titles since the first edition published in 1885. Another publication, which should be found useful to others than those for whom it is primarily intended, is the "International Exchange List" of the Smithsonian Institution, being a list of 9414 learned societies, museums, universities, and other bodies with which American publications are exchanged.

I

The Study of Variations

THE work of any generation must be largely influenced by current accepted ideas, which form, as it were, a mental environment, with which all the work of that generation will be coloured, and it follows that there must be considerable danger resulting from reactionary tendencies which frequently arise when the preceding generation has carried any given method of research beyond warrantable limits. If these current ideas do not happen to be true, errors, which may take years to eradicate, may arise, owing to theories being built upon these faulty conceptions. Harmful as this must be, it is, I believe, far less so than when a definite feeling or prejudice is formed in the mind against some method which had been previously misused.

For any prejudice thus formed will necessarily influence all work produced by all the minds so affected, and, further, while it is possible conclusively to disprove any erroneous idea, it is extremely difficult to remove any prejudice of the mind, when once formed.

In the present age it will, I believe, be found on reflection that the tendency, in all branches of science, is to neglect all purely theoretical conceptions, however sound, and to rely exclusively on practical deductions, for the most part directly deduced from experiment. This contempt for theory is, to my thinking, one of the most unsatisfactory elements in modern science, and has probably been directly induced by the extremely speculative and untrustworthy theories of the beginning of this and the latter part of the last century.

A theory of some sort is necessary to enable any investigator to collect facts or to perform experiments to any purpose, for facts or experiments, however numerous, are useless if any important factor is not accounted for, hence increasingly definite conceptions or theories always precede increasingly trustworthy investigation.

"It is a common error," says Poulton,[1] "to suppose that the intellectual powers, which make the poet or the historian, are essentially different from those which make the man of science. Powers of observation, however acute, could never make a scientific

[1] "Charles Darwin and the Theory of Natural Selection."

discoverer; for discovery requires the creative effort of the imagination. The scientific man does not stumble upon new facts or conclusions by accident; he finds what he looks for. The problem before him is essentially similar to that of the historian who tries to create an accurate and complete picture of an epoch out of scattered records of contemporary impressions more or less true, and none wholly true. Fertility of the imagination is absolutely essential for that step, from the less to the more perfectly known, which we call discovery.

"But fertility of imagination alone is insufficient for the highest achievement in poetry, history, or science; for in all these subjects the strictest self-criticism and the soundest judgment are necessary in order to insure that the results are an advance in the direction of truth."

This passage appears to me to be singularly applicable to evolutionists of the present day. If an examination of the facts, collected by evolutionists with reference to heredity, be made, it will be found that in most cases elimination of doubtful factors has not been attempted, because it has been assumed that variations must be adaptive or non-adaptive in character, and that consequently the case for or against use-inheritance will be decided by thus directly appealing to variations as they exist in nature rather than to specially prepared test cases. The aim of this paper is to endeavour to demonstrate that this question cannot be so settled, even if it were always possible to know whether a given variation were adaptive or otherwise. And when it is considered how difficult it is and what prolonged study is required to assert whether any small variation in man is useful or otherwise, the difficulty of arriving at a similar conclusion in any other sub-order or species, which of necessity is much less thoroughly studied, must be immense.

If a statement of the views held by different evolutionists be compared with objections raised to these views, the inconclusiveness of the objections will be noticed in most if not all the cases so examined.

If the opinions of Cope, Henslow, Darwin, or Weismann on this subject be shortly stated, the difficulty of arriving at a satisfactory conclusion will be rendered still more obvious, while the intermediate positions held by other evolutionists, such as Spencer, Huxley, Romanes, Galton, make the present position peculiarly unsatisfactory for future work in evolution until this question is settled.

Darwin.—"As far as I am able to judge, after long attending to the subject, the conditions of life appear to act in two ways,—directly on the whole organism or on certain parts alone, and indirectly by affecting the reproductive system. With respect to the direct action, we must bear in mind, as Professor Weismann has lately insisted, and as I have incidentally shown in my work on 'Variation under Domestication,' there are two factors—namely, the nature of the organism and the nature of the conditions. The former seems to be much the more important; for nearly similar variations sometimes arise under, as far as we can judge, dissimilar conditions; and, on the other hand, dissimilar variations arise under conditions which appear to be nearly uniform. The effects on the offspring are either definite or indefinite. They may be considered as definite when all or nearly all the offspring of individuals, exposed to certain conditions during several generations, are modified in the same manner" (p. 6 "Origin of Species," 6th edition, 1884).

"Indefinite variability is a much more common result of changed conditions than definite variability, and has probably played a more important part in the formation of our domestic races" (p. 6 "Origin of Species").

Weismann.—"The cause of hereditary variation must be due to the direct effect of external influences on the biophors and determinants" (p. 415, Weismann, "Germ Plasm: A Theory of Heredity").

"We can none the less avoid assuming that the elements of the germ-plasm—*i.e.*, the biophors and determinants—are subject to continual changes of composition during their almost uninterrupted growth, and that these very minute fluctuations which are imperceptible to us are the primary cause of the greater deviations in the determinants, which we finally observe in the form of individual variations" (p. 417, "Germ Plasm").

"Of course, I see no reason for assuming two kinds of hereditary variations different in origin. Still, it is likely that only a relatively small proportion of the numberless individual variations lie on the path of phyletic advancement, and so under the guidance of germinal selection mark out the way of further development; and hence it would be quite possible in this sense to distinguish definitely directed individual variations from such as fluctuate hither and thither with no uniformity in the course of generations. The root of two is, of course, the same, and they admit of being distinguished from each other only by their success, phyletic modification, or by their failure" (Note, p. 17, "Germinal Selection").

Henslow.—"I would describe the process, therefore, once more as the result of the responsive power of protoplasm, on the one hand, and the forces of the external environment on the other.

These two factors I take to be amply sufficient for the whole of the evolution of plant structures, without any aid from Natural Selection whatever" (p. 32, "Origin of Plant Structures").

Cope.—"Many of the zoologists of this country, in common with many of those of other nations, have found reason for believing that the factors of evolution which were first clearly formulated by Lamarck are really such. This view is taken in the following pages" ("Primary Factors of Organic Evolution." E. D. Cope).

To summarise these various positions:—

Henslow considers that the sole factors in the formation of species are the direct action of the environment and the responsive power of protoplasm, and that Natural Selection is of use only in so far as it is able to select the vigorous, solely on account of their being so, but has no power to select and develop any particular portion of an organism separately.

Cope, who, while largely agreeing with Henslow's position, considers Natural Selection a subsidiary factor in the formation of species.

Darwin, who considered Natural Selection to be the main factor in developing species, but considered variations as due largely to the direct action of environment, Natural Selection subsequently developing variations so produced, also considered that changed environment could modify directly the reproductive system and hence influence heredity.

Weismann, who believes that use-inheritance plays no part in species formation, and extends the action of Natural Selection to the biophors, etc. Natural Selection and indefinite variability are therefore the only factors in evolution.

In criticising the position of the Neo-Lamarckians, I shall mainly base my arguments on Professor Henslow's position, because he has taken the most definite, and at the same time the most extreme, position. I do not wish to be understood to doubt the facts adduced, or even to assert that the conclusions drawn are incorrect, but simply to show that the facts as stated, and the arguments drawn from these facts, do not on logical grounds prove the position taken up.

I shall choose for the statement of his views the epitome given in the preface to "Origin of Plant Structures," because it is the most condensed statement of his position, and appears to accord fully with an article contributed to this journal at a later date (September 1897).

"I. Darwin asserts that Natural Selection has no relation whatever to the primary cause of any modification of structure.

"II. A changed environment—especially that of cultivation—stimulates variability—*i.e.*, the innate capacity of varying, which results in variations of structure. This fact is recognised by Darwin, Weismann, Spencer, and all other biologists.

"III. Under cultivation variations, especially after several years, are often indefinite, as may be seen in wheat, maize, and in numerous garden plants (but not in all, as sea-kale and asparagus). Hence artificial selection is absolutely necessary.

"IV. In nature variations are always definite, and not exceptionally so, as Darwin thought. The consequence is that 'all or nearly all individuals become modified in the same way.'

"V. The result of the preceding is that a new variety, and thence a new species, would be produced 'without the aid of Natural Selection.'"

I propose to deal with the last conclusion first; the statement is made that if definite variations occur "a new species would be produced 'without the aid of Natural Selection.'"

And that this meaning should be perfectly clear, and that the total exclusion of Natural Selection in the formation of species is intended, he elsewhere places the words "a seedling survives solely because it is vigorous" in italics.

If for the sake of argument it is granted "that variations are always definite," and also that definite variations are unexplainable by Natural Selection, and therefore that "all or nearly all the individuals become modified in the same way," it is still necessary to show not merely that all are similarly modified, but also that they are all equally thoroughly so, otherwise the variation that is most adapted will probably or at least possibly be selected, and Natural Selection will thus become a factor of some importance.

Again, it is stated that "In nature, variations are always definite," and it apparently is assumed by Henslow and other Lamarckians that this of necessity upsets the Darwinian position.

It does not appear to me seriously to weaken the theory of Natural Selection, because more or less direct variations would ultimately be produced by this factor alone.

Assume that at any period, however remote, variations were completely indefinite, and suppose that an individual A gave rise to 11 varieties, a^1 a^2 a^3 up to a^{11}; assuming that the odd numbers were more or less unadaptable varieties, 2, 4, 6, 8 and 10 would be selected, and then on the simple assumption of hereditary transmission of tendencies, which all biologists admit, these varieties 2 to 10 would give rise to still more adaptable varieties, until each variety was definite, only in varying degree, the more or less unadaptable being increasingly rigorously weeded out in each succeeding generation, and this would continue to be so as long as only a very gradual migration or no migration at all recurred, provided conditions remained more or less the same. Directly, however, any sudden change occurred, the balance of adaptable variations would have to change, and indefinite variations would become once more apparent. Now this change might perfectly explain the fact that variations are more definite in nature than under domestication, because changes of environment are less extreme in nature than when under the selective power of man, also this very change in the balance of variations might itself be the cause of atavism, etc., which has frequently been noticed under some of these extreme changes in environment or when crosses between allied species have occurred.

Thus, so far from definite variations being a difficulty in the acceptance of the theory of Natural Selection, it would be precisely what on *a priori* grounds would be expected.

Finally, there are those classes of variations which are said to be directly modified in response to environment and in which the variations have possibly been inherited after the third or fourth generation of exposure to the modifying cause. To cite one instance, the fact of " chlorophyllous tissue being much more developed in sunlight than in shade."[1]

It is not enough to show that sunlight causes an increased amount of chlorophyll to be formed, for the fact that increased function brings about a compensatory increase in the tissue so exercised is admitted by all biologists: it is not enough even to demonstrate that this increase is detectable when the variety thus formed is again bred under the normal amount of sunlight. It must further be shown: (1) How many seeds or cuttings of the particular plant were set; (2) How many of those that were set died; (3) What was the result of a control experiment in which the same conditions were observed in everything but the excessive light; (4) That in all the experiments the cuttings were so situated that the growth of each was, as far as possible, entirely free from interference by the others; (5) That the chlorophyll does not develop more quickly, when, with otherwise similar conditions, rigorous Natural Selection is allowed to occur; (6) It must also be shown, as far as possible, that no immediate ancestor of the selected variety experimented upon has had a greater supply of chlorophyll, and consequently that the action that the sunlight had apparently induced was not in reality only a favourable reversion.

In short, to disprove the action of Natural Selection, it is neces-

[1] P. 72, "Origin of Plant Structures."

sary to show something further than that variations are definite, or even that direct adaptive modification may occur which can subsequently become hereditary. Natural Selection must, as far as possible, be totally excluded, reversion and influence of climate on reproductive organs also more or less satisfactorily eliminated, before anything like a test case can be adduced. This case must still further demonstrate, if it is to disprove anything more than the Weismannian position, the uselessness of Natural Selection by separate control tests.

The same arguments will apply to Cope and other Neo-Lamarckians, although less forcibly, on account of Natural Selection being considered a subsidiary factor.

But the position is if anything even more difficult for the Neo-Darwinians, as it would be necessary to produce definite hereditary variations by Natural Selection without any change in environment, and the elimination of such a wide series of factors would be exceedingly difficult to accomplish. The extreme difficulty of this position has been, to my thinking, so conclusively proved by Romanes and Spencer, that the only means of obtaining definite results will be by a complete revision of the methods used.

My object has been to show that equally plausible contrary positions may be taken up from the facts adduced by either side.

If this is so, it would seem probable that some fundamental mistake must be responsible for this curious position, and the cause, I believe, exists in the extremely uncertain meaning given to such terms as adaptive, definite, and indefinite variations. A more rigid and exclusive use of these terms might, by rendering the difficulties more apparent, help them to be more easily overcome.

Some such classification as the following might be resorted to:—

Unadaptive Variations.—Having no selective value on account of all the variations being wholly unfitted for their environment.

Indefinite Variations.—Capable of selection, but varying in all directions round a central position.

Definite Variations.—All variations being of selective value, but that value varying in degree.

Adaptive Variations.—All equally modified for special conditions, and therefore incapable of being selected except from general strength or vitality.

These divisions can only be approximately correct, but would serve to limit the meaning of these terms and aid the formation of clearer ideas on this subject.

Lastly, some classification of possible origin of variations which might be applied equally to both animal and vegetable life, by which common comparisons might be estimated, is, I think, essential to a better understanding of the subject.

Macalister[1] divides the varieties of human organism into nine classes, as follows: (1) Hetero-meral or anomalies of quantity; (2) Metaplastic or anomalies of material; (3) Didymal, anomalies of repetition; (4) Diastematic, anomalies of cohesion; (5) Antithetic or anomalies of alternation; (6) Metathetic, anomalies of position; (7) Homoiotic, anomalies of series; (8) Atavistic, anomalies of inheritance; (9) Neoplastic, anomalies of new formation. Such a classification, perhaps with slight alterations, might be universally applicable. It has an advantage over most others on account of each term having a clearly defined meaning. But whether this or some other classification be adopted, such as Osborn's, it should at least be universal in its adoption, and should cover all the chief divergences from any common average taken as standard.

In conclusion, I fail to see the use of continuing this discussion as it now stands. No case has been yet adduced by one side which has in any sense been regarded by the other as final. If I am right, the cause is to be sought in the loose way in which variations have been classed. If it is urged that to make such a close study would require almost a lifetime, and exceptional capabilities besides, it should be recollected that it would at least settle a question of extreme difficulty, and one which, until answered, will greatly interfere with the progress of biology, while its solution would in addition probably help to solve many problems in modern medicine and kindred sciences which must otherwise remain more or less incomprehensible.

J. LIONEL TAYLER.

[1] Third Boyle Lecture, Oxford.

II

The Late Lamented Latreille

A Study in Names

PIERRE ANDRÉ LATREILLE died between sixty and seventy years ago, and now some amiable persons in Washington wish to canonise him, not for any of his good works, but for one of his weakest. In 1810 he published a volume, neither his first nor his last, on the inexhaustible theme of crustaceans, arachnids, and insects. In it he still mixes up the isopods with the arachnids, although Lamarck had nine years earlier properly placed them among the crustaceans. In it he passes some not undeserved commendations on his own earlier writings, and on those of J. C. Fabricius some criticisms of questionable temper and taste. After a tolerably interesting introduction of some eighty pages, he devotes the bulk of the volume to a methodical table of genera, containing nothing, or next to nothing, that was new. He leaves his readers to guess at what dates the genera had appeared, and by what authorities all of them except his own had been established. He leaves his readers also to guess whether any particular genus contained but a single species or a score. In the last twenty-four pages, in small print, as though an after-thought or as a substitute for an index, he gives a " Table des genres avec l'indication de l' espèce qui leur sert de type." This precious performance is now being elevated into a standard of scientific nomenclature, not because of any imagined thoroughness or excellence, but simply because of its imperfection. For each genus, whether large or small, it mentions one species and only one, with no word of description, with no reason given for the selection, and this casual mention in a college manual is supposed, forsooth, to confer the rights of a type-species !

It becomes important, therefore, to cast a little light upon Latreille's catalogue, and to ascertain the meaning and application of the word ' type ' which he uses at the head of it. But a glance must first be taken at more modern writers. In 1896-97 a lively discussion was carried on in *The Annals and Magazine of Natural History*, ending, as I fondly hoped, in acceptance of the decision that the lobster was properly assigned to the genus *Astacus*. The controversy had been opened somewhat earlier in Dr Herrick's book, "The American Lobster," and the review of that book which appeared in *Natural Science* for June, 1896. Dr Herrick's advisers on the subject were Dr Walter Faxon and Miss Mary J. Rathbun. They have since returned to the attack.[1]

Dr Walter Faxon is, I believe, the greatest living authority on crayfishes. Miss M. J. Rathbun is one of our foremost authorities on crabs. Both these writers are distinguished for their learning, industry, and acuteness. To see them both relying on Latreille's feeble book is like contemplating the first Napoleon as Emperor of Elba. In the outset these eminent naturalists were confessedly under the impression that, in writing my "History of Crustacea" (Internat. Sci. Series, 1893), I had overlooked Latreille's "Considérations Générales" of 1810, whereas, in point of fact, I had given them my particular consideration and had, then as now, come to the conclusion that on questions of nomenclature, the book containing them was of no importance.

It will probably be admitted on all sides that when an author establishes a genus for a single species, that species must be regarded as the type of the genus. It will probably be further admitted that, of several species contemporaneously placed in a genus, any one singled out by the author himself as the type of the new genus holds a preferential right to the generic name. But, supposing that the author has not selected a type, and that the species of his genus have eventually to be re-distributed among several genera, which of them is then entitled to be retained in the original genus? Apart from other guidance, might not one innocently suppose that the species placed first by the author ought to have precedence? It has priority, even if it be on the same page with its successors. I ask whether one might not innocently suppose this, because the innocence of the supposition is called in question by Dr Faxon. He says:—

"It is hard to believe that this contention of Mr Stebbing's is made in good faith, involving as it does an unreasonable and long-discarded method of ascertaining a type. Such a method is repudiated every time we concede to an author, who first sub-divides a genus in which no type has been specified, the right to restrict the original name to such part of it as he pleases. It is not true that the first species is presumably the author's implied type. Fabricius's genus *Astacus* was formed by a dismemberment of the genus *Cancer* of Linnaeus, and the sequence of the two species under

[1] A revision of the nomenclature of the Brachyura, by Mary J. Rathbun. *Proc. Biol. Soc., Washington*, vol. ii. pp. 153-167. 1897.
List of the Decapod Crustacea of Jamaica, by Mary J. Rathbun. *Annals of the Institute of Jamaica*, vol, i., No. 1. 1897.
Observations on the Astacidae in the United States National Museum and in the Museum of Comparative Zoology, with descriptions of new species, by Walter Faxon. *Proc. U.S. Nat. Mus.*, vol. xx., No. 1136. 1898.

consideration [European lobster and crayfish] in Fabricius's works was undoubtedly derived from the 'Systema Naturae,' wherein (in the twelfth edition) *Cancer gammarus* stands as No. 62, *Cancer astacus* as No. 63, in the genus *Cancer*. A better, though not a valid, claim might be set up for *A. fluviatilis* as Fabricius's implied type of his genus *Astacus*, since that species is the *Cancer astacus* of Linnaeus."

On such a subject the imputation of bad faith can only make one fancy that he who imputes it has been listening to the old machiavellian advice, "You have no case; abuse the plaintiff's attorney." Between scientific comrades such innuendoes should surely be dispensed with, and they are almost always a tactical mistake, since retaliation is often only too tempting and too easy. But the whole passage seems founded on misconceptions. "It is not true," Dr Faxon says, "that the first species is presumably the author's implied type." There is really no question at all about what is implied or what is to be presumed, but only about the rule convenient to follow, when an author has himself left us in the dark. In that case to follow the rule of priority is not merely not unreasonable, but truly the simplest and most consistent plan that could be adopted. It is an astonishing argument to urge that Fabricius could not have intended the lobster to be the type of his genus *Astacus*, because he followed Linnaeus in giving it precedence. It is, besides, a plain matter-of-fact, that in arranging the species of this genus, Fabricius by no means slavishly follows the Linnean order. But that point need not be laboured, since Dr Faxon takes his stand on a rule not affected by it, a rule laid down long after the time of Fabricius and Latreille. According to this rule or recommendation, "When the evidence as to the original type of a genus is not perfectly clear and indisputable, then the person who first sub-divides the genus may affix the original name to any portion of it at his discretion, and no later author has a right to transfer that name to any other part of the original genus." Whether wisely or not, that rule gives a special privilege of ignoring strict priority to persons who deal with what may sometimes be a very difficult and troublesome task. We have now to consider the retrospective action of that rule upon the question before us.

It is not disputed that until 1819 the genus *Astacus* contained both the European lobster and the European crayfish. Then Leach assigned the latter to a new genus, *Potamobius*, that is to say, he first subdivided the genus *Astacus* as it then stood, and he affixed the original name to the lobster. You may say he followed the rule of strict priority. You may say he did what he did at his discretion. To persons of commonplace intellect like myself it must seem as if, either way, his names were bound to stand. The illustrious American professor Dana rejects them on grounds which, I think, Dr Faxon and Miss Rathbun cannot possibly accept; but, apart from those grounds, Dana declares that "Leach has undoubted priority."

Now, then, at last we may return to Latreille and his magic volume of 1810. Latreille was not the author of the genus *Astacus*, and he made no attempt to subdivide it, so that it is difficult to see by what rule he could have any right to assign the type. But it is superfluous to debate his right, if he never did or thought of doing that which Dr Faxon so confidently affirms him to have done. Observe, first, that the family to which Latreille assigns *Astacus* as the initial genus, is called by him "Homardiens, Astacini," and homard is not French for crayfish, but for lobster. Yes, but then in that table of genera "avec l'indication de l'espèce qui leur sert du type," he gives "Ecrevisse, *Astacus fluviatilis*, Fab.," and to anyone who urges that Latreille was only giving an example of the genus, not the type in any technical sense, Dr Faxon makes reply :—

"As I understand it, the French word 'type' means 'model,' 'type,' or 'standard,' not 'example' or 'illustration' (Gallicè *exemple*). I see no reason for going behind Latreille's plain words to indulge in uncertain speculation concerning his possible meaning. If Mr Stebbing is unwilling to allow Latreille the use of the word 'type' in its technical sense, by what 'statute of limitation' will he fix the year when the word acquired that meaning? Even if it be admitted that there is some doubt concerning the significance of the word 'type' as employed by Latreille, the benefit of the doubt should, by a reasonable ruling applicable to all such cases, be given to a long established terminology."

"Some doubt," indeed! Happily in this instance there is no doubt. In spite of his great learning, Dr Faxon seems to speak of Latreille as if he were a man of only one book. Latreille was a voluminous writer. He can throw abundance of light upon himself. In 1825 he published his "Familles naturelles du Règne Animal," in which genera are mentioned for the most part only under French names, and in which scarcely any species are mentioned at all. But of the very few which happen to be noticed one is *Astacus marinus*, the lobster. Now, seeing that in this work Latreille recognises three distinct genera, "Nephrops, Homard, Ecrevisse" (p. 279), and that he calls the homard *Astacus* (p. 274), he must have accepted a different generic name for the ecrevisse. He evidently knows nothing about his own fine doings. It would have amused him to learn that he was no longer at liberty to speak of the lobster as *Astacus*, because already in 1810, in a happy-go-lucky list of species, the river crayfish had been "specified as the type" of that genus—by himself. But light can be shed on the subject from his earlier works as well as his later. In 1802 he published the first four volumes of his "Histoire naturelle, générale et particulière, des Crustacés et des Insectes." Here, just as in 1810, we have "Famille cinquième. Homardiens; *astacini*," with the first genus "Ecrevisse; *astacus*," signalised by a single species thus—"*Exemple. Astacus fluviatilis*, Fab." A little research, therefore, would have shown Dr Faxon that Latreille uses 'exemple' and 'type' as precise equivalents. The sixth volume of the "Histoire," published in 1803, describes seven species of *Astacus*, giving the first place to "Ecrevisse homard; *astacus marinus*." In the "Genera Crustaceorum et Insectorum," published in 1806, where only two species of *Astacus* are mentioned, it is still the *Astacus marinus* that holds the place of honour. But if this local position on the printed page gives no indication of Latreille's opinion as to the typical species, why, it may be wondered, does he trouble himself to tell us in the "Histoire" (vol. vi., p. 230) that, "il est facile de conclure des observations d'Aristote, que son *astacos* est l'écrevisse de mer ou le homard," and offer proofs that by this name "Aristote désigne positivement le homard." It cannot be supposed that he wished to flout Aristotle by taking as type of the genus, not the lobster which Aristotle positively designates, but the river crayfish, of which, he says, Aristotle makes no mention.

The same line of argument which indicates for Leach the generic names of the lobster and the crayfish, will indicate for Stimpson the distinction between the prawns *Leander* and *Palaemon*, and once more for Leach the distinction between the sand-hopper *Talitrus* and *Orchestia* the shore-hopper. *Talitrus* is Latreille's own genus. In 1802 he gave two examples of it. The first has from then till now retained the name of *Talitrus*. The second was transferred to *Orchestia* by Leach in 1814. It is this latter species which Latreille in 1810 names by itself as 'type' of the genus *Talitrus*. But that he had the least intention of establishing it as the type, it is not only hard but impossible to believe, for as well in the "Nouveau Dictionnaire d'Histoire Naturelle," vol. i., 1816, as in the "Familles Naturelles" of 1825, and "Le Règne Animal," vol. iv., 1829, he recognises *Orchestie* and *Talitre* as distinct genera. Now *Orchestia* could only be a synonym of *Talitrus*, were the species mentioned by Latreille in 1810 as an illustrative example to be taken as a governing type.

In 1801 the brilliant Lamarck published his "Système des Animaux sans Vertèbres," a manual on much the same plan as Latreille's volume of 1810. In his "Avertissement" he says: "Pour faire connoître d'une manière certaine les genres dont je donne ici les caractères, j'ai cité sous chacun d'eux une espèce connue, ou très-rarement plusieurs, et j'y ai joint quelques synonymes que je puis certifier ; cela suffit pour me faire entendre." The expressions, 'j'ai cité une espèce connue' and 'cela suffit pour me faire entendre,' are exactly applicable to Latreille's list in 1810, which, whatever its merits or shortcomings, must be absolutely acquitted of any charge of type-mongering ambition.

Dr Faxon, while upholding *Astacus* as a genus of crayfishes, divides it into two subgenera, *Cambaroides* and *Astacus*, and, as he identifies the *Astacus fluviatilis* auctorum with *Cancer astacus* Linn., the name of this species with him will become *Astacus* (*Astacus*) *astacus* (Linn.) Faxon. It is a matter of taste, but one would scarcely expect a countryman of Frank Stockton, Mark Twain, Artemus Ward, and so many others capable of acutely distinguishing between the serious and the absurd, to struggle for the honour of being stepfather to such a designation. By ignoring the rightful claims of *Potamobius* Dr Faxon is only prolonging confusion in a group with which his own reputation is so honourably connected.

THOMAS R. R. STEBBING.

III

Pentacrinus: a Name and its History

IT is the misfortune of Systematic Zoology that it combines two very different studies: the study of things and the study of names. The former is the more important; but the latter is necessary for the clear interchange of ideas. In the study of things it may be useful to examine the writings of those who have gone before; but, except for the sake of giving honour where honour is due, or of enlivening matters by cheerful criticism, such examination is not forced upon us: it may even lead us astray. The study of names, however, cannot but be a study of other men's opinions as transmitted to us in their writings. The former study—of things or facts—is bound by no laws save those of nature and truth; the latter is circumscribed in every direction by the rules established by zoologists themselves. It is no wonder if the arbitrary, man-made branch of the science occasionally accords but ill with what we believe to be the facts of nature.

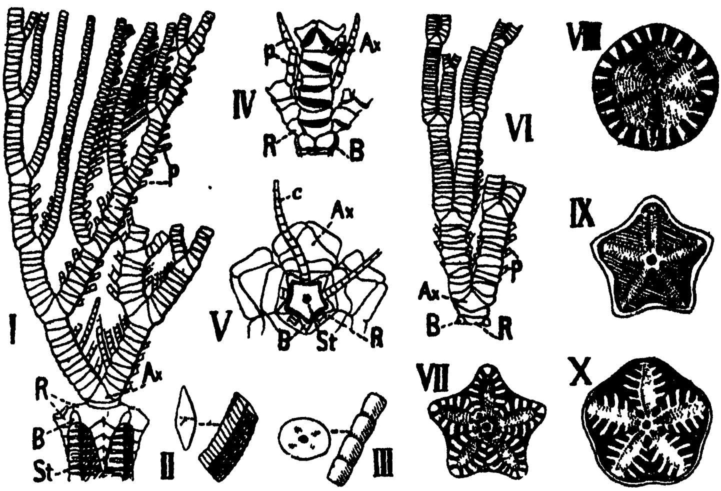
The rules of nomenclature are rigid, but knowledge changes, and, we trust, advances. Hence a name that was proposed with a definite connotation ceases to have that connotation, and may come to mean almost the opposite of what its author intended. We systematists know better than anyone else that these things are absurd, but no better method of naming has ever been proposed. Were we to make a clean sweep of all names and to start with a *tabula rasa*, the strictest adherence to the strictest code of nomenclature could not prevent a recurrence of confusion. The sole remedy is omniscience. Admitting all this, it is none the less, but rather the more, our duty to protest against those who, under the seductive guise of expediency, attempt to override or to evade the accepted rules. Authority is a shadow, customs change, "our little systems have their day," and so forth. None of these is an excuse for proven error.

These homilectics are intended to pave the way for a protest in one particular instance: the misuse of the name *Pentacrinus*. Every naturalist thinks he knows what *Pentacrinus* is, but it is probable that a palaeontologist and a neontologist would point to very different forms as representative species; while it is certain that what zoologists as a rule understand by *Pentacrinus* has no right to that ancient title.

First let us see what are the concrete facts that have to be represented in nomenclature.

The Sub-family Pentacrininae contains crinoids that have a long stem (*St*), with five angles more or less marked, and with cirri (*c*) or side-branches springing from the sides in whorls, usually of five. The stem possibly ends in a root in early life; but it has the power of breaking away just below each whorl of cirri, so that the adult crinoid is to some extent free-moving, and attaches itself by the cirri at the end of its stem. The stem-ossicles or columnals are united by five bundles of ligament, on which they are threaded, as it were, and the articular surfaces are marked by crenelate elevations which surround these bundles, and so assume a petaloid shape; the elevations of one columnal fit into depressions in the next. This stem supports a crown; and this crown may be divided into (*a*) a cup containing the viscera, and (*b*) branching arms. The cup consists essentially of three circlets of five plates alternating with one another, namely: Infrabasals, next the stem; Basals (*B*); and Radials (*R*) supporting the arms. But as a rule the infrabasals are minute or atrophied in the adult, and the basals also are often so reduced in size that the radials rest partly on the top columnal; while to make up for this the lower ossicles of the arms (the proximal brachials) form part of the sides of the cup, *i.e.*, help to surround and support the viscera. The arms always bear fine branchlets, given off regularly on alternate sides, and known as pinnules (*p*), and in addition the arms always fork at least once.

The Sub-family includes at least four genera, which, to avoid controversy, I shall call A, B, C, and D. A begins in the Trias and persists to Eocene time. It has a cylindrical or prismatic stem, and the crenelations on the articular faces of the columnals are confined almost entirely to the circumference, while the five ligament areas are limited by five thin, raised and slightly crenelate lines, which radiate from the centre of the columnal (fig. VIII.). The cup is not well known; the arms fork more than once into equal divisions. B was dominant in Liassic seas, less abundant in Oolitic. The crenelations on the articular faces of the columnals are delicate, and are arranged in five narrow petals quite distinct from one another (fig. IX.). The cirri are often numerous and are laterally compressed so as to have an elliptical or rhomboid section (fig. II.). In the cup, infrabasals can usually be distinguished; the radials are prolonged downwards over the columnals next the cup in spine-like processes (fig. I.); they support large arms which branch in such a way that each main division bears a number of minor armlets all coming off on the same side (fig. I.). C is found in Triassic and Jurassic rocks, but became more prominent in Cretaceous times, and is the chief representative of the Sub-family at the present day. The crenelations of the columnals are coarse, and are arranged in petals so broad that they run together towards the centre (figs. VII., X.) and are not distinct as in type B. The cirri are transversely elliptical or circular in section (fig. III.), and the whorls not so numerous. Infrabasals have only been detected once, and in that instance are quite minute. Basals may or may not form a complete circlet. The radials are not prolonged downwards into spines. The arms fork once or many times, but always equally



PENTACRININAE.

I. *Pentacrinus fossilis*, Blumenbach. Portion of stem, cup, and portion of arm showing rami, ramules, and pinnules.
II. *Pentacrinus fossilis*. Portion of a cirrus, and articular facet of a cirral.
III. *Isocrinus asteria*, Linn. sp. Portion of a cirrus, and articular facet of a cirral.
IV. *Metacrinus moseleyi*, P. H. Carpenter. Cup and proximal portion of an arm. After P. H. C.
V. *Isocrinus pendulus*, H. v Meyer. The cup seen from below, with portion of stem, bearing cirri, still attached, and with proximal brachials. After H. v. Meyer, em P. de Loriol.
VI. *Isocrinus pendulus*. A radius, showing regular bifurcation of arm. After De Loriol.
VII. *Isocrinus amblyscalaris*, Thurmann. Closely allied to or identical with *I. pendulus*. Articular facet of an internodal columnal. After De Loriol.
VIII. *Balanocrinus subteres*. Munster, sp. Articular facet of an internodal, diagrammatised from E 991 in Brit. Mus. Our type A.
IX. *Pentacrinus fossilis*. Articular facet of an internodal, diagrammatised from 51170 in Brit. Mus. Our type B.
X. *Isocrinus asteria*. Articular facet of an internodal, diagrammatised from E 6456 in Brit. Mus. Our type C.

(fig. VI.). There are two brachials below the first forking (*Ax*), and these bear no pinnules. D is known only from recent seas, and occurs in the West Pacific. It resembles C, but has 5-8 brachials below the first arm-forking, and of these the 3rd, 4th, 6th and 7th bear pinnules (fig. IV.). The basals form a complete circlet.

No dispute arises with regard to the names of A and D. De Loriol has perhaps strained a point in extending to A the name of

Balanocrinus, primarily applied by L. Agassiz to an abnormal stem-fragment of another genus; but nothing would be gained by contesting his action. D is the genus called *Metacrinus* by P. H. Carpenter, who took the name from a MS. label of Wyville Thomson.

C and D are the types about which there is confusion. C is called *Extracrinus* by Carpenter (1884) and De Loriol (1886), while the name *Pentacrinus* is restricted by them to D. This course has been followed in the Geological Department of the British Museum. I shall now prove that by the rules of nomenclature, by the practice of many early naturalists, and by a literary history of three centuries, the name *Pentacrinus* is the property of type C.

First let us remember that living examples of a Pentacrinid have been known to men for less than a hundred and forty years, while the fossils have been well known for centuries. The name *Pentacrinus* itself is supposed to be due to Georg Bauer, better known as Agricola. This is what he says in his work "De Natura Fossilium" Lib. v. pp. 256-8 (1558). First he describes the "trochites," circular bodies with smooth circumference but with upper and lower surfaces covered with striae that radiate from the centre, with a glittering fracture and regular cleavage like Jew-stones [*i.e.*, fossil spines of sea-urchins], giving off gas when placed in vinegar. The "entrochos" consists of trochites not yet separated from one another. Next, under the name Ceraunia, Agricola describes certain bodies, which appear to be fossil sea-urchins. He then proceeds: "But as the Entrochos when divided represents the effigy of wheels, so the Encrinos does of lilies. For when one angular part is separated from another, each shows a set of five lilies. For the elevations of the one fit into the striae of the other. And any such part has five angles and as many sides, and on each is a quintet of lilies. Whence it may be called pentacrinos in Greek. But just as the entrochos consists of many trochites, so the encrinos consists of many pentacrini. . . . The stone of the pentacrini when broken has the colour, smoothness, and glitter of the Jew-stone."

It is clear from this that the syllables "crinos" originally referred solely to the leaf-like or petaloid markings on the joint-surfaces of the stem, and not to the lily-like appearance of the whole animal, just as the Japanese call a similar fossil 'the plum-flower stone.' Further, that both *Encrinos* and *Pentacrinos* applied to the same fossil under different aspects. This was understood by the successors of Agricola, among whom Lachmund (1669) is conspicuous. It was, however, this writer who introduced confusion, for in his chapter "De Encrino et Pentacrino" he says, "To Encrinus I refer a stone, which consists of many other small and angular stones, the elevations of which are inserted into the striae of another, just as the skin, *imprimis* in the anterior part, around the legs of fowls, as is seen in the figure, where are two series of separate ash-coloured stones, and others whitish, elegantly joined into one mass; the substance is that of the Jew-stone." The figure represents five biserial arms of a crinoid, broken away at one end, so as to expose the pinnules. There has never been any doubt as to Lachmund's meaning; the genus to which he referred was soon figured by others from more complete specimens, and was distinguished as *Encrinus* or *Encrinos Lachmundi.* Under that name it was referred to by the illustrious Luidius or Lhuyd (1699). But it was soon discovered that the arms of *Encrinus Lachmundi* were attached to an *Entrochus*, or stem of round columnals with radiating striae, and not to an *Encrinus*, or stem composed of *Pentacrini.* Then there arose a false etymology, and many who did not know their literature supposed that *Encrinus* meant a fossil like a lily with its flower closed: "Κρίνον heisst eine Lilie, Ἔγκρινος soll daher eine geschlossene Lilie bedeuten," wrote J. S. Schroeter (1778), and laughed at those who, as Bertrand (1763) and Guettard (1761), were conservative enough to use *Encrinus* in what, as he seems to have forgotten, was its original sense. The idea seems to have arisen with Harenberg (1729) who supposed that *Encrinus Lachmundi* actually was a petrified lily; hence the term "stone-lilies" even now applied to the Crinoidea. There were not wanting writers to protest with a mild solemnity that this fossil was really much more like a cob of maize than any lily whether open or closed.

About the time of Linnaeus then there were two views or practices. The one applied *Encrinus* or *Encrinites* (the terms were used indifferently) to stems composed of *Pentacrini, i.e.* star-marked columnals, also known as *Asteriae.* The other, and the more favoured, distinguished *Encrinus* from *Pentacrinus* thus:—The Encrinite has a round stem or *Entrochus*, composed of *trochitae*, above this is the "Gelenkstein," called *Pentagonum* by Rosinus (1719), Lachmund (1669), and others, [= Cup], and this supports a crown which consists of rays. The Pentacrinite has a stem, usually pentagonal, the separate columnals of which are the so-called *Asteriae*, which form the *Asteriae columnares*; it has no "Gelenkstein," and its crown is fasciculate.

Linnaeus in the tenth edition of his "Systema" mentioned no recent stalked crinoid; while both here and in the later editions, fossil crinoids were placed in the mineral kingdom under the comprehensive and inappropriate heading *Helmintholithus*, with hardly an attempt to arrange them under the binominal system.

The earliest writer after 1758 to help us in this matter is C. F. Schulze (1760), whose work appears to Mr C. D. Sherborn and myself to contain Latin names used in the Linnaean manner, such as *Astropecten regularis, Palmipes coriacea*, and *Astrophyton arach-*

noides. This is the earliest post-Linnaean work to which I can trace the name *Encrinus*, here applied in the sense of Schroeter to the fossil that is now universally known by that name, though ascribed to Lamarck, who was forty years later. There is a full description with figures of the common species usually known as *E. liliiformis*, but no trivial name is used by Schulze. This step finally removes *Encrinus* from all rivalry with *Pentacrinus.*

Again the fact must be emphasised that up to this time the name Pentacrinus or Pentacrinite had of necessity been applied to some fossil crinoid. Further, I would draw attention to the word "büschel-formig" (= fasciculate) applied to the arms of the Pentacrinite; interpreting this by the various figures given (*e.g.*, of Hiemer's Caput Medusae, 1724; plate i. in vol. iii. of Davila, 1767; the Gmelin Pentacrinite in Knorr, 1755, plates xi. b and c), I suggest that it refers to the presence of numerous armlets borne by the main arm-branches, the structure seen in our type B. No figure of a fossil crinoid of our type C is known to me before 1800.

In 1761 was published the first account of a living stalked crinoid, the Palmier marin, so admirably described and figured by Guettard (1761), and also made known by the curious work of Don Antonio Parra (1787). This was seen to have a stem formed of *Asteriae*, and for this reason no doubt it received from Linnaeus (1767) the trivial name *Asteria*,* when he placed it erroneously in the genus *Isis.* The discovery of this form gave the long-desired explanation of the true nature of fossil Encrinites and Pentacrinites, and since the species resembled the Pentacrinites more closely than the Encrinites, it was by many regarded as "the true original" of the former fossils. This phrase demands explanation. The old naturalists regarded fossils as the representations or images of beings now living on the Earth, and the living *Echinus*, for instance, was the original of the fossil Echinite, just as a picture might be the original of an engraving. Some then, among whom was the learned J. E. Walch, considered the *Pentacrini* with cirri to be imprints of an animal like the Palmier marin. The zoologists do not seem altogether to have accepted this view: those who, as Ellis (1762) and Lamarck (1801 and 1816) compared it with the fossils, referred it to the genus *Encrinus;* while Blumenbach in the various editions of his "Handbuch der Naturgeschichte" (1779 onwards) insisted that the Pentacrinite, though it indeed resembled the Palmier

* P. H. Carpenter (1884) writes *Pentacrinus asterius*, saying (p. 308) "the expression *Pentacrinus asteria*, used by Lütken, Thomson, and myself being a false concord; for it is evident that the etymology of Linnaeus's name *Isis asteria* is the adjective ἀστέριος, starry, and not the noun *Asteria*, cat's eye. I am indebted for this tardy correction to the critical acumen of my friend, Professor F. Jeffrey Bell." I believe that *Asteria* is a substantive in apposition, and that no question of concord of genders arises.

marin, was not congeneric therewith. Blumenbach was well aware that, as he said in 1790, "Hiemer's Medusa-palm (*Helmintholithus portentosus* Linn.) has long had the claim to the name Pentacrinit."

People in those days did not attach such importance to names, priority, and preliminary notices; but even so it is rather surprising that the name Pentacrinite was not used in a regular Latin form and in accordance with the binominal system until 1804. In that year was published Heft 7 of Blumenbach's "Abbildungen naturhistorischer Gegenstände," a work that rigidly employs the Linnaean system of nomenclature. No. 70 of this is the description and figure of a "Medusenpalme" from Dorsetshire, under the name *Pentacrinites fossilis*; as such it is distinguished from *Encrinites fossilis*, first named in the preceding Heft (1802), as well as from the species common at Boll in Würtemberg. This then, a well-known representative of our form B, is the type-species of the genus *Pentacrinus*.

As to the difference of termination, it is perfectly clear that the early writers made no real distinction between *Encrinus* and *Encrinites*, *Pentacrinus* and *Pentacrinites*. The termination *ites* was generally applied to the organism when in a fossil state, but was not held to indicate generic distinction. It is often confused with *lites*, and said to be a corruption of λίθος, a stone; but this is an etymology invented afterwards, like Schroeter's derivation of Encrinos. It is merely the Greek suffix—ίτης, meaning "of the nature of." It has never been maintained that the chance addition of this suffix constituted a difference of name, and all modern writers on Crinoidea have merely dropped it as out of harmony with modern views as to the nature of fossils.

Now, if most early writers drew a correct distinction between *Pentacrinus* and the Palmier marin, how, it may be asked, has the name become restricted to the genus (C) of which the Palmier marin is the chief living representative? There may have been one or two writers who, as Oken (1815), referred *Isis asteria* to *Pentacrinus*; but it was not till J. S. Miller published his "Natural History of the Crinoidea" (1821), that this view became at all general. In his "*Pentacrinites* vel *Pentacrinus*," Miller included *P. caput-medusae* (= *Isis asteria*, Linn., *Encrinus caput-medusae*, Lamarck), *P. briareus* (= *P. fossilis*, Blumenb.), *P. subangularis* (? = *Caput-medusae*, Hiemer, *Helmintholithus portentosus*, Linn.), *P. basaltiformis*, and *P. tuberculatus*. The last two and the first one belong to type C, the others to type B. It was the authority of J. S. Miller, whose fame is indeed well-deserved, that led subsequent writers to follow his example; and the name *Pentacrinus caput-medusae*, though incorrect in both its parts, was further fixed in the minds of zoologists by the classical memoir of Johannes Müller, "Ueber den Bau des Pentacrinus caput-medusae" (1843).

The next step in a wrong direction was that taken by T. & T. Austin (1847), who, while rightly distinguishing types B and C, retained the name *Pentacrinus* for the latter, and gave to the former the new name *Extracrinus*. This name, however, was not accepted by Quenstedt (1874) the chief authority on Liassic crinoids; and in fact the distinction between types B and C was not admitted until the important works of P. H. Carpenter (1884) and P. de Loriol (1886-9) placed it on a firm basis. This was the opportunity for a correct application of the rules of nomenclature. Unfortunately Carpenter chose to exalt the authority of J. S. Miller, and to regard him as the Linnaeus of the Crinoidea. Hence he took as the genotype of *Pentacrinus*, the species first mentioned by Miller, which happened to belong to type C, and for B he adopted the Austins' name, *Extracrinus*. It is worth noting that Miller himself, in the circular announcing the publication of his book, stated that it would include "the genera *Encrinus* and *Pentacrinus* of former Authors." Miller never claimed to be the founder of *Pentacrinus*; his diagnosis of it was in terms applicable only to type B, "the *Pentacrinus* of former authors," and all that was original was the transference of the *Encrinus caput-medusae* of Lamarck to this genus despite the correct general opinion that it was distinct. Moreover, if he intended to date crinoid nomenclature from Miller, Carpenter should have accepted the trivial name *Caput-medusae*, instead of going back to *Isis asteria*, Linn.; also he should, for another genus, have retained the name *Comatula*, instead of going back to De Frémenville's *Antedon*. As a side-issue it may be mentioned that *Caput-medusae* was really the name applied by pre-Linnaean naturalists to the branched ophiurids, the *Astrophyton* of Linck and his successors, and the *Gorgonocephalus* of Leach. The fossil pentacrinid crowns of Boll were supposed by some to belong to this genus (*e.g.*, Hiemer, 1724), but Lamarck's assignment of the name to the Palmier marin merely showed an ignorance of its previous history.

It is now, I trust, clear that the name *Pentacrinus* belongs to type B. There remains for consideration the name to be applied to C. The earliest name found is *Polycerus*, Fischer de Waldheim (1811). A statement twice repeated in this author's rare pamphlet, that "le palmier marin est le vrai original de ce genre," might be taken as a convincing argument in favour of the adoption of this name. But the word 'original,' as already explained, was in no sense equivalent to our modern expression 'type-species' or 'genotype.' *Polycerus* was the "genre que quelques Naturalistes ont appellé avec WALCH *pentacrinite*," and was proposed, in allusion to the numerous arm-branches, simply because Waldheim objected to the name *Pentacrinus* as inappropriate or ambiguous. It is a pure synonym of *Pentacrinus*, and the above-quoted sentence merely means that in Waldheim's opinion the Palmier marin was the living representative of the genus. He emphasised this, because confusion had arisen, thanks to Ellis, Mylius (1753), and others, between true crinoids and a polyp dredged off the coast of Greenland—the *Isis encrinus* of Linnaeus (1758), *Pennatula encrinus*, Pallas (1766), or *Umbellularia groenlandica*, Lamarck (1801). Waldheim, in short, was far from proposing any distinction between the Lias fossil and the living Palmier marin; on the contrary he was one of the first to refer the latter to the old-established genus.

For an appreciation of the distinction between our types B and C, couched in scientific language and expressed by a correct nomenclature, we must turn to H. von Meyer (1837). This eminent palaeontologist described a new crinoid from the Calcaire corallien of Besançon (Doubs), and, while recognising its affinity to *Pentacrinus* pointed out the following differences: the cirri are round, not compressed rhomboid or oval; there is no downward prolongation of the radials over the stem; the arms fork regularly and equally instead of bearing armlets (figs. V., VI.). The last feature suggested the name *Isocrinus*, the name of the species being *I. pendulus*. It is clear from his description and figures that Von Meyer was dealing with a species of our type C; this was recognised by P. H. Carpenter (1880,-84). Unfortunately Von Meyer was unable to see the sutures between basals and radials in his specimen, and described it as having a base formed of a single pentagonal plate, with no elements corresponding to what we now term radials. This led either to the non-recognition of *Isocrinus*, or to its recognition in a wrong sense, as by D'Orbigny (1849), Desor (1845), and Pictet (1857). De Loriol (1889), however, has re-examined the type-specimen of *I. pendulus*, now in the Natural History Museum of Vienna, and has shown that it possesses five small basals, and five radials. When Von Meyer described the specimen it retained a portion of the stem and cirri, and this no doubt obscured the sutures of the minute cup. Since then these structures, along with portions of the radials and basals, have been destroyed, and the true composition of the cup exposed. Any doubt that may have existed as to the systematic position of *Isocrinus pendulus* is now dispelled. It is congeneric with all species of our type C, and to them the name *Isocrinus* must be extended.

In consequence of the confusion caused by the lumping action of Miller, by the erroneous repartition due to the Austins, by Von Meyer's mistake, and by the slow acquisition of knowledge concerning the structure of the cup in Pentacrininae, various other names were proposed subsequently. *Cainocrinus*, Forbes (1852), of which *Picteticrinus*, De Loriol (1875) is admitted by its own author (1897) to be a synonym, was founded on the presence of a closed basal circlet a character which, as Carpenter has shown, is of no classificatory value; the species belong to our type C and the names are therefore synonyms of *Isocrinus*. *Chladocrinus*, L. Agassiz (1835), was separated from *Pentacrinus* of Miller in these words: "On pourrait désigner sous le nom de Chladocrinus les espèces dont les rayons accessoires forment des verticilles plus ou moins distans." De Loriol and Carpenter suppose the word "rayons" to refer to the cirri of the stem; but this makes the sentence a mere repetition of "tige portant de distance en distance des rayons simples, verticillés," which is given as a character of *Pentacrinus*. I suggest that "rayons" refers to "rayons du disque," *i.e.*, arms, which may, as Agassiz says, "se ramifier en de nombreux appendices pinnés à leurs bords"; these appendices or accessory branches are, surely, the armlets of our type B. On this view the sentence has a meaning that does not insult the intelligence of Agassiz, while the name *Chladocrinus* is appropriate and antedates *Extracrinus*, Austin. At the same time it is a synonym of *Pentacrinus* (*sens. str.*). *Heterocrinus*, O. Fraas, has been quoted more than once as a synonym of type B. Fraas (1858) did distinguish between two types of branching, "Isocrine" and "heterocrine"; but he carefully explained that he did not intend to propose any new generic name, and he doubtless knew well enough that the name *Heterocrinus* was preoccupied by J. Hall.

The conception of *Pentacrinus* here maintained has been urged by no less an authority than Sir C. Wyville Thomson (1864). Referring to *P. caput-medusae*, he says: "Another and a widely different species, *Pentacrinus briareus*, from the Lias of the South of England, seems, however, to have a just claim to be recognised as the type of the genus *Pentacrinus*." For *P. caput-medusae* and one or two fossils which closely resemble it, Sir Wyville therefore proposed the name *Cenocrinus*. In no case could this name be accepted, since it has the same derivation as *Cainocrinus*, Forbes; and whichever be the correct spelling, in that way should both be written. This question, however, does not arise, since both were anticipated by *Isocrinus*. In the same paper Sir Wyville proposed the separation of *Pentacrinus decorus* and allied species as a subgenus, *Neocrinus*; but the need for this step has been denied by Carpenter.

The conclusions to which we are brought concerning the names applicable to types B and C are as follows:—

B.—PENTACRINUS, Blumenbach, 1804; Type, *P. fossilis* (= *P. britannicus*, Schlotheim; *P. briareus*, Miller). Synn. *Polycerus*, *pars*, Waldheim, 1811; *Chladocrinus*, Agassiz, 1835; *Extracrinus*, Austin, 1847.

C.—ISOCRINUS, Von Meyer, 1837; Type, *I. pendulus* (= *Pentacrinus amblyscalaris*, Thurmann ?). Synn. *Isis*, *pars*, Linnaeus, 1766; *Encrinus, pars*, Lamarck, 1801 and 1816, Blumenbach, 1779-1807; *Pentacrinus, pars*, J. S. Miller, 1821; *Pentacrinus*, T. and T. Austin, 1847, P. H. Carpenter, 1884; *Cainocrinus*, Forbes, 1852; *Picteticrinus*, de Loriol, 1875; *Cenocrinus* and *Neocrinus*, W. Thomson, 1864.

These conclusions will not be agreeable to those who follow P. H. Carpenter in driving a coach-and-six through the rules of nomenclature, a pastime in which my regretted friend too often indulged. But they will please those palaeontologists who, with the eminent Von Zittel, still call the fossils of Würtemberg and Dorset by the name *Pentacrinus*, a name that has been theirs for three centuries and a half, in the possession of which they were legally confirmed by the illustrious Blumenbach, to whose action Eichwald (1829) is a trustworthy contemporary witness.

With the necessity for some such step I have long been acquainted, but the accumulation of the requisite literature has been a slow process. The general neglect of ancient writers, due partly to the apotheosis of Linnaeus, partly, as Sir Archibald Geikie puts it, to the engrossing interest of the present activities of our science, is a neglect that leads to much misconception, and to much that is not merely unjust but illegal. Because Linnaeus lived, are we to ignore Rosinus, Walch, and Schroeter? Because J. S. Miller wrote a learned monograph, are Blumenbach and Von Schlotheim to suffer total eclipse? Because Johannes Müller turned the light of his genius on the Palmier marin, shall we forget its first admirable description by an equally great genius, Jean Étienne Guettard?

F. A. Bather.

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IV

The Antiquity of the Deep-Sea Fish-Fauna

IT has long been generally admitted that among marine organisms the keenest struggle for existence and the most favourable conditions for the processes of evolution, are to be met with along the shore-line. Those out-of-date forms of life which can no longer compete with the vigorous shore-dwelling races, are compelled to retreat to the freshwaters on the one hand, or to the deep sea on the other. Among fishes the antique mud-fishes (Dipnoi) have thus survived only in the freshwaters of parts of Africa, Australia, and South America; the fringe-finned ganoids, so abundant in the seas and estuaries of Palaeozoic times, are now confined to African rivers; while the higher bony ganoids of Mesozoic age, related to *Amia* and *Lepidosteus*, seem to have become exterminated in the seas of the early Eocene, thence retreating to the freshwaters both of Europe and North America, and at present surviving only on the latter continent. Some of the primitive forms of the higher fishes—the so-called Teleostei—which first appear (so far as known) in the seas of the Cretaceous period, have also survived to the present day solely or chiefly in freshwaters; but many of these races have migrated instead to the dark and gloomy abyss of the deep ocean.

It is interesting to note that, so far as the present state of knowledge permits the expression of an opinion, there is no evidence in the geological record of the last-mentioned refuge before the latter part of the Cretaceous period. There may have been seas of great depth, and there may have been organisms living at the bottom of them; but, if so, none of these pre-Cretaceous sea-beds have hitherto come under the observation of geologists in any part of the world. The chalk and associated strata, however, must have been deposited at a depth sufficiently great to accommodate a fauna of an essentially deep-sea type. It is thus to these formations that we turn for possible forerunners of the inhabitants of the depths of the sea at the present day.

It is, of course, difficult to decide from the fossils of any deep-water deposit, which of the organisms represented have fallen from the surface and which have actually lived at or near the bottom. Moreover, it is quite likely that some of the animals, which inhabit great depths at present, lived in shallower waters so long ago as

Cretaceous times, and have descended more recently. Among fishes, however, it is usually possible to distinguish the deep-water forms by their comparatively delicate skeleton, or by their attenuated fin-rays, or by indications of a great development of the slime-canal system which is related to the production of luminosity or even to the formation of special luminous organs. Judged by these criteria, the majority of the deep-sea fishes of the Cretaceous period are more or less closely related to the Scopeloids and Berycoids, which still form so conspicuous an element in the abyssal fauna. They are best known thus far from the Chalk of England, and from equivalent deposits in Westphalia (4), Bohemia, Dalmatia, and the Lebanon (1).

Some of the Cretaceous Scopeloids with a delicate skeleton—such as the so-called *Sardinius* and *Sardinioides* from Westphalia (4)—can scarcely be distinguished from existing deep-sea members of the family Scopelidae. Others, however, clearly belong to extinct groups, and among the latter may be particularly mentioned the long and slender fishes of the family Dercetidae or Hoplopleuridae. The little arrow-head-shaped, spiny scutes of *Dercetis* are common in the English Chalk, and nearly complete skeletons of the fishes to which they belong are found both in Westphalia and the Lebanon (1). The head and trunk are almost eel-shaped, but the skin is armoured with longitudinal series of the small spiny scutes. Both pairs of fins are present; and the median fins are divided into three, namely, one comparatively extensive dorsal, a slightly forked caudal, and a small anal. The vertebral bodies are not solid, but merely constricted cylinders, and the notochord must have been originally continuous through them. The soft parts, of course, are never preserved; but one specimen of *Dercetis* from the Lebanon, now in the British Museum (No. 49540), seems to prove that some of these fishes at least were provided with a distensible stomach—a feature so common among existing deep-sea fishes, but not hitherto demonstrated among extinct forms. The fish in question is preserved on a slab of limestone, and is shown of two-thirds the natural size in the accompanying photograph (Plate X). In the region which must have been occupied by the stomach, it exhibits a comparatively large and deep-bodied fish, with the head pointing backwards towards the tail of the *Dercetis*; and the dermal scutes of the latter are displaced in such a manner that there can be no doubt as to the deep-bodied fish having been swallowed head foremost. Judging from other specimens, the normal depth of this *Dercetis* at the back of the head would be only 10 millimetres, and below the dorsal fin only 13 millimetres; so that the swallowing of a fish 22 millimetres in depth necessitates considerable capability of distention.

Besides being related to the Scopeloids, the Dercetidae of the

SLAB OF CRETACEOUS LIMESTONE FROM SAHEL ALMA, MOUNT LEBANON, SHOWING A FOSSIL FISH (*DERCETIS*) WITH ANOTHER FISH IN ITS DISTENDED STOMACH.

(Two-thirds natural size.)

Cretaceous are also probably somewhat allied to the strange eel-shaped fishes of the family Halosauridae, which are a characteristic feature of the surviving deep-sea fish-fauna. These fishes have a very long tapering tail, with no separate caudal fin, but their dorsal fin is short in the middle of the back, and both the pectoral and pelvic paired fins are present. The body is covered with scales, and those of the lateral line, displaced close to the ventral border, are enlarged to bear a series of luminous organs. It is interesting to note that these unique fishes have also been identified quite lately (6) in the Cretaceous of Westphalia; the so-called *Echidnocephalus* of the latter formation being essentially identical with the surviving *Halosaurus*, and displaying even the enlarged series of scales observed in the latter.

As to the fishes most closely related to *Elops* and the herrings, there are not many in Cretaceous deposits which can be claimed as truly deep-sea forms. Of these, indeed, there is only one of much importance—*Istieus* from Westphalia (4) and the Lebanon (1); but this genus is of the greatest interest, because there are no means at present of distinguishing it from the surviving *Bathythrissa* of the deep sea near Japan. The latter fish is known only by two specimens, one in the British Museum, the other in the Berlin Museum, which can hardly be dissected; and the fossil form, which is abundantly represented in Westphalia, is thus destined to afford the first information concerning the skeleton. It is a fish much like *Elops* in general character, but the dorsal fin is considerably extended and fringes the greater part of the back.

With regard to the Berycoids of the Cretaceous, there is nothing worthy of remark, except that very few of the so-called *Beryx* truly belong to that existing genus. As at the present day, some genera have the aspect of deep-sea forms, while others are as clearly adapted for a shallow-water or surface habitat.

The most striking difference between the deep-sea fish-fauna of the Cretaceous period and that of the existing ocean consists, indeed, in the preponderance of physostomous species in the former and in the complete absence of eels and the Anacanthini—the cod-fishes and their allies. At the present day, according to Dr Gunther, the species of Scopeloids form about one quarter of the total fauna, while the Anacanthini constitute another quarter, and the strange forms of eels are an important element. In the Cretaceous fauna, it is scarcely too much to say that (so far as known) the Scopeloids comprise half the species, the other Physostomous fishes at least another quarter, and the Berycoids and their allies the remainder. The only traces of eels hitherto discovered in the Cretaceous are a few specimens of quite normal shallow-water types in the Lebanon (1); and no fishes of this remote date from any part of the world have hitherto been plausibly interpreted as the forerunners of the Anacanthini.

The next deep-sea fish-fauna after that of the Cretaceous occurs in the black hardened shales or slates of the Canton Glarus, Switzerland, which are commonly assigned to the latter part of the Eocene period (5). Here the higher spiny-finned fishes are already represented by such specialised forms as the ribbon-shaped *Lepidopus*; and there are also true cod-fishes of at least one genus named *Nemopteryx*. There are still no deep-sea eels, so far as known; nor are there any traces of the degenerate angler-fishes related to *Lophius*, such as characterise some of the greatest depths at the present day.

The general conclusion to be derived from palaeontology, therefore, is that the tenanting of the deep sea with fishes has been a gradual process, beginning at latest in early Cretaceous times and gradually proceeding until the present day. As new types have arisen successively in the shallower waters, a few have been driven from the regions of greatest competition to the refuge of the inhospitable depths. At the time when physostomous fishes were the dominant type, the refugees were chiefly Scopeloids, allied families, and primitive relatives of the herrings; in the early Tertiary period, when Anacanthini first appeared in this part of the world, a few cod-fishes were added to the abyssal tribes; while at some later but undetermined period the strangest of all deep-sea fishes—the highly modified eels—must have completed the remarkable fauna as we now know it. A. SMITH WOODWARD.

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V

A New Scheme of Geological Arrangement and Nomenclature

Part IV

IN my previous paper I ventured to criticise the current nomenclature and arrangement of the later Tertiary beds as inconsequent and misleading. I will now proceed to a more difficult and hazardous duty—namely, that of proposing a scheme of my own.

It will be understood that at present we are dealing with the latest beds only, inclusive of those now being deposited, that our geographical horizon is limited to the Archipelago of the British Isles and the surrounding seas, and that, consistently with the views I have previously maintained, I propose to treat the land and its fresh waters in one category and the seas in another.

In accordance with the views of some other students, and notably those recently propounded by Dr Hicks, I have always maintained that if we start with to-day and march backwards in surveying the land surfaces of those islands, we shall not arrive at a really substantial halting-place in our geological journey until we reach the base of the so-called Forest Bed. From the base of the Forest Bed until the very latest deposits of silt and mud, of blown sand, of humus or of bog, there has been one continuous stream of unbroken animal and vegetable life in these islands. During this long period some animals and plants have disappeared and become extinct, others have been introduced, and the general facies of the animal and vegetable life has changed considerably, but this has been for the most part a gradual and gentle process. It has not involved a general destruction of all life here, as the older theologians thought, nor the equally fantastic notion of the migration of its animals and plants *en masse* goodness knows how and goodness knows whither, and their re-migration back again in the same condition after a long geological interval, as the wilder Glacial men have urged. The evidence seems to me to be complete and very decisive on this point, and I am very glad it was so strongly emphasised by the President of the Geological Society in his recent address.

Secondly, when we travel beyond the Forest Bed it seems to me as plain as can be that we have a real gap and a real hiatus that points in these latitudes, whatever may have been the case elsewhere, to some very important change in the fauna and the flora of the land. For Britain, therefore, whatever may be the case elsewhere, we must claim that a very natural and logical geological epoch or period is constituted by the series of beds, commencing with the base of the Forest Bed and coming down to the handiwork of tide and river, rain and frost at this moment, and that this forms a very natural substantive and independent division, or series of our beds, marking a very distinct period, and that it marks, in fact, the very last one of all in the geological series.

What name should this period bear? As we have seen, the various names now current have the double infirmity that their meaning and connotation have become utterly confused and worthless, and that they have been applied indifferently to submarine and subaerial beds, which we wish to treat apart. If we are to select a name, it ought to be a name connected with the land and its products, and it seems to me that those names are the best which are taken from some type of animal or plant which marks the particular series both in time and in place. I am not sure that the term Anthropozoic or 'Human period' has not much to recommend it.

I, of course, object, as I have said before, altogether to making the mere advent of man a special reason for creating a new geological horizon, as Lyell and others have done, but it happens that this particular series of beds which (in Britain at all events) has a complete *solidarité* of its own, is also marked by the presence of man and his works all through, and that man is in effect a very good type animal by which to ear-mark the series. If any one can suggest a better name, let us have it by all means. What it is important to secure is the thing itself—viz., a natural and logical nexus of Beds. Names are after all only indices of knowledge, and the best name is that which best discriminates and defines the fact, and which connotes the idea we want to fix with the greatest precision.

Having suggested a generic name for them, let us next try and consider how we are to divide the long series of deposits in question into sections or groups, and let us try to secure some criterion which is not indefinite, but one which marks real changes and real frontier lines.

Apart from the shifting and unstable criteria which have led to the confusion in the commonly accepted nomenclature and arrangement of the later Tertiary beds, there is an underlying difficulty of more critical importance—namely, the actual divergence of opinion in explaining the facts. This divergence is very remarkable in regard to some facts which are really very elementary, and whose correct interpretation one would suppose was a matter of no great difficulty. But here, as elsewhere in English geology, we are met with an awkward difficulty. An individual geologist who makes a mistake of diagnosis can be easily pushed aside. If he does not choose to perform the happy dispatch, and to correct his own error himself, he and his craze can be jumped upon, and he speedily passes into oblivion. It is not so if the mistake is made by an official geologist. In that case it becomes the foundation of a school creed. Not only have Tom, Dick, and Harry, who come after the original sinner—like the French Staff in the Dreyfus trial—to write memoirs on corresponding phenomena elsewhere, to rub down their eccentricities to one level of official orthodoxy, but they have also to remember that the office and its staff has been already committed to an opinion which must be upheld. Every member of the staff is an Athanasius in *esse* or *posse*. Athanasius having spoken, the Council of Nicæa must uphold his view! and the subsequent memoirs issued under official sanction have to be coloured by this view until some irresistible earthquake arises, like the birth of Dr Hicks or Professor Lapworth, for instance, when the whole set of memoirs on the particular issue go tumbling down together. This sometimes takes a long time. The individual student feels that if his work is to live at all, he must not only work in the field but in the library also. He must not only know what he has done himself, but what other people have done, and he would consider it a piece of impertinence, when writing upon a subject in which others have taken a different view, to pass by that view without notice or criticism, and to write scores of pages of controversial matter with real or affected ignorance of what has been done elsewhere. He cannot take up the position that the only good geological wine is that contained in official bottles and that the official seal is as conclusive as veuve Clicquot's corks.

This is a very fine position to take up, so long as the public appealed to is that which always requires an official seal for its information, and is content with no other test, but a good many people are not made that way, and among them are, perhaps, some readers of this Review, who see perfectly well that the particular policy in question must necessarily have its Nemesis.

To come down to concrete issues, it seems to some of us that the really important horizon to fix, if we are to divide the Anthropozoic period as above defined, is the real position of the Drift beds; the beds once known as Diluvium, and now very often spoken of as Glacial by a school which was some time ago a great deal more cocksure than it is now. Lyell's term drift is a neutral one. Do the drift beds lie over or under the beds in which the Mammoth occurs, and in which so-called palaeolithic man has left his traces? Upon this issue a great deal has been written which does not seem to have penetrated official ears, nor, perhaps, the conservative atmosphere of Burlington House. Now that the subject has been made the special matter of an annual address from the president of the Geological Society, perhaps it will receive a little more attention.

To some of us who have diligently collected and published the evidence for many years past, and I myself have been allowed to do so at great length in the *Geological Magazine*, thanks to the ever-green benevolence of my old friend Dr Woodward, there remains little or no doubt that in every case where the evidence is clear, in the Old World, and probably in the New, the Drift beds plainly and distinctly overlie the so-called Mammoth beds. This was the view of Falconer and John Phillips. It is the view of Mr James Geikie and Dr Hicks, with neither of whom do I agree on all matters, but who on this one have done yeoman service in illustrating the question. We have not been content with mere rhetoric; we have shown in detail and by an immense concurrence of facts that this is the case, and that when the facts appear to lead to a different conclusion, the bones or other *débris* enclosed in the drift are as much boulders in it as the marine shells have been shown to be and as the travelled stones are.

No attempt has been made to seriously answer the case thus made out, but with the persistent monotony of that erratic bird the cuckoo, we have had on the other side one echoed and re-echoed assertion in the form of an *obiter dictum*. If there was ever an instance in which the burden of proof was upon the other side, it is this one. It is not surprising, therefore, if there has been some resentment about it and some impatience, and if in my hearing some phrases have been used about it, which had better be left to the imagination.

The views which have prevailed about the true position of the Mammoth beds in regard to the Drift are virtually three. One view has been that the Mammoth lived entirely after the distribution of the Drift. This view is, I believe, extinct. The evidence is too overwhelming that in numberless places the remains have been found in undisturbed beds below the drift for this view to be any longer tenable. A considerable number of people still maintain, however, that the Mammoth both preceded and outlived the Drift phenomena, having thus lived in two epochs, whence the name Dicyclotherium given to the beast by Geoffroy St Hilaire. I have tried with all the patience and care I could to examine the various finds of Mammoth remains in the northern hemisphere where there is any evidence of succession, and I claim to have shown that in no instance, so far as we know, does the Drift actually underlie any land surface containing the remains of the Mammoth and of its contemporaries (*Geol. Mag.* for 1892-1893 *passim*).

No doubt Mammoth's teeth and bones, and the remains of its companions, have been found in boulder clay and in pockets intercalated in that clay; so have ammonites and belemnites and bones of Liassic saurians of coal plants, of Triassic shells of Carboniferous corals, etc., etc. But in all these cases the remains in question are adventitious, and have been collected by the force, whatever it was, that distributed the clay, and they occur in the Drift beds as boulders, just as certain broken trunks of trees do. To call the remains of the Mammoth when so found interglacial, and to refuse the same style to the ammonites and belemnites, is inconsequent. Nay, more, it would be almost incredible if some of those massive teeth and bones had not occurred as boulders in the drift, since they must have been lying about on the old land surface over which the drift was deposited.

The fact is that there would not have been any question raised about the matter in these later days with the present knowledge we possess if it had not been that a certain number (a rapidly diminishing number, if I am to judge by the conversations and correspondence I have had lately) of geologists are committed to the purely *a priori* theory of interglacial periods, which theory again was the product of the now entirely discredited astronomical theory of an ice age. It is the desperate straits to which the champions of interglacial periods are driven that makes them cling to these boulder-bones and teeth as evidences of so-called interglacial beds. And this clinging is emphasised by the fact that more inconsequent, unscientific, and childish nonsense seems to me to have been written on so-called interglacial beds in the memoirs of the Geological Survey dealing with the surface deposits than upon any other subject. The term is used over and over again in these memoirs without any attempt to justify it, as if there was any general concurrence of opinion to support it, whereas if the geologists of the world were polled, the number of believers in what is a mere hypothesis, eventually based upon Croll's now exploded astronomical theory of an ice age, would be found to be a mere handful. Surely we have a right to complain that in Government publications, paid for by the taxpayer, and carrying the authority of official documents, an hypothesis so generally discredited and so generally discarded should be so calmly assumed as if it had the authority of an axiom.

No one objects to the theory of interglacial periods, or of the possibility of squaring the circle, or of extracting sunbeams from cucumbers being defended in private and individual memoirs like

"Gulliver's Travels." What we object to is these fallacies being published as the State geological religion.

It is really too amusing how far the matter is carried. At a recent meeting of the Geological Society a paper was read upon the never-ending problem of the relation of the Drift to the Mammoth beds in the caves of North Wales. It was shown by the author of that paper, Mr Pollen, very conclusively that the Drift overlies the beds contemporary with the Mammoth in the particular case in question. Therefore my friend, Mr C. Reid, who has done an immense amount of careful and very valuable observation, to which I personally am under great obligations, but who is an arch-sinner in regard to the championship of transcendental theories, at once suggested that the beds in question were interglacial. "Interglacial" to most people means "placed between two glacial beds," but this is not its meaning apparently when used by Mr Reid. To him a bed becomes interglacial if it is merely covered by Drift. It was a curious epilogue to the discussion in question that Mr Strahan, who has surveyed and mapped the surface beds of North Wales with singular care and skill, should have got up and declared very positively that there is not the slightest trace of interglacial beds in North Wales at all.

I must be allowed to quote another amusing example. So-called palaeolithic man, as is well known, was the contemporary of the Mammoth. But for some reason a certain number of writers committed themselves long ago to the idea that palaeolithic man did not live before the distribution of the Drift, but after it. The evidence drawn from the Mammoth beds, etc., however, was getting very awkward, and pointed very definitely the other way. The British Association was accordingly invited to make a grant to test by an excavation the famous typical site at Hoxne, where the evidence had been read in different ways by different explorers. Some of us gladly welcomed the appointment of a committee, but were taken very much aback when we found that the only men put upon it were those already committed (may I say violently committed) to the view that the implement-bearing deposit at Hoxne was newer than the Drift. This was not very promising, nor was it quite judicial. It was, in fact, indecent. When the report came out our suspicions were more than justified, for a more extraordinary discussion and summing up of a serious polemical subject was perhaps never seen.

This is not the occasion to discuss the Report in detail. That may be done on another occasion. At present I would merely refer to one point (perhaps the strongest argument used) as a sample of the kind of arguments used. It is well known that in East Anglia the crag beds occur in more than one form, and in most places in the form of barren red or yellow sands. When these sands occur in a somewhat disturbed or dishevelled form, they have been called "Middle Sands," and have been associated with the Drift. Why, I know not, for they are essentially crag sands, and Mr Horace Woodward has shown how difficult or impossible it is to separate them from true crag. When the principal pit was dug at Hoxne the other day, the supposed equivalent of the beds containing palaeolithic implements was found to be underlaid, not by boulder clay, either brown or chalky, or by any deposit containing foreign erratics, but by a bed of barren sand (unmistakably, as it seems to me, for I know the neighbourhood) belonging to the crag series. It is curious, notwithstanding all this, that the bed of sand in question has been labelled Glacial Sand on the diagram attached to the report! And in this way the palaeolithic bed is made out to be inter-glacial or post-glacial.

I shall not pursue this matter further, but content myself by reaffirming that the very numerous facts quoted by Dr Hicks and myself and others, prove that the Mammoth bed or beds, with palaeolithic implements in them, in all cases where they can be properly tested, underlie the Drift beds. Those who say they don't had better answer our facts and arguments instead of shouting cuckoo to each other.

The next point upon which I entertain absolutely heretical views is as to the origin of these beds. In my view they have been distributed by water and not by ice. Upon this issue I have printed two considerable volumes, and many papers in the *Geological Magazine*, and every day strengthens me in my opinion. I further hold and think that I have proved that of the contents of these beds the greater part are derivative. This is the case with the rounded boulders, with the rounded and smoothed pebbles in the gravels, and with the sands and the older clays incorporated with them. It is so, as Mr Horace Woodward has shown, and as is now admitted by Mr C. Reid, in regard to the marine shells which were formerly styled glacial, and it is so with the logs of wood, the deer's antlers, and the molar teeth, and great limb-bones of the Mammoth and other animals found with them. All the contents of the beds, so far as we can make out, are adventitious and derivative. This I have maintained at great length elsewhere.

Secondly, I also maintain that these beds, instead of representing a long period with manifold changes, glacial or otherwise, represent a transient and rapid diluvial movement. In the great kames of the Lancashire valleys as on the cliffs at Cromer, the laminae of sand and finely levigated clays are arranged in immense curves reaching sometimes from the base to the summit of the beds. These curves are unbroken and continuous, and are therefore clearly the product of one impulse and not of a series lasting many ages. The unworn character of the teeth and bones and shells, all derived and found at different horizons in the beds, are also inconsistent with anything but a single and violent impulse. For these and other reasons which I have elaborated at length elsewhere, I must conclude, as I have said, that these beds do not represent any prolonged period, but a transient movement, a transient movement coincident with the breach in continuity between England and the Continent.

These last conclusions are, I know, far from being shared by the geological prophets of these later days. They were the views, however, of my masters—Murchison and Sedgwick, Conybeare, and Whewell, Hopkins and Phillips. Where are better men to be found now? Where, in fact, are such men to be found at all now? They nearly all wrote, be it remembered, after the glacial craze had been published, and all repudiated it. The views in question will, I am convinced, again become the views of another generation which is rapidly abandoning the extravagant teaching of Agassiz and his scholars.

Whether true or false, however, they do not affect the earlier part of this argument, which is supported by a large majority of the geologists of the world—namely, the fact of the priority of the Mammoth beds to the Drift. The Drift, in either case, forms a great "divide," a great frontier, between two main divisions of the Anthropozoic period.

To myself it separates an antediluvian from a post-diluvian period. These terms, however, are ambiguous, since they might be accepted as in some way associated with the ideas of the theological geologists with their Noachian or universal floods, an idea far from any views of mine, and I would therefore apply some other nomenclature to them. I dislike the words palaeolithic and neolithic as geological terms whatever virtue they may possess in archaeology. The former use of stone instead of metal by man was an archaeological accident. Nor does the term neolithic include any but a fraction of the beds which overlie the drifts.

I prefer to differentiate these beds by their animal contents, which are much safer criteria than the different shapes of chipped axes, whose types sometimes overlap. The great cardinal difference of the animal life before the drift and that subsequent to the drift is the presence of domesticated animals in the latter and the presence of wild animals only in the former, and by this test I would separate them, calling the former *Theriozoic* and the latter *Himerozoic*, the one connoting that division of the Human period in which wild animals alone lived with man, and the other the corresponding period in which tame ones did so.

Let us proceed again. The Theriozoic or praedrift beds were, so far as we know, continuous with each other, and if the geological record was intact in these realms the evidence would show it, but unfortunately the debris of the land fauna and flora at all periods, save one, are broken and fragmentary and scattered. The one exception is that of the fauna contemporary with the Mammoth, which abounds in enormous quantities in all parts of the world. In fact the contrast between this poverty in other beds and the abounding richness in the Mammoth beds is one of my strongest arguments in favour of the catastrophic destruction of the latter fauna, as compared with the slow and gradual decay of its predecessors. This slow and gradual decay, in which bones and shells, instead of being rapidly covered up, as they were in the case of the Mammoth and its companions, were exposed to rain and snow, and faded and weathered away, have left us only shreds of the former life in certain fortunate localities, and there is apparently a considerable gap in our evidence, which compels us to separate the Theriozoic beds into two series,—one of them known as the Forest Bed, occurring in a very limited locality near Cromer in Norfolk, and represented also by some scanty remains in Dorsetshire; the other represented by the contents of the older caverns, the brick-earths of the Thames valley, etc., and generally by the remains of the old land surface immediately underlying the Drift. The former is marked by certain mammals which do not occur in the latter, and which had been probably driven away by increasing cold or otherwise, and had at all events become extinct in this country; and the latter marked by certain animals which had apparently migrated hither in consequence of the increasing cold. Fresh discoveries, however, yearly add to the list of mammals once thought to specially mark the later beds, and which are now found to have existed also in the earlier ones; and in view of possible discoveries in future I should be disposed to simply discriminate the two sets of beds by the words older and younger,—the former especially marked by the presence of *Elephas meridionalis, Rhinoceros etruscus, Ursus arvernensis,* *Trogontherium cuvieri,* and by a series of deer which in my view have not been yet quite sufficiently studied, but which were apparently limited to this horizon. The latter was perhaps best marked by the introduction of certain northern beasts, such as the Reindeer, etc., and by certain Steppe animals like the Saiga antelope.

Turning to the *Himerozoic* beds, those commencing with so-called neolithic times and coming down to to-day, they have sometimes been classified as prehistoric and historic. I cannot attach any meaning to these terms which is admissible in a scientific classification. We have long ago got past the stage of looking upon history as necessarily based upon written documents. A great deal of all history is in fact archaeology, and it matters little in reconstructing the story of a people whether we piece together our description of their appearance, surroundings, habits, etc., from their art remains or their written records. I don't know where in fact to draw any rigid line between what is prehistoric and what is historic. Nor do I think the introduction of writing can be accepted as marking a geological hoiizon. On the other hand, I confess, however paradoxical it may look at first sight, that I know of no better horizon where to divide the beds in question than the invasion of the Romans. This, I know, does not sound like a geological test, but it really has a certain geological meaning. It was the Romans who first began to import into our fauna and flora a great many of our non-indigenous animals and plants, our fruit trees, and also some of our best known forest trees, our garden plants, and perhaps some of our best known strains of domesticated animals, and if we are to choose a dividing line on biological grounds, I know of none better than this one. To sum up, then, this attempt to arrange the subaerial beds of England, which intervene between the base of the Forest Bed and to-day, I would condense the result in the following table:—

- Anthropozoic Beds—
  - —Theriozoic Beds—
    - —Older or Forest Bed.
    - —Younger or Brick Earths and Caves.
  - —The Drift
  - —Himerozoic Beds—
    - —Pre-Roman.
    - —Post-Roman.

I do not consider that this arrangement and nomenclature in any way interfere with the arrangement and nomenclature of the later beds prevailing in the text books, since it is entirely confined to the subaerial beds which have hitherto been mixed up and confused with the submarine beds whose arrangement ought to be dependent on entirely different considerations. To these latter alone the terms Pliocene, older and younger Pliocene, Pleistocene, etc., were originally applied; and if these terms have any virtue to them they ought to be limited, and I propose to consider them and to fill up some gaps in the present paper on another occasion. In conclusion, it is necessary that I should apologise for the too frequent use of the first personal pronoun in the foregoing pages. It is the inevitable consequence of polemics of this kind. I feel sure it does not mean that I am more cocky and self-asserting and impertinent than the friends I love to criticise, and whose criticisms are so welcome to me. If I did not greatly value their work, mistaken as I deem some of it to be, I should not be creeping away from the House of Commons, at the risk of being abused by the Whips, in order to write this paper.

HENRY H. HOWORTH.

SOME NEW BOOKS

PROTOPLASM

THE LIVING SUBSTANCE AS SUCH: AND AS ORGANISM. By Gwendolen Foulke Andrews (Mrs Ethan Allen Andrews). Supplement to the *Journal of Morphology*, Vol. xii., No. 2. Boston: Ginn & Co., 1897.

THIS attractive and well printed volume departs from a recognised tradition in an unusual fashion. It contains an intolerable deal of sack but a very excellent pennyworth of bread. Let us be rid of the sack first. The language of the volume is singularly repellent: it is not British English, nor American English nor Germanised English, nor in fact any of the recognised varieties of English adopted by scientific writers: it is a curiously inflated and lyrical jargon occasionally rising into a pretty but misplaced rhetorical eloquence, but frequently taking revolutionary views of grammatical construction. The treatment resembles the style: it is diffuse and pompous; there is no coherent argument, and a mania for irrelevance and repetition. The good lady insists upon magnifying the application of her observations: from her investigation of the minute structure of protoplasm and of the filose pseudopodia of Gromia she passes gaily to theoretical conclusions concerning habit and heredity and natural selection. To choose a sample of the book, the old method of divination has been followed exactly; a hat-pin was stuck between the leaves and its point indicated the following passage:—"Fosterhood. (146) The word parasitic having by a certain frequent use associations which in some connections are jarring, it should suffer as idea a transformation of verbal form gracious enough to follow that of the fact into the beautiful phenomena of parenthood. Dependent perpetuation areas may be called fosterling areas; the supporting substance foster substance, area, organ, or organism. Of all areas differentiated to live at expense of other parts of an organism, there are none so grossly egotistic, none which so take all and keep all for themselves as the perpetuation or fosterling areas. From their inception, for variable periods, often covering the whole term of their existence, they receive largely from the foster-substance in many of its phases." For the life of him, the present reviewer, who has enjoyed the advantage, likely (he fears) to be unusual, of reading this passage, not only as isolated but as part of the whole volume, cannot see that this means more than that if a protoplastic area is fed from another protoplastic area it receives its food from that other area. The same kind of criticism may be applied to very large portions of the volume: there may, of course, be deeper meanings and a wealth of veiled conclusions, but the bewildering infelicity of the style has obscured them from at least one willing mind.

None the less it is certain that Mrs Andrews had valuable and novel things to say. She has spent long years on the patient study of living protoplasm: she has compared the kaleidoscopic changes in living cells and nuclei with the arrested, possibly distorted, phases of them seen in fixed and stained preparations. Apparently she has unusual keenness of vision and patience, and has thought no trouble too much did it secure a glimpse into the life of living matter. Had she drawn a great deal more and written a great deal less, knowledge were more surely her debtor. As it is, she has secured many valuable observations.

In his main idea, Bütschli has found corroboration from Mrs Andrews. Protoplasm is a froth or foam or emulsion. It consists of closely packed bubbles, the contents of which are different from their walls. Mrs Andrews, however, believes that the vesicular structure extends far beyond the limits of Bütschli's observations, and that the visible bubbles grade into series so minute as to appear homogeneous even under the highest possible magnification. Moreover, while Bütschli was inclined to attach particular importance to the contents of bubbles, and to regard many of the phenomena of protoplasm as the result of mechanical and chemical relations between the bubble contents and surrounding fluids, Mrs Andrews is more of a vitalist and regards the stuff of the bubble films as the real protoplasm, the active living agent. She describes a large number of cases in which changes of shape, visible under the microscope, cannot be attributed solely or even chiefly, after the manner of Bütschli, to chemical changes between the contents of the bubbles and the surrounding media. Those who have watched Bütschli's artificial foams know that when they are brought into a medium different in chemical constitution from the contents of the bubbles, osmotic changes are at once set up and these result in curious pseudopodia-like protrusions from the surface of the foam, in streaming movements within its mass and even in movements of translation of the whole mass. Many of these changes are exceedingly similar in appearance to the movements of real protoplasm, and there have been many who have gone beyond Bütschli and roundly declared the causes of the movements of life to be similar in kind to these results of diffusion currents. Mrs Andrews, however, describes many cases in which the conspicuous feature of vital movements was actual shifting in position of the continuous substance which forms the walls of the bubbles. She describes that as flowing from place to place, now thickening the walls of the bubbles in one region, now withdrawing itself into invisible tenuity. Moreover, she shows reason to believe that this continuous substance itself may have a foam structure far minuter than anything Bütschli has described. Upon Bütschli's basis, she has increased for us the complexity of the conception of protoplasm, and has shown that a large part of living movement may be the result of chemical differences and emulsion currents within the protoplasm.

Mrs Andrews has made and recorded a specially interesting set of observations on the outer surface of living cells. She declares that in most cases the living matter extends in an attenuated form far beyond what is usually figured and described as its limit. The flickering edge of a cell is usually a net-work of delicate protrusions, ceaselessly changing their position and form. These are in a state of most active sensibility, and the slightest mechanical or chemical stimulus causes an instantaneous change on them. Moreover, the interior of living protoplasm is in a constant state of assuming local differentiations which are as ephemeral and transitory as the movements of the outer edge. The authoress insists, with reason, that not sufficient attention has been paid to the changes that killing and staining agents must produce upon so sensitive a mass. They must act first as drugs, however rapid their action may be, and the preparations of microscopists may be chiefly pathological conditions.

We hope that Mrs Andrews may be induced to publish figures of a number of the interesting observations that she has made. If structure can be described, it is possible to figure it, and much that is at present obscure in her text might become of great use to other workers. A sentence from the 'conclusion' will explain shortly the general attitude of the authoress to protoplasm. "The facts seem to warrant present belief that the living substance of all organisms is one physiologically continuous, living, plasma, homogeneous throughout in its living powers and properties, but having varied local and temporary habits of self-expression, which are largely and inextricably correlated with physical and chemical conditionings of its form and composition as complex emulsive foam—yet not to be wholly identified with or explained by these."

Allies of the Worms

Traité de Zoologie Concrète, V. Les Vermidiens. By Y. Delage and E. Hérouard. 8vo, pp. xii. 372 and 46 coloured plates. Paris: Schleicher Frères, 1897. Price 25 francs.

We welcome this volume of the "Zoologie Concrète," which, although the second to appear, is really the fifth of the great work undertaken by Messrs Delage and Hérouard. The first volume was published last year, and reviewed in *Natural Science*, p. 200, vol. xi. Under the title of "Les Vermidiens" the authors now deal with a number of interesting groups of Invertebrata, at one time or another considered to be more or less closely related to the Annelids.

First of all the Gephyrea are described, then the Bryozoa. *Phoronis*, *Rhabdopleura*, and *Cephalodiscus* follow, forming a new class, the Axobranchia. The Rotifera and Gastrotricha come next as the class Trochelmia; the isolated genus Echinoderes is placed in a separate class, the Kinorhynchia. Finally, we have the Chaetognatha and Brachiopoda.

We need not again discuss in detail the very original plan adopted in describing these forms—it is essentially the same as in the previous volume. After the anatomy, physiology, and development of the theoretical "type morphologique" of each class has been given, and the types of the subclasses, orders, etc., have been as far as necessary similarly treated, a description accompanied by figures is given of the chief characters of every known genus. Some idea of the completeness of the work may be gathered from the fact that the whole volume contains nearly 400 pages, illustrated by 523 figures in the text, and 46 coloured plates. The Rotifers alone occupy some 40 pages, 10 plates, and 60 figures in the text.

Near the end of the volume are general discussions on the characters and affinities of the Vermidea, a review of the range of variation in the anatomy and physiology of the group, and some useful synoptic tables of the classification. Finally, there are a bibliography, an index to technical terms, a list of the hosts of the parasite forms, and an index to the genera described in the volume.

Throughout the book the style is fresh and interesting, the descriptions lucid, and, considering the immense amount of detail dealt with, very accurate. It may be pointed out, however, that the "tentacles" of Sipunculus do not form a simple circle round the mouth as described and figured on pages 12 and 16. They are the lobes of a horse-shoe-shaped fold—probably a lophophore in fact—a matter of some importance. The description of Sternaspis is neither as accurate nor as up-to-date as we should like to see it. Considerable vagueness prevails with regard to the nomenclature of the excretory organs and genital funnels. Whatever may be the authors' opinion concerning the homology of these organs (and we hope it will be much more clearly expressed in subsequent volumes), surely the term "organes" and "entonnoirs *segmentaires*" is singularly inappropriate when applied to the nephridia or genital funnels of such unsegmented animals as the Rotifera and Polyzoa. On the whole, the descriptive part of this volume seems to us excellent, and bears witness to the great industry and ingenuity of the authors. Particularly clear and well illustrated are the accounts of such complicated processes as the budding in the Bryozoa and the development of the Ectoprocta.

Let us now turn from the purely descriptive to the more general parts of the volume, those parts which deal with the classification and affinities of the animals under consideration. Here Messrs Delage and Hérouard seem to us to have been much less fortunate. In the class Bryozoa, for instance, the authors still include the Entoprocta, although they are naturally enough unable to construct a real "type morphologique" common to the Entoprocta and the Ectoprocta or Bryozoa proper. It is difficult indeed to see how with any amount of ingenuity they could possibly synthesize these two groups which differ in every essential of their structure. Whilst in the typical Ectoprocta, as is well known, a lophophore bearing hollow tentacles surrounds the mouth, there are no nephridia, and the coelom forms an extensive body-cavity communicating with the exterior by genital funnels; in the Entoprocta we find a ring of solid tentacles encircling the body so as to surround both mouth and anus, a substantial parenchyma in which lie a pair of true flame-cell nephridia, and the coelom is represented by a small pair of genital sacs opening by two funnels to the exterior. True, budding occurs in both groups, and in both the larvae have a more or less vague resemblance to a trochosphere; but the buds are of different nature, and what group of Invertebrate Coelomata is there the larva of which does not bear a certain likeness to a trochosphere? Moreover, recent researches have rendered very probable the view that, whilst the Ectoprocta are fixed on their ventral surface, *Pedicellina* lies, so to speak, on its back. In this case even the nervous ganglia of the two forms would not be homologous. Taking all these things into consideration, we fail to see what excuse can be found for placing the two groups under one name.

The class Gephyrea is another case in point. In spite of all the evidence which has been brought forward to show that *Sternaspis* is a highly modified Polychaete, probably nearly allied to the Chlorhaemidae, it is again placed with the Echiurids. Now the chief, if not only, real differences between the Chaetopods and Echiurids are the presence in the latter of a peculiar preoral proboscis, and of posterior organs with ciliated funnels—these are both absent in Sternaspis. The paired anterior processes described by Sluiter do not contain the cerebral region of the periœsophageal nerve ring, and appear to be comparable not to the proboscis of *Echiurus*, but to the palps of Polychaetes. The brain of *Sternaspis* is situated in a prostomium, in normal Polychaete fashion. Perhaps more striking still, in such a *fin de siècle* zoology as this, is the retention of the Echiurids and Sipunculids in one class. An attempt to build up a 'type morphologique' possessing the structure of both these forms can only result in the production of a caricature of an animal having the essential characters of neither.

With regard to the new group Vermidea, we were at first inclined to believe that the authors merely introduced the name as a convenient but loose term, somewhat as we use the word Invertebrata. Such, however, is not the case. Messrs Delage and Hérouard claim that the various animals they have brought together by a process of *épuration des vers* are really closely enough related to form a true group. Discarding the Molluscoida and the Podaxonia, and indeed paying little attention to the important work recently done on the subdivision of the coelom in these forms, they make the artificial assemblage of Gephyrea into a central group, having *des affinités réelles* on the one hand with the Annelids, and on the other with the Chordata. From the point of view of phylogeny, such a position seems to us quite untenable. The fallacy of the method may be illustrated by an exaggerated example. Suppose a systematist to be classifying the mammalia; what should we think of his system, if, having first united the Cetacea with the Sirenia and Pinnipedia, he then proceeded to argue that this new group has obvious and real affinities on the one hand with the Carnivora, and on the other possibly with the Ungulata! Yet this, it seems to us, is just the method pursued by the authors in the case of the Gephyrea and Vermidea. The process of *épuration*, or sifting, may be very good for the true Worms, which are not treated of in this volume; but if we pour all the rejected forms into one group, the Vermidea, the classification is left in a worse condition than before, the confusion is worse confounded.

If we have insisted somewhat at length on what seem to us defects in the taxonomy of this volume, it is because we believe no greater mistake can be made than to consider classification as a matter of secondary importance. If rightly understood, classification is not only a valuable summary of the knowledge acquired, but also a guide to farther progress. However clearly and accurately described, facts lose half their value and interest if not presented in their proper relations. When the Bryozoa were shown not to be Hydroids, when the Brachiopods were shown not to be Molluscs, when the Rotifers were shown not to be Infusorian Protozoa, when the Sipunculids were shown not to be Holothurians, real advances were made, not only in classification, but also in the knowledge of the morphology of those animals. The attempt to establish an obviously polyphyletic group like the Vermidea, containing such forms as *Sternaspis* and *Lingula*, Rotifers and *Phoronis*, *Echiurus* and *Cephalodiscus*, can only, we think, retard the progress of zoology, and confuse the points at issue. To the authors' exclamation that "*les Molluscoides ont vécu*,' it can only be answered that we hope the Vermidea, as a group, will never live.

Throughout the volume the numerous figures in the text are both new to text-books, and good; many of them are much improved by the use of colours. Quite a new feature, of course, is the introduction of plates with highly-finished coloured figures. Always instructive, and generally original, these plates are often excellent in conception and execution. We may note especially those dealing with the Bryozoa and Rotifera.

In this work Messrs Delage and Hérouard have successfully kept up the high standard of excellence set in their first volume on the Protozoa, and this notwithstanding the many and great difficulties they must have met. All teachers and advanced students of zoology will be grateful for such a reliable guide to the study of rather obscure groups of animals, generally somewhat neglected in text-books, and will look forward with eagerness to the appearance of the next volume of the *Zoologie Concrète*. E. S. GOODRICH.

AN AMERICAN TEXT-BOOK OF GEOLOGY

AN INTRODUCTION TO GEOLOGY. By William B. Scott. 8vo, pp. xxvii + 573, with 169 figs. and 12 special plates of fossils. New York: The Macmillan Co., 1897. Price 8s.

FROM the first glance there is a refreshing air of newness about this beautifully printed volume, giving a good impression which improves on further acquaintance with the work. It is copiously illustrated, not only with the usual diagrams, but also with exquisite photographs of scenery and special features of geological interest; and it is further embellished with twelve plates of typical American fossils, drawn by Mr Rudolph Weber in his well-known effective style. It is essentially an American book for American students; but the chapters on physical geology can, of course, be appropriately used for teaching purposes in any country, while those devoted to historical geology, though essentially from the American standpoint, are characterised by a breadth of view which is unusual in a geological handbook of a strictly elementary character.

Dr Scott has for many years held the Professorship of Geology and Palaeontology in the University of Princeton, New Jersey, and the arrangement of his book is the outcome of considerable experience as a teacher. After a few introductory remarks, there is a chapter on the rock-forming minerals. Dynamical geology follows, the igneous agencies being first treated, then the surface agencies, and the latter classified under the headings of destructive and reconstructive processes. Structural and physiographical geology form the subject of the next two sections; and historical geology occupies the final 200 pages of the book.

Dr Scott treats each period of geological time in a definite order. He begins by mentioning the origin of the name applied to it, and gives a tabular list of the American strata representing the period. He then proceeds to describe these rocks and the geographical conditions under which they must have been formed; and finally he refers in a delightfully broad manner to their foreign equivalents. The life of the period is then systematically enumerated, with occasional brief descriptions of specially important types; and there is the plate of sketches of common fossils to guide the student in his practical work.

Professed geologists and palaeontologists will naturally turn to Dr Scott's account of the Tertiary formations, to our knowledge of the mammalian fauna of which he has made so many important contributions. Here they will not be disappointed; for, although the chapter is necessarily very brief, it gives in an authoritative manner just such a broad view of the subject as an ordinary student of geology or vertebrate zoology requires. Our only complaint is that the author should still make use of the misleading term 'Quaternary' for the Pleistocene period. He, himself, indeed mentions the fact that the transition between the Pliocene and Pleistocene deposits is perfectly gradual; it would, therefore, have been more logical to refer them to one and the same great Tertiary series. He is to be congratulated, however, on the brevity of his reference to the speculations on the Glacial epoch, which often occupy an undue space in elementary text-books; and his table of strata at the end is also commendably brief and free from bewildering detail.

Dr Scott has indeed produced an elementary text-book of geology of which the University of Princeton may feel proud. It is clearly the work of a teacher, an experienced original investigator, and of one whose knowledge is far beyond the once-common parochial stage.

THE GEOLOGY OF CAMBRIDGESHIRE

A HANDBOOK TO THE GEOLOGY OF CAMBRIDGESHIRE. [Cambridgeshire Natural Science Manuals.] By F. R. Cowper Reed. 8vo, pp. x+276. Cambridge: The University Press, 1897. Price, 7s. 6d.

CAMBRIDGE, fosterer of all sciences, mother of famous geologists, ought to give the world a model Handbook of local geology. What should be the aim of such a Handbook? Not, certainly, to include all that has been written, and so save a student from study; nor yet to be a mere description of the geological map, assigning each area to its stage. A more usual object is to catalogue the formations which occur, indicating the places where they may be examined, and noting the peculiarities which they present. A higher ideal would be to make a geological history of the district, describing its character as land and sea in each successive age: with this might well be combined remarks on the present visible surface, as resulting from its history and its structure. But such a book should not teach general principles, nor debate unsettled controversies. If the writer be great, he may hope to attract students to the science, but if he is wise he will not attempt to teach it to them. The author of this book seems to set before himself several of these aims, and to reach some of them. He, for the most part, follows the same lines as Woodward's

"Geology of England and Wales," but he hardly equals its self-control in adherence to a plan.

The book opens with a physical description of the county; not a very happy one. The Cam Valley breaches higher ground and carries the drainage of a basin into the wide plain of the Fen-land. When the author calls this a division into four parts, he sets before us separation instead of unity. He gives a good account of the relation of its water channels to its earth-structure, but why force upon his readers the most newly-invented jargon of technical terms? We object to obsequious or consequential persons, and to 'obsequent' or 'consequent' rivers.

The papers of Teall, Keeping, Roberts, W. Hill and Jukes-Browne, with the Survey memoirs, have added largely to our knowledge since the time of Dr Bonney's graphic sketch. The author makes good use of them, and his account of the Secondary formations seems in general good and full. He once or twice ventures on a little geological history, in describing the Lower Greensand as accumulated in a narrow strait flanked by Palaeozoic rocks on the east (the latter statement is given without the grounds for it), and again in connection with the Red Chalk. Generally, however, this highest form of description is entirely left alone. "The Portland and Purbeck Beds are wholly wanting and perhaps never existed in this part of England." "All the Tertiary beds are now absent." What was the condition of Cambridgeshire in these periods? land? shore? or still, pellucid sea? Silence is strictly maintained. Silence is also maintained as to the buried Palaeozoic floor. We know of no deep boring in Cambridgeshire; still, in the present interest about possible Coal-measures, some persons may consult this handbook for information, and consult it in vain.

The Pleistocene Deposits are treated at considerable length. Few who take ice in hand can keep cool heads; we have fifteen pages on the "Mode of formation of the Chalky Boulder Clay." The author gives a temperate account of the different theories, but what place have such theories in such a book? Why does he not also discuss Permanence of Ocean Basins in connection with the Chalk, or Formation of Coral Reefs when upon Upware? He gives just such a review of the theories as a cautious Professor would give to thirsters after certainty, but occasionally he strays into positivism. "The chalk floor beneath the clay does not show any striated or polished surface, owing to its softness and the removal of the original surface by subterranean denudation." He probably meant to write 'perhaps owing'; chalk fragments in the clay itself are abundantly striated. We should not have expected that more than one writer could be found who would divide the theories 'into two groups (i.) the glacial, and (ii.) the non-glacial'; this second group consisting of Sir Henry Howorth's alone. The only place for theories in such a handbook is where the district is providing important evidence which bears upon one or other of them. Thus the author rightly gives some space to the great transported boulder of the Roslyn pit. His account of the gravels seems good and full: probably some of it is original, but ought not a reader to be enabled to know? Should not the statements of a handbook be either matters of universal acceptance, or else opinions of named authors whose names give weight to (or, take it away from) their statements ?

An appendix contains a valuable list of books and papers bearing upon the county. The volume is printed as the Cambridge University Press can print. Errata are very few. It is a book likely to assist geologists, but not likely to create them. We have no right to expect the illumination of genius and we do not find it. Dr Bonney in the preface to his sketch of 'Cambridgeshire Geology,' hoped that it might be superseded by a Geology of the Valley of the Cam, to form a companion to that which Phillips executed for Oxford; and hoped that the present Professor would take it in hand. These hopes are not fulfilled.

A Student's Petrology

Petrology for Students. An Introduction to the Study of Rocks under the Microscope. By Alfred Harker, M.A., F.G.S. Second edition, pp. viii+834. 75 figures. Cambridge Natural Science Manuals. Cambridge: University Press, 1897. Price 7s. 6d.

The first edition of Mr Harker's "Petrology for Students" took rank as the most practical and best of the elementary English text-books on that science, and as perhaps the most useful students' book in the Cambridge "Natural Science Manuals." The present edition may be expected to be even more useful, for though only 27 pp. longer, it has been extensively revised and considerably improved. The references to foreign rocks especially have been largely increased, which in a general sketch of the subject is a step in the right direction. The author ingeniously overcomes the difficulty of dealing with many rock names, the value of which is doubtful, by not using them in the text, but quoting them in the index with a reference to the page where the rock is described. Thus, for instance, Monchiquite is given in the index with a reference to p. 143, where the rock is described as a type of ultra-basic lamprophyre, but the actual term is not used. The "yogoite" of Weed and Pirsson, the "grorudite" and "akarite" of Brogger, and the "litchfieldite" of Bayley, references to which are added in this edition, are all thus treated.

Another improvement is the adoption of the name "Hypabyssal" rocks instead of "Intrusive" rocks for the author's second group, in which are included the acid intrusives, porphyrites, diabases, and lamprophyres; for there can be no doubt that many of both plutonic and volcanic rocks are sometimes intrusive.

Northern Spain

In Northern Spain. By Hans Gadow. 8vo, pp. xvi+421. London: A. & C. Black, 1897. Price 21s.

In this entertaining volume Dr Gadow has combined a general account of two extended journeys through little known parts of northern Spain, with a valuable synopsis of his observations on the natural history of the country. He also makes special reference at times to practical matters affecting a traveller; so that his narrative will not only be read with interest by those who are unable to imitate his experiences, but will also prove of much value to those who contemplate similar wanderings.

The greater part of the work is occupied with the narrative, which is illustrated by many beautiful photographs and several outline sketches. Then follows a brief account of the dolmens or cromlechs of the province of Alava, with some sketches and plans, and this chapter concludes with a list of all the known dolmens and similar prehistoric remains in Spain and Portugal. The next 40 pages are devoted to a condensed account of the history of Spain, with special reference to the northern provinces. The final two chapters, occupying 50 pages, are those to which the naturalist will eagerly turn, as containing Dr Gadow's valuable notes on the fauna and flora of the country. These include not only observations on the organisms themselves, but also a record and discussion of the native names for the various plants and animals. These names prove of exceeding interest, for it appears that the languages of the Basques and Celts, the Romans, the Goths, and Arabs have all contributed to the vernacular nomenclature.

As the result of combined observations up to the present time, Dr Gadow points out that all the animals now confined to the Spanish Peninsula have their nearest allies in other European countries, except a species of ichneumon of the African genus *Herpestes.* This is supposed by some naturalists to have been introduced by the Moors to kill rats and mice; but it is specifically distinct from any ichneumons now living elsewhere, and there are other considerations which suggest that it is truly indigenous to Spain. Very little is known of the Pleistocene fauna, except from the caverns of Gibraltar; but there are many indications of a former glaciation of the mountains of the Peninsula, and Dr Gadow himself has contributed an interesting additional proof of the former prevalence of a colder climate in this part of Europe by the discovery of mummified bodies of the lemming in some caverns to the north of Santarem.

The collection of plants identified by Mr Burkill comprises only about eighty species, and Dr Gadow's notes only relate to seventy more; but several hitherto recorded forms are added to the flora of the Asturian and Cantabrian mountains.

A short appendix contains further etymological notes and a list of works referred to in the text, while the volume concludes with a useful map and index.

Practical Botany

Das Kleine Botanische Practicum fur Anfänger. By Dr Eduard Strasburger. Third revised edition. 8vo, pp. viii+246, with 121 woodcuts. Jena: Gustav Fischer, 1897. Price 6 marks.

Teachers and students will welcome the third edition of Prof. Strasburger's "Practical Botany." The arrangement is the same as that of the two previous editions, but the results of experience gained in the four years that have passed since the appearance of the second are embodied in the third. The most noteworthy alteration is the reduction in the number of objects to be examined in each lesson, the author wisely remarking that it is better for learners to gain a more complete knowledge of fewer things. Even as it stands at present, there is matter for several hours' diligent application in each division, and the student who has honestly worked through the thirty-two chapters will have a very fair knowledge of microscopic technique, and of the structure of a representative series of plants. It is often stated that illustrations are out of place in a book dealing with practical work. The excellent figures (prepared by the author himself) form one of the most useful features of the book, and give the lie to such statements; but it is only because they are not mere diagrams, but actual pictures of what the student may see in his own section when he has attained the requisite skill in the use of his razor. Those who have tried both know how much easier it is to work out a section with the help of a picture than by following a mere description, however carefully drawn up. Another good point in Strasburger's arrangement is its commencement with the study of the cell and its contents, followed by a general introduction to plant structure before passing to a more special investigation of individual types. The type system is in fact throughout subordinated to the general idea, and herein lies the chief superiority of Strasburger's over our English introductions to the practical study of plants. It is matter for surprise that, considering the number of our teachers of botany who have been attached to Strasburger's laboratory at Bonn, there is no good elementary book in our language on the same lines.

New Serial

In 1897 Messrs Georg et Cie of Geneva published the first volume of the *Annuaire du Conservatoire et du jardin botanique de Genève.* It is edited by Dr John Briquet, and sold for 5 francs. The volume contains an account of the garden and Delessert herbarium for 1896, and various scientific papers.

Recent Serials

Professor G. S. Boulger will take over the editorship of *Nature Notes* with the April number. The Selborne Society could scarcely have found a more useful officer.

As some of our readers may be meteorologists, we should draw attention to the March number of *The Photogram,* which contains an article on the "Photography of Clouds," illustrated by admirable reproductions of photographs.

With the present year, the excellent little Norwegian Magazine *Naturen,* blossoms out in a new cover of very artistic design. Nos. 1 and 2, which are issued together, contain an article by Prof. R. Collett on "The Mode of Life and Occurrence of the Beaver in Norway," illustrated by figures and two plates. Following it is a careful account by L. Schmelck of some researches on the water from ice-fields and glaciers, discussing the cause of the green colour of the ice-water.

The *American Naturalist* is now published by Messrs Ginn & Co., of Boston, and its January number appeared in a new cover of pleasing appearance, and printed on highly calendered paper of somewhat larger size. The aims of the *American Naturalist* appear, from an editorial note, to remain very much what they were before. They are precisely the same as those of *Natural Science*, and although we cannot fail to miss the guiding hand and original criticisms of the late Prof. Cope, we trust that the new editors of the *Naturalist* may be more successful than ourselves in approaching their ideal. The number in question begins well with a Synopsis of Recent Progress in the Study of Graptolites by Dr R. Ruedemann.

The new "Geological literature added to the Geographical Society's Library during the year ended 31st December 1897" is a bulky volume of 196 pp. As this is published at two shillings, and as the Geological Society gets a large proportion of the publications on geology during any given year, one has practically a "Geological Record" for quite a trifling sum. Moreover, as ninety pages out of the one hundred and ninety-six are allowed for an index to the titles appearing in the first half, our readers can readily understand the value of such a publication. We think a word of thanks is due to the assistant librarian for his compilation, a second to the assistant secretary for his editing, and a third to the society, who certainly provide us with a very respectable if incomplete guide to the year's geology.

No. 2 of the *Bulletin of the Liverpool Museum* was published Feb. 28, and continues the catalogue of birds in the Derby Museum. Dr Forbes also describes, under the name of *Necropsar leguati*, a new species of bird from the Mascarene Islands, supposed to be already extinct. This bird was noticed somewhere about 1730 by some marine surveyor (name unknown), who was sent to Mauritius by the Compagnie des Indes; but the bird to which he refers was unknown to scientific men until Dr Forbes discovered a perfectly preserved skin, among the specimens of *Hypsipetes*, in the Derby Museum, where it had lain unnoticed for nearly fifty years. A sub-fossil species of the genus *Necropsar* was described by Dr Günther and Sir E. Newton in 1874. This number concludes with an illustrated account of some highly ornamented metal-work from Benin, brought from that city by the punitive expedition of 1897.

Further Literature Received

Lessons with Plants, Bailey: Macmillan & Co. Grundzuge d. Geogr.-morph. Methode d. pflanzensystematik, Wettstein: Fischer, Jena. Naturalists' Directory: Upcott Gill. L'Algérie, Battandier et Trabut: Baillière, Paris. Catalogue of Madreporaria, vol. iii., Bernard: Brit. Mus.

Studies from Yale Psychol. Lab., vol. iv. Odours and Tastes of Surface Waters, Jackson & Ellms: *Technol. Quarterly*. Report Lanc. Sea-fisheries lab., 1897. On a Quartz Barytes Rock, Holland: *Rec. Geol. Surv. India*. Ann. Rep. Field Columbian Mus., 1896-7. Fishes and Reptiles from Somali-land, Meek & Elliot. *Field Columb. Mus. Publication*, No. 22. Ann. Rep. Museum, Bolton, 1897. Aus der 22 Jahresversammlung deutsch Ornitholog. Gesell.: *Abh. Zool. Anthrop. Mus. Dresden.*

Actes Soc. Sci. Chili, vii. 4; Amer. Journ. Sci., Mar.; Amer. Nat., Jan.; Botan. Gazette, Feb.; Bull. Liverpool Mus. i., 2; Feuille des jeunes Nat., Mar.; Irish Nat., Mar.; Knowledge, Mar.; Literary Digest, Mar.; Naturae Novit., Feb., No. 3; Naturalist, Mar.; Nature, Feb. 24, Mar. 3, 10; Nature Notes, Mar.; Naturen, Jan. and Feb.; Pearson's Mag., Feb.; Photogram, Mar.; Plant World, Nov. 1897, Jan. Feb. 1898; Revue Scient., Feb. 26, Mar. 5, 12; Riv. Psichologia, 19, 20, 21; Science, Feb. 11, 18, 25; Revue, Sci. Nat. Ouest, No. 2, 1 Ap. 1897; Scientific Amer. Feb. 12, 19, 26; Mar. 5; Scot. Geogr. Mag., Mar.; Scot. Med. Journ., Mar.; Victorian Nat., Jan.; Westminster Rev., Mar.

OBITUARIES

SIR RICHARD QUAIN, Bart.

Born at Mallow, Oct. 30, 1816. Died in London, March 13, 1898

Richard Quain was the son of Mr John Quain, of Carrigoon, Cork, and a relation of Dr Jones Quain, the co-editor of "Quain and Sharpey's Anatomy." He spent the first twenty years of his life in Ireland, having been educated at Cloyne, and later apprenticed to a surgeon at Limerick. At twenty Quain came to London, and continued his medical studies at University College Hospital, where he was House Surgeon for five years. In 1840 he took the degree of M.B. at the University of London, winning the scholarship and gold medal in physiology. In 1842 he took his M.D. degree, being the only candidate to whom the gold medal and the certificate of proficiency were awarded; and within the next four years (before he was thirty) he was elected a Fellow of the University. He is best known as a specialist on diseases of the heart, chest, and kidneys, and was for many years consulting physician to the Hospital for the Diseases of the Chest at Brompton. In 1851 he was elected a Fellow of the Royal College of Physicians, of which College he had been a member for five years, and of which later he became the vice-president. In 1860 he was nominated a member of the Senate of London University, and in 1863 Crown representative on the General Medical Council. He was a most valuable member of this body, acting as its treasurer for some years, and was elected its president in 1891. In 1887 the Royal University of Ireland conferred the honorary degree of M.D. upon him, an example which was followed by Trinity College, Dublin, in 1890, while in 1889 he had received from Edinburgh University the title of LL.D. He was a Fellow of the Royal Society.

Of his numerous writings he will be best remembered by his "Dictionary of Medicine," edited in his leisure hours between 1872 and 1882, in which year it was published in a massive volume of 1800 pages. In 1852 he published his treatise on "The Fatty Diseases of the Heart." In 1872 he delivered the Lumleian Lecture on the "Diseases of the Muscular Walls of the Heart," and in 1885 the Harveian Oration on "The Healing Art in its Historic and Prophetic Aspects."

The death is also announced of Dr T. C. Winkler, for many years curator of the Teyler Museum, Haarlem. He was much interested in the fossil vertebrate animals, of which he had a very fine collection under his charge. He contributed several articles on the specimens in the Teyler Museum to the *Archives* published by that institution.

He also wrote a popular work in the Dutch language on "The Vertebrate Animals of the Past," which we noticed in *Natural Science*, vol. viii., p. 347 (1896). Dr Winkler was elected a Foreign Correspondent of the Geological Society of London so long ago as the year 1874.

PROFESSOR WILHELM JOEST, whose death we recorded in February, died on Nov. 25, 1897, on the Island of Santa Cruz, Australia, while on an ethnological expedition from Berlin, aged 45.

The following deaths are also announced:—

N. ALBOFF, Russian botanist, at La Plata; Dr DELMAS, geologist, at Castries, Aveyron; the Portuguese explorer, Captain ROBERTO IVENS; JEAN LINDEN, the botanist of Brussels, on Dec. 12, 1897, aged 81; Mrs NETTLESHIP, mother of the famous family of Nettleships, at Oxford, in February, aged 81; HEINRICH RIBBE, the entomologist, at Radebeul, near Dresden, on Jan. 19, aged 65; OSKAR VON RIESENTHAL, the ornithologist, on Jan. 21, at Berlin, aged 67; F. W. SEYDLER, botanist, at Braunsberg, aged 80; C. G. SOHST, the conchologist, at Hamburg; IMBAULT HUART, French consul at Canton, well known for his geographical and ethnographical researches in Formosa, aged 40; Dr JOHANN VOLENTIN, of Frankfurt-on-Maine, while on an expedition in Patagonia for the National Museum of Buenos Ayres; Prof. ALBERT ZIONMETER, the botanist, at Innsbruck, on Dec. 15, 1897, aged 49; GUSTAV ZIMMERMANN, the entomologist of Brüx, Bohemia, on Dec. 29, 1897, aged 66.

NEWS

The following appointments are announced :—G. C. Bourne, of New College, Oxford, to be lecturer in comparative anatomy in Oxford University; Dr Pio Mingazzini to be professor of zoology and comparative anatomy at Catania; Francesco Saverio Monticelli, as professor of zoology in the University of Naples; Dr Karl Hurthle, of Breslau, to be professor of physiology and director of the Physiological Institute of that University in succession to the late R. P. Heidenhain; William S. Carter to be professor of physiology in the University of Texas; Dr Gregor Kraus to be professor of botany in the University of Wurzburg; Prof. Eugen Askenasy to be hon. professor of botany at Heidelberg; Dr Magueene to be professor of plant-physiology at the Collège de France; Dr Lüstner, of Jena, to be assistant at the experimental station for plant-physiology in Geisenheim; Lewis V. Pirsson to be professor of physical geology in Harvard University; Baron von Firks, of Mitau, to be assistant in geology in the Bergakademie of Freiburg i. S.

Dr John Murray has been elected a corresponding member of the Russian Geographical Society.

Miss A. Mallet has presented to the Geological Laboratory of King's College, London, a collection of minerals and recent shells.

Owing to the illness of his infant son, Dr Nansen was obliged to cancel his lecture engagements and return hurriedly to Christiana.

Under the will of Mr J. H. Harris, of Wagnesville, Ohio, the U.S. National Museum has received his collection of Ordovician fossils and of antiquities from that locality.

On March 17th, Prof. Boyd Dawkins delivered the 'James Forest' Lecture the Institute of Civil Engineers, speaking on "Geology in relation to Engineering."

After two years' exertion the Hornsey District Council has at last made arrangements to complete the purchase of churchyard, Bottomwood, for the sum of £25,000.

The Dublin Naturalists' Field Club, at a recent meeting, adopted a resolution urging the Boards of Primary and Intermediate Instruction to introduce natural science in Irish schools.

Mr C. W. Andrews, whose stay in Christmas Island, S. Java, is extended for the requirements of his researches, has forwarded five more cases of specimens of natural history objects to the British Museum.

The Lectureship in Geography at the University of Cambridge supported by the Royal Geographical Society has now been made a readership, with a total stipend of £200 a year. The Reader is Mr H. Y. Oldham.

The fifth International Congress of Hydrology, Climatology, and Geology, will be held at Liège this year, from September 25 to October 1, under the patronage of His Royal Highness Prince Albert of Belgium.

kyaks, and a portable boat. Captain Bernier is applying to the Canadian Government for assistance, and is backed by the Geographical Society of Quebec.

The Duke of the Abruzzi, nephew of King Humbert, accompanied by Drs Gonella and Defilippi, intends to visit Spitzbergen during the approaching summer, and in 1899 to visit Franz Josef Land, whence an attempt will be made to reach the Pole by means of sledges and dogs. The latter are to be obtained from Greenland.

THE Annual Report of the Geological Society shows that the Society stands possessed of £13,226, 3s. 8d., exclusive of the library, collections, furniture, and stock of publications. We are glad to see that the Council contemplate spending some of their riches on the "Publication of Hutton MS." We note that electric light in 1896 cost 11s. 6d., and in 1897 £13, 3s. 6d., and suppose we may take the 1897 amount as a fair and reasonable yearly amount for lighting.

PROF. HADDON, whose expedition to Torres Straits we announced last July, left Tilbury on Thursday. Before returning the members will visit New Guinea and Borneo, and carry out a thorough scientific exploration of their mineral and other wealth. We are indebted to the *Echo* for the following details of the expedition. Prof. Haddon is accompanied by several well-known scientists, each of whom will devote his attention to the special branch of study of which he is a master. The leader of the expedition has decided to study the decorative art of the various tribes whom they are about to visit, while native music will be a subject taken up by Dr C. S. Myers, Caius College, Cambridge, who, as part of his equipment, has taken out a phonograph with him, as well as a cinematograph for reproducing dances and other native ceremonies. The well-known Polynesian scholar, Mr S. A. Ray, will direct all observations with regard to the different languages and folk-lore of the tribes, while Dr Rivers, St John's College, Cambridge, will study their beliefs and superstitions. Dr W. M'Dougall, Dr Seligmann, of St Thomas's Hospital, Mr A. Wilkin, of King's College, will make notes on botany, geology, zoology, &c. The object of Prof. Haddon is to obtain full information as to the physical characteristics, customs, amusements, songs, and condition generally of these tribes, and to supplement these observations with collections of scientific interest.

NOTICE

TO CONTRIBUTORS.—All Communications to be addressed to the EDITOR of NATURAL SCIENCE, at 29 and 30 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.

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NATURAL SCIENCE:

A Monthly Review of Scientific Progress.

In addition to Notes and Comments on the progress of Natural Science, brief Reviews of Current Literature, Obituary Notices, and News of Universities, Museums, and Societies, the last number of this Journal comprised the following specially contributed Articles:—

No. 73, March 1898.

I. A New Scheme of Geological Arrangement and Nomenclature. Part III. By Sir Henry H. Howorth, M.P., F.R.S.
II. The Broadening of Atoll-islets. By Charles Hedley, F.L.S., Australian Museum.
III. Second Contribution on Numerical Variation of Parts in Ranunculus repens (L.). By John H. Pledge.
IV. Egyptian Fisheries. By J. C. Mitchell, D.Sc.
V. The Red and Blue Colouring Matters of Flowers. By P. Q. Keegan, LL.D.

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NATURAL SCIENCE

A MONTHLY REVIEW OF SCIENTIFIC PROGRESS

MAY 1898

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NATURAL SCIENCE

A Monthly Review of Scientific Progress

No. 75—Vol. XII MAY 1898

NOTES AND COMMENTS

"Science Falsely So-called"

Not infrequently we receive pamphlets that we ascribe without much hesitation to some hard-worked curate or member of a college essay society, and consign to our rubbish-box with the thought that the young man will grow wiser some day, and 'twould be shame to harm him. Of His Grace the Duke of Argyll we cannot be quit so easily: his pen has a commercial value, and what it writes does not meet with speedy oblivion. For the Duke's latest essay—a lecture entitled, "What is Science?" delivered at the opening of the Literary Society of Inverary, December 23rd, and now published by Mr David Douglas of Edinburgh—we predict a large sale. There is some sound sense in it, with useful warnings against the fallacies that may be involved in a loose use of words, and against travesties of scientific conceptions, but so far as its treatment of actual scientific conceptions is concerned, it is little advanced beyond the usual college essay. We have all of us written plenty of this kind of thing, and we have all of us wondered why scientific workers were so contemptuous of it: pig-headed, short-sighted pedants that they were! Now we ourselves are of the practical pigheads; we are doing definite work in science, and our college essays are thrown aside with the poetry that we have also all written in our springtime. But the Duke of Argyll—should we not honour him for it?—retains all the rashness and verdure of youth, and, despite his own solemn warning, still serves up travesties of scientific conceptions and beliefs that we had thought outworn.

After the 'flower in the crannied wall' has been dragged forth for the usual purpose, we find some curious remarks on Linnaeus, who, we are told, "invented what is called the Binomial system of classification for the organic world. . . . This system . . . has been found to be so truly representative of the facts of Nature that it has been universally adopted by scientific men all over the world."

Here is a strange confusion between classification and nomenclature, between the conception and the mode of expression. This confusion is not due to scientific men, for there can scarcely be a working zoologist or botanist who has not protested that the binominal system does not represent the facts of nature; nor is the system universally accepted. How far the Duke is from apprehending this is shown by the still more startling remarks on pp. 36, 37. The following is given as a certain, undoubted, indisputable fact: "All the fresh forms of life in the past were specifically constant—just in the same way, and to the same degree, as existing forms are specifically constant now. There was no mixture. Each new pattern of creature kept its shape and pattern till it vanished. Each came and went with equal suddenness. . . . Experts in the fossils of each great rock-formation can discriminate all the species of a contemporary and of a succeeding time from each other, with the same precision and absolute identification as we can discriminate living species from each other now." Now this statement is not merely uncertain, doubtful and disputable, but we affirm that it is absolutely untrue, and that if its illustrious author had ever spent a week in the scientific study of those Cretaceous *Terebratulae* or Liassic ammonites that he mentions with such assumption of knowledge, he would have had its untruth forced upon him. People who are not experts can discriminate species with all the precision the Duke of Argyll thinks necessary for his argument, but this is just what the expert finds impossible—or, when it is possible, then he knows that the gaps which limit species are but the expressions of his own ignorance.

We need not combat the statement that the genus *Terebratula* "appeared in some of our oldest rocks," but it is surprising to see the idea of the persistence of certain organic types brought forward as though no modification of it had been rendeıed necessary by the advance of knowledge during a quarter of a century. So too the peculiarly erroneous remarks about the Lias ammonites might pass as mere slips, were it not that they convey a distinctly false impression, and prejudice the mind of the audience. It is not true that ammonites appear in the Lias for the first time, but were it true, their connection with the ancestral goniatites would be none the less close. It is not true that the species of ammonites are rigidly confined to zones, and that there is no link between them, "no uncertainty as to the minutest marks of difference and of identity." This is perhaps the idea one gets from text-books and from lists of fossils, but it is not the idea of the field-geologist or the practical monographer of ammonites.

To treat this kind of thing seriously is almost as absurd as the Duke's own scarification of Mr E. Stenhouse, A.R.C.S., who, it seems, wrote a popular and chatty article on "How plants live and work" in an esteemed contemporary devoted to such literature. That the Duke should regard this seriously as 'descriptive science' by 'an advanced Darwinian' seems to show a curious absence of the real article from the bookshelves of the Athenaeum and Inverary Castle. When we come to the real argument, we find but little. The case against Darwinism may thus be summed up: 'Natural Selection' is a shocking bad metaphor, and the first 'Living Forms' arose from something that was not a 'common Parentage.' "Clearly, then, the solution offered by the Darwinian theory is . . . worse than inadequate." Not the Duke of Argyll himself can be expected to demolish the Darwinian explanation of evolution in an hour's lecture, but even a local literary society might ask with justice for more argument and less majuscular rhetoric.

The History of Geology at Oxford

In the last number of *Science Progress* Mr Sollas gives us the first fruits of his appointment to the Geological Chair at Oxford. We hope it will be followed by many harvests from the same inspiring pen. Mr Sollas must accept it as a proof of our appreciation of his work that we should raise a small polemic with him. The object of his paper is to trace the connection of Oxford with the history of geology, but Oxford has very little to do with a good deal of it. The account of Steno the Dane, which is interesting, is followed by an amusing and freshly written notice of Plot, the queer and old-world author of the Natural Histories of Oxfordshire and Staffordshire. We do not quite see how he represents the serious science taught at Oxford at any date, nor has he any real claims to a position in the history of modern science at all. Like Kircher, he was a survivor from the Middle Ages, writing in a style which is perfectly delightful for us to read. He takes us far away from orthodox thought to the kind of scientific notions which perhaps in our day sometimes floated through the brain of Charles Lamb. He was no doubt an F.R.S. and much laughed at by his friends, but he had as much in common with the little band of real men of science who founded the Royal Society as he had with the committee which tried Galileo. If we want to see what a really thoughtful man in England had made out at this time in the shape of a geological theory, we ought to turn to Martin Lister, whose work, and especially his paper in the *Philosophical Transactions*, is a remarkable production, and is only casually referred to by Mr Sollas. Though originally a Cambridge man, he was connected with Oxford, presented much to the Ashmolean Museum, and was made an M.D. of the University in 1684. He is generally credited with having been one of the first to suggest geological maps.

We also think that Prestwich might have occupied rather a larger space in a paper devoted to the connection of Oxford with geology. It is quite true that neither Prestwich nor Green fulfilled the real functions of a professor of geology. They neither of them formed a school or stirred at Oxford the ingenuous youth who are so easily stirred into an interest in the most catholic of sciences, nor did they even set their geological museum in order; but Prestwich filled the chair of geology for a long time at Oxford, was distinctly an old master in the science, and did much to help us to understand the gravels and clays and sands that cover the broken bones of the world with such a gently contoured mantle. May we hope that when the mocos have gathered round Mr Sollas' head, there will be as numerous, as distinguished, and as hearty a band of old Oxford pupils to do him honour as the other day gathered round the Woodwardian professor of Cambridge.

LUIDIUS

A MORE serious and truly strange omission from the lecture by Professor Sollas demands a separate note. It is indeed remarkable to find no mention whatever of a man whose reputation was European, and whose fame has lasted to our own day—except, apparently, "in his own country." Need we explain that we refer to Edward Lhwyd, who succeeded Plot as keeper of the Ashmolean Museum? Honest Lhwyd, as he was often called, was a man of no common industry, and spent much of his time travelling, chiefly in Wales, to collect specimens for the museum, or to examine into the languages, histories, and customs of the United Kingdom. The merest fragment of his results was published, as the first and only volume of the "Archaeologia Britannica" (1707). In geology, Lhwyd's first work was the publication of a systematic catalogue of the fossils and minerals in the Ashmolean collection, under the title "Lithophylacii Britannici ichnographia" (1699). The University, with remarkable meanness, refused to print the small octavo, and an edition of 120 copies was issued at the expense of Sir Isaac Newton, Sir Hans Sloane, and a few other learned men. After his death a second edition, with much supplementary matter, was published by William Huddesford (1760). No salary was attached to the post of Ashmolean Cimeliarch, and the chief reward that Lhwyd obtained from the University was the degree of M.A. *honoris causa*; but in return for this he had to deliver a public lecture upon natural history, "one every year during the space of six years." Thus originated his famous "Praelectio de Stellis marinis," first published in J. H. Linck's folio of the "De Stellis marinis liber singularis" (Leipzig, 1733). In November 1708, despite the opposition of Dr Woodward, whose diluvian views as to the origin of fossils Lhwyd had attacked, he was elected a fellow of the Royal Society; and four months later the University recognised his scientific attainments by electing him superior beadle of divinity. But he died soon after, of pleurisy supervening on asthma, on June 30, 1709, being but 49 years of age.

Lhwyd was no mere cataloguer of fossils as curiosities; he recognised their true nature. "Many fossils which from long centuries ago down to the present day have been regarded as minerals, proclaim their animal origin if examined more closely: this I specially assert of the Asteria, Encrinus, and Entrochus, which I am wont to contend are nothing other than the petrified ossicles of sea-stars." As to his drawings, we have the opinion of an expert in Mr A. C. Seward's recently issued volume on "Fossil Plants":—"The oldest figures of fossil plants from English rocks which are drawn with any degree of accuracy are those of Coal-Measure ferns and other plants in an important work of Edward Lhwyd." For all this, Lhwyd did not arrive at true geological conceptions; he argued to excellent purpose against the diluvial origin of fossils, but in a letter to John Ray he suggested that they might have developed from germs brought up by vapours arising from the sea. At the same time, he insisted on the tentative nature of the suggestion, saying, "For hypotheses I have cared the less, as I have always loved natural history the more." He was, in short, an insatiable collector of facts, and capable of using his wide knowledge acutely in destructive criticism of contemporary hypotheses; but he knew too much to think that he could himself put forward a satisfactory explanation.

THE CAUSE OF SUNSTROKE

THE current physiological theory of the existence in the body of a heat-regulating nervous centre is often supported by reference to the phenomena of sunstroke. The theory accordingly suffers by a remarkable paper on the etiology of sunstroke in a recent number of the *British Medical Journal*, in which Dr Luigi Sambon practically destroys all faith in the physical cause of that malady. Dr Sambon points out that two different conditions are often classed together as "sunstroke," and that these must be clearly distinguished before any progress can be made in understanding the genuine sunstroke. Thus he holds that many reported cases are only due to syncope. When these and other analogous cases are eliminated there remains a thermic fever, which Dr Sambon attributes to a specific organism, and for which he adopts the older name of "siriasis." This theory is startling, but Dr Sambon adduces convincing evidence in its support. He shows that it is a disease with definite symptoms and a definite geographical distribution. That it is not due to the excessive heat he shows, and quotes cases in which people work exposed to the hottest sun, or under artificial conditions in temperatures far higher than that of any climate in the world, but nevertheless never suffer from the disease. Animals have been subjected to heat so intense as to cause their death, but they have not shown the symptoms of siriasis. The geographical distribution of the disease gives very strong support to Dr Sambon's theory. For true sunstroke is quite absent from many areas where the sun's heat is exceptionally powerful, as on the dry plains of Colorado, while it is prevalent in low, damp regions, such as the Mississippi valley, where the actual temperature is lower. In India again, it is common on the low-lying plains of the Ganges, but is unknown on the higher central plateau where the sun's power is much greater. Another argument in favour of the organic origin of the malady is the fact that it comes in epidemics, which often decimate hospital wards, where the patients are protected from the sun, and are less fatal among men working exposed to the full fury of the sun. Dr Sambon compares the bacterium to that of tetanus, for he considers that it lives in the soil, and is carried with dust either into the lungs or alimentary canal, where it forms a toxic poison which is the actual instrument of death. Dr Sambon's suggestive paper shows how little is yet known regarding some of the most fatal of tropical diseases, the further study of which is a duty on the part of the nation, which is responsible for the administration of so large a proportion of the tropical regions.

The Effect of Aquatic Plants on Water Supplies

Now that the legislature throws upon our urban and district councils the onus of providing 'pure' water, the study of everything affecting either the purity or palatability of water has assumed great practical importance. As a broad fact it is doubtless true that the growth of green plants in water causes the water to be oxygenated and makes for purity. It has been known for some time that the growth of certain low forms of vegetable life in water is capable of imparting to it a disagreeable odour owing to the impregnation of the water by bodies resembling essential oils. Thus it is recorded by Dr Thresh, that in 1891 the water of Bolton in Lancashire acquired a 'fishy' odour and taste owing to the growth of a certain fresh-water alga, and a similar accident happened at Cheltenham in the same year.

The latest addition to our knowledge on this subject comes from Massachusetts in a paper by Messrs Jackson and Ellms, who are respectively assistant biologist and assistant chemist to the State Board of Health.

In 1895 an abundant growth of *Anabaena*, one of the Cyanophyceae, took place on a reservoir at Springfield, Mass. The growth imparted to the water an odour of mouldy grass, and by extracting a large quantity with gasolene, the authors of the paper succeeded in obtaining an essential oil having the characteristic smell of 'mouldy grass.'

It is evident that any vegetation taking place in water may die and undergo a process of rotting and putrefaction, and it must be clearly understood that *Anabaena* and other organisms impart their odour during growth and not after death. When the *Anabaena* dies and decays it imparts to the water the odour of a pig-stye, or, as the authors of the paper put it, "the odor of a pig-pen."

The mutual relation between aquatic plants and the medium in which they grow is a question well worthy careful study, and is calculated to throw some light on the seasonal variations in the quality of water from the same source, which are often a puzzle to the analyst and not without interest to the epidemiologist.

The North Sea Fisheries

The Northumberland Sea Fisheries Committee have continued their trawling experiments, and the results obtained during the summer of 1897 are embodied in a small Report. So far as the observations extend, they appear to point to the conclusion "that the flat fishes during the last two years have increased to the extent of thirty per cent compared with the four previous years." This result must be regarded as encouraging, though the experience of similar experiments elsewhere demands extreme caution in drawing conclusions.

In the account of pelagic eggs, those of the weever, dab, and gurnard are said to occur in the inshore waters in abundance, in addition to unknown eggs, 'perhaps Müller's topknot,' some of which may be *Motellae*. So long as trawling operations are commenced as late as the third week in June, the Committee must not expect to obtain the eggs of gadoids or of the commoner pleuronectids in either the inshore waters or beyond the three-mile limit. If a serious attempt at investigating the pelagic eggs of the district is to be made, systematic trawling should commence at the latest in early March.

The founding of a laboratory at Cullercoats is due, as is so often the case in our country, to the generosity of a private individual, and in this instance the donor is Mr Dent, the owner of the steamer *Livingstone*, with which the trawling experiments have been accomplished. With the College of Science in easy distance, and with many well-known local naturalists, this laboratory should have a bright career before it.

Plankton Crustacea of Lake Mendota

For two years and a half, namely, from the middle of 1894 to the end of 1896, Prof. E. A. Birge, of the University of Wisconsin.

carried out the very praiseworthy but arduous work of systematically watching the Crustacea of the plankton of Lake Mendota. The results, which have now been published in the *Transactions* of the Wisconsin Academy of Sciences, etc. (Vol. xi. pp. 274-448), form extremely instructive reading to those who, like ourselves, are interested in the progress of fresh-water biology.

Briefly, the method adopted in the work was as follows: Vertical hauls were made with a plankton net, at very frequent intervals, at a spot about half a mile from the southern shore of the lake. A separate haul was made for every three metres of the total depth (18 m.), the mouth of the net being closed at the proper moment by a messenger sent down the line. The Crustacea contained in a definite fraction of each collection were then counted and the figures so obtained formed the basis of all the subsequent calculations as to the periodicity, vertical distribution, etc., of the different species.

It appears that Lake Mendota has altogether eleven or twelve species of limnetic crustacea (Entomostraca). Of these, only eight are of real importance as constituents of the plankton. As regards the seasonal distribution of the Crustacea as a whole, Prof. Birge finds that there are, during the course of each year, three maximum periods of development, occurring in May, July, and September-October respectively. During the spring maximum, which is the greatest, and is produced almost entirely by the marvellous increase of a single species of *Cyclops*, as many as 3,000,000 individuals may be present under each square metre of the surface of the lake. As regards the occurrence of the individual species a very valuable collection of facts has been obtained, but it is impossible to refer to these in detail. It may be specially mentioned, however, that in one case, *Daphnia pulicaria*, the evidence seems to show that biennial periodicity may occur among plankton organisms as well as annual.

The vexed question of the uniformity or otherwise of the horizontal distribution of the plankton is answered by Prof. Birge as follows: "In general, there is no evidence of swarms in my observations, either of all the Crustacea or of single species," but "the variation of the numbers of the Crustacea in Lake Mendota does not support extreme views either on the side of uniformity of distribution or the opposing theory of swarms."

THE "SPRUNGSCHICHT" OR THERMOCLINE

ONE of the most characteristic features of the work carried out on Lake Mendota is the attention which has been paid to what is known to German authors as the "Sprungschicht," or, as Prof. Birge calls it, the Thermocline. It was first announced by E.

Richter in 1891 that there existed in many lakes, during the summer, a layer of water in which an extremely rapid decrease in temperature took place. For instance, the difference in the temperature of the water of a lake from the surface down to perhaps ten metres might only be one or two degrees, whereas in the next metre or two the decrease might amount to ten degrees or more. Below this, again, the temperature would decrease comparatively slowly and uniformly to the bottom. The layer showing the great jump in temperature was christened the "Sprungschicht," and its position was found to vary with the season, gradually sinking as the year advanced.

References to the "Sprungschicht" or Thermocline have been made by many subsequent writers, but Prof. Birge seems to be one of the first to have shown how important the phenomenon may be to the plankton organisms in certain cases. In Lake Mendota, for example, he finds that the formation of the Thermocline is followed by the practical stagnation of the whole mass of the water lying below, and as the latter is obviously shut off from direct exposure to sun and air it soon becomes unfit to support the majority of the inhabitants of the lake. In less than a month, therefore, after the appearance of the Thermocline, *i.e.* before the middle of July, from 90 to 95 per cent. of all the Crustacea present in the lake are found above the 9-metre level, which represents approximately the middle of the Thermocline at that date. This state of things continues during July, August, and part of September, except that, as the Thermocline moves gradually downwards, the Crustacea have more room in which to develop, and so, by the middle of September, the high percentages mentioned can only be obtained by collecting to a depth of about 12 metres. Two of the effects of this practical exclusion of life from the lower water during the summer are, that the total number of Crustacea in the lake is far smaller than it would otherwise be, and that the perennial species which are unfavourably affected by heat, being unable to retire into the cooler water, decline in numbers. Perhaps also the small number of periodic species in the lake may be due to the fact that they have little chance to develop when the habitable water above the Thermocline is so completely occupied by the perennial forms.

The Thermocline is closely connected with another peculiarity of Lake Mendota, and that is the enormous increase of the vegetable plankton in the late summer and autumn, for it appears certain that this is rendered possible by the liberation of the products of decomposition stored in the deeper water, as the Thermocline moves downwards.

As regards the production of the Thermocline, Prof. Birge believes that, in Lake Mendota at least, it is due to the concurrence of gentle winds and hot weather, and not to the day and night differences in temperature of the surface water, as is stated by Richter and others.

THE MOLLUSCA OF LAKE TANGANYIKA

SINCE his return from Africa, Mr J. E. S. Moore has been hard at work examining the animals, especially the mollusca, brought back by him from Lake Tanganyika. It has already been announced that the fauna of this lake furnishes evidence for its connection with the sea at no very distant date, geologically speaking. On January 27th Mr Moore read before the Royal Society a paper on this subject, of which the following extract has been issued:—

"The results of the morphological examination of the animals obtained have made it evident that the fauna of Lake Tanganyika must be regarded as a double series, each half of which is entirely distinct in origin and nature from the other. The remarkable molluscan shells which were brought home by Burton and Speke, form but a small part of the molluscan section of the more abnormal of these fresh-water stocks. Besides molluscs, the lake was found to contain fishes, Crustacea, Coelenterata, and Protozoa, all of which, like Speke's shells, present the most curious marine affinities; and for distinctive purposes the individual members of this unique assemblage of quasi-marine fresh-water organisms are described as members of the Halolimnic group.

"The distribution of the aquatic faunas occurring in Lakes Shirwa, Nyanza, Kela, and Tanganyika, all of which were visited and dredged during the expedition, shows (together with what is already known respecting the Victoria Nyanza and the more northern lakes) that the Halolimnic animals are exclusively restricted to Tanganyika. It is thus rendered inconceivable that the Halolimnic forms can have arisen through the effect of ordinary conditions operating upon the population which the lake originally possessed. For the same reasons, it becomes equally clear that the Halolimnic animals cannot be regarded as the survivors of an old fresh-water stock. Since, if we accept either of these suppositions, we are bound by the facts of distribution to believe also that the Halolimnic animals have been destroyed in every African lake but one; a supposition which may be ingenious, but which, when the number of lakes existing in the African interior is fully realised, becomes grotesque.

"Apart from the physical difficulties which the present effluent of Tanganyika presents to the ingress of organisms from the sea, it is impossible to regard the Halolimnic forms as having recently transmigrated thither from the ocean, since none of these animals is exactly similar to any marine organism at present known. They must, therefore, have been in Tanganyika long enough to be modified into their present condition from the living oceanic species which we know, or they retain the characters of a sea-fauna that has elsewhere become extinct.

"The delicate nature of the lake medusae, and the fact that most of the Halolimnic molluscs are exclusively deep-water forms, renders it impossible that these organisms can have made their way into Tanganyika at any time under the physical conditions which now exist.

"The facts of distribution and the general character of these forms, as well as the geographical conditions of the lake in which they are now found, lead then to the conclusion that the Tanganyika region of Central Africa must have approximated to a deep arm of the sea in ancient times.

"This view is finally confirmed by the details of the anatomy of the Halolimnic animals themselves. For some of the individual molluscs of this group combine the characters of several of the most modern marine genera. The Halolimnic fauna of Tanganyika, therefore, cannot represent an extinct fresh-water stock, since the characteristic fresh-water organisms of the present day (which would, in such a case, have to be regarded as their linear descendants) possess the anatomy of vastly older types.

"To the Halolimnic animals there thus attaches the unique interest that they themselves constitute the few surviving indications of an old sea which once extended far into the African interior, and which, judging from the characters of the animals it left behind, must have retained its connection with the ocean at least as late as Tertiary times.

"These conclusions, it will be observed, are directly in opposition to the views which were originated by Murchison, and which depict the African interior as never having been below the sea, at least since the New Red Sandstone age."

Some New Bivalves

There is evidently much work to be done amongst small bivalves. Most collectors shirk them owing probably to the difficulty they find in discriminating between young individuals of large species and genuine small forms. Mr Felix Bernard's researches into the development of the hinge in the Pelecypoda are, however, doing much to make the task easier; whilst to the fact that in the course of his studies, small shells have of late principally occupied his attention, must be due his numerous recent discoveries of minute but fully grown pelecypods. Three new genera are now founded by him (*Bull. Mus. Hist. Nat. Paris*, 1897, pp. 309-14) on specimens in material from Stewart Island, near New Zealand. One of these, *Pachykellya*, belongs to the family Erycinidae, the other two, *Cyamiomactra* and *Perrierina* are doubtfully referred to the Mactridae. The first named measures but 1 × 2 mm. and the second 4·5 × 3 mm. The third is, however, perhaps the most interesting, seeing that its hinge, like the Tertiary genus *Woodia*, Desh., combines cardinal teeth of the heterodont type, with laterals that recall those of the Anisomyaria and even the Taxodonta.

North American Copepods of the Genus Diaptomus

The Illinois State Laboratory of Natural History has lately issued another of its very useful little Bulletins dealing with North American fresh-water Invertebrates. It treats of the genus *Diaptomus* and is essentially a systematic paper, appealing mainly to students of the Copepoda. But it also brings out a point of some general importance, namely, that not one of the twenty-two species described occurs in any part of Europe. This is a very remarkable fact when it is remembered that nearly all the North American species of the genus *Cyclops*, and more than fifty per cent. of the fresh-water Ostracoda and Cladocera are common to North America and Europe. It looks very much as if, notwithstanding a continuous process of intermingling of the majority of the Entomostraca of the two continents, the American forms of *Diaptomus* have been effectively isolated from all European influences for a sufficiently long time to allow of the development of an entirely new set of species. This, of course, implies that the Atlantic forms an impassable barrier to the latter, but that it does not seriously affect the active dispersal of the Entomostraca as a whole, and it also probably implies that the comparatively short time (geologically) which has elapsed since the breaking up of the northern land connection between America and Europe has been sufficient for the production of great difference in the *Diaptomus* faunas of the two continents which now exists.

Some Remarkable Earwigs

Among recent contributions to the life-histories of insects, none will prove more interesting than the paper by Mr E. E. Green, which was read before the Entomological Society last March. About two years previously the author had exhibited to the Society some remarkable insects from Ceylon, which he believed to be later stages in the development of the species figured and described by Westwood in 1881 under the name of *Dyscritina longisetosa.* Westwood was unable to assign a definite systematic position to his new genus, the type of which was a small insect, somewhat resembling an earwig, but provided with two long slender tail-filaments, each of which was about three times the length of the creature's own body, and made up of about fifty minute segments. Mr Green was able to shew by means of drawings that, except for the segmented tail-appendages, the insects he exhibited possessed all the characters of true earwigs, and he ventured to express an opinion, in which he was supported by Mr Gahan, that these insects represented larval stages of forms which should be referred to the family Forficulidae. Though this view met with strong opposition at the time, it is now proved to be entirely true. Since his return to Ceylon, Mr Green has studied pretty completely the life-histories of the insects in question, and in the paper referred to has communicated the results of his investigations. He has shewn that the long-tailed larvae become transformed into ordinary-looking earwigs, each provided with the orthodox pair of pincers. The segmented tails are retained by the larvae up to the final moult preceding the change to the imago. They then disappear, with the exception of the long basal segment of each tail. The pincers of the imago may be seen formed within these basal segments, and Mr Green is of opinion that the pincers represent the basal segments only of the larval tails. Mr Gahan, however, considered it more probable that the whole interior substance of each tail was contracted within the basal segment, and there became changed into the pincer, in support of which view he mentioned the fact, noted by himself, that traces of segmentation might be observed in the pincers of the common earwig at a stage just preceding the exclusion of the embryo from the shell.

PHOTOGRAPHY IN GEOLOGY

IN our March number we alluded to the work being done in collecting photographs illustrative of British Geology. It is no doubt a fact that the prints receive house-room at the Museum of Practical Geology, but the actual work is one carried out by private individuals. The April number of the *Photogram* gives a very interesting account of the photographic work in connection with the U.S. Geological Survey. We have, in this country, various scientific institutions under Government, which appear to make far less use of photography than might be expected; but this is a subject to which our opportunities of returning will be perennial. At present we content ourselves with quoting some of the paragraphs from the *Photogram*.

"The official photographer of the Survey, J. K. Hilliers, who is responsible for the work of a very great number of field photographers, and also of a considerable in-door staff, is entirely a self-taught man, who originally joined one of the geological parties as a baggage packer. Assisting the photographer attached to the party, he gradually picked up the methods of working, and, when the photographer died, was able to continue his duties. From this time he always remained a photographer, gradually rising in the service, until with the increase of the service itself, he has now a very responsible position. His work shows that he is a photographic genius, as well as a good deal of an artist, as our readers will have an opportunity of proving for themselves at the Paris Exhibition of 1900. For this exhibition the Geological Survey is preparing a magnificent collection of very large-sized photographic transparencies of American scenery, all beautifully coloured, and from what we have seen of the few that were already completed, and others in progress, when we were in Washington, we feel safe in saying that they will be amongst the most interesting and striking of the photographic exhibits at the Paris World's Fair."

"The Photogram" on the U.S. Geological Survey

"The Geological Survey, unlike the War Department, is very well housed in a good studio and excellently fitted dark-rooms and work-rooms. Its space is none too large for the number of men who are busy therein, but they have, at any rate, every convenience they can fairly ask. Of course the bulk of their work consists of developing, printing, and generally making useful the exposed plates which are sent to them by workers in the field, and, as there are over one hundred cameras in constant use by members of the Survey, for each field party has at least one camera in its equipment, it can easily be imagined that the work is no sinecure. Perhaps we ought to explain that the Geological Survey in America occupies a much more important position, and has much more extensive duties than its name would seem to indicate, for its work includes that which, in Britain, is undertaken by the Ordnance Survey, in addition to the work of a Geological Survey proper. Even geology is understood in a very wide sense by the American Government, for the most important 'rock' with which they deal is water, and it is one of the duties of the Survey to give information to people all over the States, who may be contemplating the driving of wells or the planning of irrigation works. This being the case it can easily be understood not only that the hydrographer's department, with F. H. Newell at its head, is an important and busy section, but also that the work generally is much more extensive than the work of a similiar survey in Europe. The printing and publishing department of the Geological Survey (for it issues all the official maps of the States) is decidedly interesting, though decidedly disappointing from the point of view of the photographic and photo-mechanical enthusiast. Probably there is no Survey in the world in the map-printing establishment of which photography is so little used. This does not, in any sense, arise from any objection to photographic methods, for, as a matter of fact, the head of the printing department is most anxious to use photography wherever practicable, and has an unusually good knowledge of its possibilities and application. At the same time, the introduction of photography to any great extent would mean a revolution of the whole methods of the Survey, and would have only doubtful advantages, since the present working seems to compare very favourably, in point of both cost and accuracy, with the methods of other countries working similar territory. One reason why it is difficult to use photography to the same proportionate extent, as is done, say, in the printing offices at Southampton, is that the American maps are all printed in a considerable number of colours. Of course, even in this case, it might be urged that the black printing could be done from photo-gravure plates, and the colour plates made on transfers therefrom, but the American surveyors seem to decidedly prefer to have their maps finished up by the workers actually in the field. The result of this is that, although the field worker's map is perfectly accurate for engraving by hand, it is not sufficiently clean and sharp in its lines for direct photo-engraving; hence, if photographic reproductions were used, it would be necessary to make a clean copy by hand in the establishment, and it is considered more accurate and more economical to make a transfer from the field map direct to the copper-plate and to engrave it by hand. The exceedingly low price at which the maps of the Survey are sold, shows that their cost of production must be reasonable, since they are supposed to be sold at cost price, and certainly their cost to the public compares very favourably with the price of the maps produced by the British Surveys."

Rust on Cereals

Professor J. Eriksson has published in the *Botanical Gazette*, vol. xxv., Jan. 1898, a short account of the results he has obtained from his researches on the rust of cereals at the experiment station of the Royal Swedish College of Agriculture.

He subdivides the three species of rust, *Puccinia graminis* Pers., *P. rubigo-vera* D.C., and *P. coronata* Corda into ten distinct species, and he finds further that forms of these have become restricted to particular hosts; the danger of infection spreading from one diseased grass to that of another genus or species is thus very much lessened. Another point of extreme interest is the difficulty he found in inducing certain teleutospores to germinate until, by laying them on ice, he imitated natural conditions of winter temperature. This explains the difficulty experienced in germinating the spores obtained from straw used as manure, and it gives the agriculturist greater confidence in using rusted straw.

After many experiments Professor Eriksson has come to the conclusion that though rust increases by infection it is largely propagated by inheritance. He supposes that "the fungus lives for a long time a latent symbiotic life as a mycoplasma in the cells of the embryo and of the resulting plant, and that only a short time before the eruption of the pustules, when outer conditions are favourable, it develops into a visible state assuming the form of a mycelium."

This is evidently a matter of theory and will be difficult to prove, but we await Professor Eriksson's further publications with great interest. The whole subject, as he himself states, is of great practical importance.

The New Academy of Sciences at Washington

The following have been elected officers of the Washington Academy of Sciences:—President, J. R. Eastman; secretary, G. K. Gilbert; treasurer, Bernard R. Green; managers—Alexander Graham Bell, Frank Baker, F. W. Clarke, C. Hart Merriam, H. S. Pritchett, George M. Sternberg, Charles D. Walcott, Lester F. Ward, and Caroll D. Wright. The seven vice-presidents will be nominated by the seven affiliated societies — Anthropological, Biological, Chemical, National Geographic, Geological, Entomological, and Philosophical. The Act of Incorporation of the Academy states that its object is the promotion of science with power:—(*a*) To acquire, hold and convey real estate and other property and to establish general and special funds; (*b*) to hold meetings; (*c*) to publish and distribute documents; (*d*) to conduct lectures; (*e*) to conduct, endow, or assist investigation in any department of science; (*f*) to acquire and maintain a library; (*g*) and, in general, to transact any business pertinent to an academy of sciences.

The South Kensington Museum

In connection with the proposed action of the Government at South Kensington Museum, the following little extract from the report of proceedings in Parliament on March 29th may interest and possibly amuse our readers:—

"Mr H. Lewis moved to reduce the amount proposed to be expended at South Kensington by £100,000, in order to draw the attention of the Government to the fact that in Wales there was no provision for museum purposes such as existed in England, Ireland and Scotland. What he asked was that in Wales they should have a storehouse for their national art treasures.

"Mr Labouchere suggested that the Imperial Institute should be purchased and used as the new museum for science and art treasures. The Colonies had declined to subscribe to it, and as it was principally used as a species of music-hall, he thought it might be bought cheaply.

"Mr Akers-Douglas said he sympathised with the desire of the Welsh people to have a proper museum, but it was a matter that he could not enter into upon this Bill. As to the Imperial Institute, he understood that the owners were not willing to sell, and it was not the intention of the Government to ask for compulsory powers of purchase."

The Newcastle Museum

We much regret to learn that the building of the Museum at Barras Bridge, Newcastle, is in a somewhat dilapidated condition, and that it was necessary to call a special meeting of the members of the Natural History Society of the counties of Northumberland, Durham, and Newcastle, on March 16th, to consider how funds could be raised to provide the necessary repairs. The connection of this museum with Albany and John Hancock is well known, and many other naturalists of repute have carried on their work there. In consequence of this, the collections are of more value than is usual in a provincial museum, and it is certainly some consolation to find that the members of the above-mentioned Society fully realise the necessity of doing their work thoroughly while they are about it. £110 was promised by those present at the meeting. Lord Armstrong, who is President of the Society, has offered £500, and Mr Watson-Armstrong, who took the chair at the meeting, subscribed another £250. There can be little doubt that to such an example the public will respond, and that before long the £2500 necessary will be acquired. So long as the members of the Natural History Society of Northumberland, Durham, and Newcastle realise their responsibilities in this excellent fashion, so long, no doubt, will their Museum continue to be cared for, as it deserves. But it must not be forgotten that Societies are composed of men, and that men are but mortal. Those may arise in the land who know not Hancock, and who do not feel inclined to double their subscriptions, or to subscribe odd £500-notes. As has often been urged by Sir William Flower, the only way in which museums of this kind can be secured against possible disaster is to place them in the hands of some public body. We will not say that a municipal corporation is in itself more friendly to science than a private scientific society, but it has the one great advantage of permanence.

The Chadwick Museum, Bolton

A striking novelty in museum exhibits has been introduced at the Chadwick Museum of Natural History, Bolton, Lancashire, in the shape of a colony of honey bees. These are kept in an 'observatory hive,' which stands on one of the side tables overlooking the Park, with an aperture through the wall for egress and ingress. We are told in the *Report* that "to watch the movement of these industrious insects inside their house has excited the wonder and admiration of thousands of visitors." We do not suggest that a museum should usurp the functions of a zoological garden, but anything which tends to enliven these storehouses of the dead is to be commended, and we hope the example set by Bolton will be followed.

It is a move similar to that made by the Australian Museum at Sydney and the Whitechapel Museum, London, of holding temporary exhibits of cut flowers.

We notice that the Chadwick Museum is making progress in other directions. It has acquired a collection of British birds' eggs containing 229 species, and the number of birds' nests has been increased, chiefly by a loan for an indefinite period by Mr F. W. Peaples. During the past year 2500 specimens of British insects, chiefly British Coleoptera and Lepidoptera, have been acquired. A large Polar bear has been presented by Messrs Jennison; and the mammalian collection has been rearranged in a new large cabinet. Numerous visits have been made by classes from the elementary schools and by Natural History Societies from the neighbourhood. The Bolton Botanical Society holds its fortnightly meetings in the building. The whole *Report* gives evidence of praiseworthy activity. The curator, who also combines the functions of meteorological observer, is Mr W. W. Midgley.

Pigeon-holes for Zoologists

In our April number we gave some account of the present position of the International Bureau for the Bibliography of Zoology at Zurich, and we then stated the facilities that are now offered to subscribers. By arrangement with Dr Field we are able to present purchasers of the present number with the scheme of classification now adopted for a portion of the subjects dealt with by the Bibliographic Bureau, and we need only remind our readers that they can purchase, in card-catalogue form, the entries relating to any one of these headings or to any combination of these headings. For example, one can order the entries relating to 591412, the anatomy of the heart; or 59932, Rodentia; or 59(4346) zoology of Baden; or 59932 (4346), Rodents of Baden. We are told that about 100 entries are printed every day under the headings contained in this list, and this gives some idea of the flood of literature under which the isolated worker must sink if he does not use some such means as this to help him. This list need not be regarded merely as a sale-catalogue, but it may offer suggestions to workers as to a plan of arranging their own pamphlets or even manuscript notes. Many people no doubt like to make their own pigeon-holes, but others never find the time for that, and are glad to take them ready-made. As has often been pointed out, this system of division and notation permits of indefinite extension; and in this especially lies its value to the private worker.

I

Cell-Physiology

[THE revised *Principles of Biology* which I am slowly preparing is to contain additional chapters; sundry of them made needful by recent developments of biological science. One of these, entitled "Cell-Life and Cell Multiplication," describes, so far as brevity allows, the revelations which late years have witnessed respecting the processes of cell division and cell fertilization. Study of the facts and hypotheses, as set forth in recent works, have suggested to me some interpretations which I have not met with. I have thought it as well to publish them now, not waiting for completion of the first volume of the *Principles of Biology*; as this will be long delayed, even if ill-health does not prevent completion of it. The following are the relevant passages.—HERBERT SPENCER.]

THE chemical composition of chromatin is highly complex, and its complexity, apart from other traits, implies relative instability. This is further implied by the special natures of its components. Various analyses have shown that it consists of an organic acid (which has been called nucleic acid) rich in phosphorus, combined with an albuminous substance: probably a combination of various proteids. And the evidence, as summarised by Wilson, seems to show that where the proportion of phosphorized acid is high the activity of the substance is great, as in the heads of spermatozoa; while, conversely, where the quantity of phosphorus is relatively small, the substance approximates in character to the cytoplasm. Now (like sulphur, present in the albuminoid base), phosphorus is an element which, besides having several allotropic forms, has a great affinity for oxygen; and an organic compound into which it enters, beyond the instability otherwise caused, has a special instability caused by its presence. The tendency to undergo change will therefore be great when the proportion of the phosphorized component is great. Hence the statement that "the chemical differences between chromatin and cytoplasm, striking and constant as they are, are differences of degree only;" and the conclusion that the activity of the chromatin is specially associated with the phosphorus.

What, now, are the implications? Molecular agitation results from decomposition of each phosphorized molecule: shocks are continually propagated around. From the chromatin, units of which are thus ever falling into stabler states, there are ever being diffused waves of molecular motion, setting up molecular changes in the cytoplasm. The chromatin stands towards the other contents of the cell in the same relation that a nerve-element stands to any element of an organism which it excites: an interpretation congruous with the fact that the chromatin is as near to as, and indeed nearer than, a nerve-ending to any minute structure which it stimulates.[1]

Several confirmatory facts may be named. During the intervals between cell-fissions, when growth and the usual cell-activities are being carried on, the chromatin is dispersed throughout the nucleus into an irregular network: thus greatly increasing the surface of contact between its substance and the substances in which it is imbedded. As has been remarked, this wide distribution furthers metabolism—a metabolism which in this case has, as we infer, the function of generating, not special matters but special motions. Moreover, just as the wave of disturbance a nerve carries produces an effect which is determined, not by anything which is peculiar in itself, but by the peculiar nature of the organ to which it is carried—muscular, glandular or other; so here, the waves diffused from the chromatin do not determine the kinds of changes in the cytoplasm, but simply excite it: its particular activities, whether of movement, absorption, or structural excretion, being determined by its constitution. And then, further, we observe a parallelism between the metabolic changes in the two cases; for, on the one hand, "diminished staining capacity of the chromatin [implying a decreased amount of phosphorus, which gives the staining capacity] occurs during a period of intense constructive activity in the cytoplasm;" and, on the other hand, in high organisms having nervous systems, the intensity of nervous action is measured by the excretion of phosphates—by the using up of the phosphorus contained in nerve-cells.

For thus interpreting the respective functions of chromatin and cytoplasm, yet a further reason may be given. One of the earliest general steps in the evolution of the *Metazoa*, is the differentiation of parts which act from parts which make them act. The *Hydrozoa* show us this. In the hydroid stage there are no specialized contractile organs: these are but incipient: individual ectoderm cells have muscular processes. Nor is there any "special aggregation of nerve-cells." If any stimulating units exist they are scattered. But in the *Medusa*-stage nerve-matter is collected into a ring round the edge of the umbrella. That is to say, in the undeveloped form such motor action as occurs is not effected by a specialised part

[1] While the proof was in my hands there was published in *Science Progress* an essay by Dr T. G. Brodie on "The Phosphorus-containing Substances of the Cell." In this essay it is pointed out that "nucleic acid is particularly characterised by its instability. . . . In the process of purification it is extremely liable to decompose, with the result that it loses a considerable part of its phosphorus. In the second place it is most easily split up in another manner in which it loses a considerable part of its nitrogen. . . . To avoid the latter source of error he (Miescher) found that it was necessary to keep the temperature of all solutions down to 0°C. the whole time." These facts tend strongly to verify the hypothesis that the nucleus is a source of perpetual molecular disturbance—not a regulating centre but a stimulating centre.

which excites another part; but in the developed form a differentiation of the two has taken place. All higher types exhibit this differentiation. Be it muscle or gland or other operating organ, the cause of its activity lies not in itself but in a nervous agent, local or central, with which it is connected. Hence, then, there is congruity between the above interpretation and certain general truths displayed by animal organisation at large. We may infer that in a way parallel to that just indicated, cell-evolution was, under one of its aspects, a change from a stage in which the exciting substance and the substance excited were mingled with approximate uniformity, to a stage in which the exciting substance was gathered together into the nucleus and finally into the chromosomes: leaving behind the substance excited, now distinguished as cytoplasm.

Some further general aspects of the phenomena appear to be in harmony with this interpretation. Let us glance at them.

There is good reason for concluding that in the animal organism nitrogenous substances play the part of decomposing agents to the carbo-hydrates—that the molecular disturbance set up by the collapse of a proteid molecule destroys the equilibrium of sundry adjacent carbo-hydrate molecules, and causes that evolution of energy which accompanies their fall into molecules of simpler compounds. Here, if the foregoing argument is valid, we may conclude that this highly complex phosphorised compound which chromatin contains plays the same part to the adjacent nitrogenous compounds as these play to the carbo-hydrates. It may be pointed out that in animal organisms the various structures are so arranged that evolution of a small amount of energy in one, sets up evolution of a larger amount of energy in another; and often this multiplied energy undergoes a second multiplication of like kind. If this view is tenable, we may now suspect that this method displayed in the structures of the *Metazoa* was initiated in the structures of the *Protozoa*, and consequently characterises those homologues of them which compose the *Metazoa*.

When contemplated from the suggested point of view, karyokinesis appears to be not wholly incomprehensible. For if the chromatin yields the energy which initiates changes throughout the rest of the cell, we may see why there eventually arises a process for exact halving of the chromatin in a mother-cell between two daughter-cells. To make clear the reason, let us suppose the portioning out of the chromatin leaves one of the two with a sensibly smaller amount than the other. What must result? Its source of activity being relatively less, its rate of growth and its energy of action will be less. If a protozoon, the weaker progeny arising by division of it will originate an inferior stirp, unable to compete successfully with that arising from the sister-cell endowed with a larger portion of chromatin. By continual elimination of the varieties which produce unequal halving, necessarily at a disadvantage if a moiety of their members tend continually to disappear, there will be established a variety in which the halving is exact: the character of this variety being such that all its members aid the permanent multiplication of the species. If, again, the case is that of a metazoon, there will be the same eventual result. An animal or plant, in which the chromatin is unequally divided among the cells, must have tissues of uncertain formation. Assume that an organ has, by survival of the fittest, been adjusted in the proportions and qualities of its parts to a given function. If the multiplying protoplasts, instead of taking equal portions of chromatin, have some of them smaller portions, the parts of the organ formed of these, developing less rapidly and having inferior energies, will throw the organ out of adjustment, and the individual will suffer in the struggle for life. That is to say, irregular division of the chromatin will introduce a deranging factor, and natural selection will weed out individuals in which it occurs. Of course no interpretation is thus yielded of the special process known as karyokinesis. Probably other modes of equal division might have arisen. Here the argument implies merely that the tendency of evolution is to establish *some* mode. In verification of the view that equal division arises from the cause named, it is pointed out to me that amitosis, which is a negation of mitosis or karyokinesis, occurs in transitory tissues or diseased tissues, or where degeneracy is going on.

But how does all this consist with the conclusion that the chromatin conveys hereditary traits—that it is the vehicle in which the constitutional structure, primarily of the species, and secondarily of recent ancestors and parents, is represented? To this question there seems to be no definite answer. We may say only that this second function is not necessarily in conflict with the first. While the unstable units of chromatin, ever undergoing changes, diffuse energy around, they may also be units which, under the conditions furnished by fertilization, gravitate towards the organization of the species. Possibly it may be that the complex combination of proteids, common to chromatin and cytoplasm, is that part in which the constitutional characters inhere; while the phosphorised component falling from its unstable union and decomposing, evolves the energy which, ordinarily the cause of changes, now excites the more active changes following fertilisation. This suggestion harmonises with the fact that the fertilising substance which in animals constitutes the head of the spermatozoon, and in plants that of the spermatozoid or antherozoid, is distinguished from the other agents concerned by having the highest proportion of the phosphorised element; and also harmonises with the fact that the extremely active changes set up by fertilisation are accompanied by decrease of this phosphorised element. It is useless to speculate, however. We can only say that the two functions of the chromatin do not apparently exclude one another; but that the general activity which originates from it may be but a lower phase of that special activity caused by fertilisation.[1]

Before fertilisation there occurs in the ovum an incidental process of a strange kind—"strange" because it is a collateral change taking no part in subsequent changes. I refer to the production and extrusion of the "polar bodies." It is recognised that the formation of each is analogous to cell-formation in general; though process and product are both dwarfed. Apart from any ascribed meaning, the fact itself is clear. There is an abortive cell-formation. Abortiveness is seen firstly in the diminutive size of the separated body or cell, and secondly in the deficient number of its chromosomes: a corresponding deficiency being displayed in the group of chromosomes remaining in the egg—remaining, that is (on the hypothesis here to be suggested), in the sister-cell, supposing the polar body to be an aborted cell. It is currently assumed that the end to be achieved by thus extruding part of the chromosomes, is to reduce the remainder to half the number characterising the species; so that when, to this group in the germ-cell, the sperm-cell brings a similarly-reduced group, union of the two shall bring the chromosomes to the normal number. I venture to suggest another interpretation more congruous with the ordinary course of Nature, namely, that gamogenesis begins when agamogenesis is being arrested by unfavourable conditions, and that the failing agamogenesis initiates the gamogenesis. Of numerous illustrations to be presently given, I will, to make clear the conception, name only one—the formation of fructifying organs in plants at times when, and in places where, shoots are falling off in vigour and leaves in size. Here the successive foliar organs, decreasingly fitted alike in quality and dimensions for carrying on their normal lives, show us an approaching cessation of asexual multiplication, ending in the aborted individuals we call stamens; and the fact that

[1] The writing of the above section reminded me of certain allied views which I ventured to suggest nearly fifty years ago. They are contained in the *Westminster Review* for April, 1852, in an article entitled "A Theory of Population deduced from the General Law of Animal Fertility." It is there suggested that the "spermatozoon is essentially a neural element, and the ovum essentially a haemal element," or, as otherwise stated, that the "sperm-cell is co-ordinating matter and the germ-cell matter to be co-ordinated" (pp. 490-493). And along with this proposition there is given some chemical evidence tending to support it. Now if, in place of "neural" and "haemal," we say—the element that is most highly phosphorised and the element that is phosphorised in a much smaller degree; or if, in place of co-ordinating matter and matter to be co-ordinated, we say—the matter which initiates action and the matter which is made to act; there is disclosed a kinship between this early view and the view just set forth.

sudden increase of nutrition while gamogenesis is being thus initiated, causes resumption of agamogenesis, shows that the gamogenesis is consequent upon the failing agamogenesis. See then the parallel. On going back from multicellular organisms to unicellular organisms (or those homologues of them which form the reproductive agents in multicellular organisms), we find the same law hold. The polar bodies are aborted cells, indicating that asexual multiplication can no longer go on, and that the conditions leading to sexual multiplication have arisen. If this be so, decrease in the chromatin becomes an initial cause of the change instead of an accompanying incident; and we need no longer assume that a quantity of precious matter is lost, not by passive incapacity, but by active expulsion. Another anomaly disappears. If from the germ-cell there takes place this extrusion of superfluous chromatin, the implication would seem to be that a parallel extrusion takes place from the sperm-cell. But this is not true. In the sperm-cell there occurs just that failure in the production of chromatin which, according to the hypothesis above sketched out, is to be expected; for, in the process of cell-multiplication, the cells which become spermatozoa are *left* with half the number of chromosomes possessed by preceding cells: there is actually that impoverishment and declining vigour here suggested as the antecedent of fertilisation. It needs only to imagine the ovum and the polar body to be alike in size, to see the parallelism; and to see that obscuration of it arises simply from the accumulation of cytoplasm in the ovum.

HERBERT SPENCER.

II

The Study of Variations

A Reply

"THE DIRECT ACTION OF THE CONDITIONS OF LIFE . . . IS A TOTALLY DISTINCT CONSIDERATION FROM THE EFFECTS OF NATURAL SELECTION; FOR NATURAL SELECTION . . . HAS NO RELATION WHATEVER TO THE PRIMARY CAUSE OF ANY MODIFICATION OF STRUCTURE." [1]

IF all writers on this subject would or could carry this passage from Darwin's writings branded in their minds, there would be no confusion in any question upon the Origin of Species. It is when Neo-Darwinians and others speak of Natural Selection either as a "cause," direct or indirect, of variation, in any sense of the word, or even, as Darwin said, an "aid" or "means," misconceptions will arise.

I do not know who Mr J. Lionel Tayler may be, but he has elected "to base his argument on [my] position." He is good enough to say that he does not "doubt the facts adduced, or even . . . assert that the conclusions are incorrect . . . but that . . . the arguments drawn from the facts do not . . . prove the position taken up."

I do not quite see, if the conclusions be correct, how the argument can be faulty!

He quotes my five summaries and proposes "to deal with the last conclusion first," viz.—"A new variety, and thence a new species, would be produced 'without the aid of Natural Selection' (Darwin)."

He says:—"It is still necessary to show not merely that all are similarly modified, but also that they are all equally thoroughly so, otherwise the variation that is most adapted will probably, or at least possibly, be selected, and Natural Selection will thus become a factor of some importance."

This passage shows incontestably that Mr Tayler has not yet grasped the purport of Neo-Lamarckism, which is precisely that embodied in the words of Darwin quoted, or the discovery of "the primary cause of modifications." There is no necessity whatever for all the individuals to be equally, though they be all similarly, modified. Natural Selection need not enter so timidly as he imagines, but may boldly kill off as many as it pleases, and thus "become a factor of some importance"; but this has nothing whatever to do with the primary cause of the origination of the definite

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

variations. These being now known, it is at once seen that Natural Selection plays no part at all in causing them.

If I sow a quantity of seed of the wild carrot, radish or parsnip in a rich garden soil, and at the end of the season select and pull up all the individuals with roots having the same size as, or less than one-half larger in diameter than those of plants growing wild, leaving all with roots at least half as large again as the wild ones,—what has my selection and rejection of the former to do with the cause of the swelling of the roots of those individuals I consider to be the fittest to survive?

This, it appears to me, would pretty accurately illustrate Mr Tayler's and Neo-Darwinians' arguments.

Again, he says, "it is apparently assumed by Henslow that variations, being always definite, upset the Darwinian position." Such, at least, was the opinion of Romanes, who wrote me as follows:—"Of course, if you could prove that indiscriminate variations have not occurred in wild plants . . . you would destroy Darwinism *in toto*." No one can be called upon "to prove a negative," but I am told by many that my books do prove the contrary, that variations are definite. Darwinism is, of course, absolutely based on the supposed existence of indefinite variations, otherwise there would be nothing for Natural Selection to do for it.

Mr Tayler alludes to a prevalent "contempt for theory." I am not aware of such; but there undoubtedly exists a strong tendency to reject *à priori* assumptions, offered without a particle of fact. Thus, Mr Tayler writes:—"Assume that at any period, however remote, variations were completely indefinite," &c.; and then follows a paragraph full of assumptions containing the words "assume" twice, "suppose" once, "would" five times, "might" twice. It is, therefore, needless to follow him, as the paragraph consists solely of *à priori* statements (but no theory), for which he offers no basis of facts whatever.

The result of this argumentation on indefinite variations is a strange one, viz., that "Definite variations would be precisely what on *à priori* grounds would be expected."

If this be so, how was it that neither Darwin nor Dr Wallace ever expected them? Darwin admits them, but puts them on one side as rare or insignificant; while Dr Wallace said he did not even understand the meaning of the terms "definite" and "indefinite," until I pointed out that they were not mine but Darwin's own, and that the whole of Darwin's theory is based on the imaginary—unproven—assumption of plants always varying indefinitely in nature.

Darwin has really blended together two quite distinct things:—(1) The origin of actual variations in organisms, as they occur in nature; a physiological work which can be seen going on slowly or quickly in nature, and more easily under cultivation; (2) the subjective idea of a "species," which is only a mental creation, of which Nature knows nothing.

A systematist calls slight variations from a given type "varieties," and as long as they are linked (and there are not too many of them) in a series, a_1 to a_6, say, a_6 is often regarded as much a variety as a_1; but if a_2 to a_5 be wanting, then he chooses to call a_6 a "species"; but his naming it as such makes no difference as to the origin of it. If Natural Selection has killed off a_2 to a_5, a_6 existed long before Natural Selection came on the scene, call a_6 what you please.

Natural Selection thus applied only separates varieties, and makes them more distinct for the benefit of the classifier.

With all this Neo-Lamarckism has nothing whatever to do. It is only concerned with tracing out the causes which originate or bring about the variations themselves. These are now palpably plain to all who can or will open their eyes bodily and mentally.[1]

I could criticise a good deal more in Mr Tayler's paper, but will only refer to the conclusion.

Mr Tayler proposes a new kind of Classification of Variations, viz.:—

Unadaptive Variations, *i.e.*, "Wholly unfitted for their environment."

Indefinite Variations, *i.e.*, "Varying in all directions round a central position."

Definite Variations, *i.e.*, "All variations being of selective value, but that value varying in degree."

Adaptive Variations, *i.e.*, "No selection, except from general strength."

In this last I seem to recognise my own position: and all I would ask Mr Tayler to do is to supply, say, half-a-dozen examples of plants and animals, living in a wild state, which he can place within the first three groups respectively. If he can do so—or he may reduce the number to three each, if he likes—then one may credit him with theorising; if he cannot, then the above remain as valueless *à priori* assumptions.

Until he and other modern Darwinians can see—with Darwin himself—that natural selection has nothing whatever to do with the Causes of Variations, but only with the distribution of plants and animals, in space and time, like Mr Tayler, I too "fail to see the use of continuing this discussion as it now stands" in his paper on The Study of Variations.

GEORGE HENSLOW.

[1] It is a great gratification to find, that while I have been busy with these questions in England, Mr J. Costantin has been studying them simultaneously on the Continent. His new work, "Les Végétaux et les Milieux Cosmiques," consisting of 280 pp. 8vo, embraces a great deal of what I have given in my books; but he also adds a great deal which I have not touched upon. His conclusions however, are absolutely identical with my own.

III

A New Method of Asexual Reproduction in Hymenopterous Insects[1]

ASEXUAL reproduction may manifest itself among insects at different stages in their life-history. Sometimes it is the larvae which reproduce new larvae by budding in the interior of their body (paedogenesis); at other times it is the adults which give birth to new individuals which develop in the ovaries of the parents (parthenogenesis). We have just discovered in the parasitic Hymenoptera a new mode of reproduction which completes this series of phenomena, of which it constitutes to some extent the first step; in *Encyrtus fuscicollis* which we have observed, it is in fact at the very beginning of the life-history, in the egg itself, that the dissociation of the body is produced, and it is at the expense of a single egg that we have just seen a very large number of embryos arise, perhaps more than one hundred, and all destined to become perfect insects which will be for the most part of one and the same sex.

Mr E. Bugnion had already observed that the caterpillars of the *Hyponomeuta* of the spindle-tree may, during June, contain chains of very curious parasitic embryos. These chains, of which only a single example is usually found in each caterpillar host, are formed on an average of from fifty to a hundred individuals arranged one after the other, enclosed in a granular mass similar to a vitellus and surrounded by a common long epithelial tube which is closed at both ends and floats in the lymph of the caterpillar by the side of the digestive tube. Bugnion followed the development of these embryos and observed that each of them gave birth to an *Encyrtus fuscicollis*. How and where does the *Encyrtus* accomplish its oviposition? What especially are the origin and significance of the common epithelial tube enveloping the chain of embryos? These are questions well adapted to excite the curiosity of the naturalist. Mr Bugnion thought that the *Encyrtus*, hatched in the spring, hibernated or exhibited a second generation with an unknown animal as its host; he assumed that, in any case, it must lay its eggs in packets during the month of May inside the caterpillar of the *Hyponomeuta*; as to the epithelial tube, it arose, according to him, from the enveloping

[1] Translated from a reprint from the *Comptes Rendus* of the Paris Academy of Sciences, Feb. 28, 1898, communicated by the Author.

membrane of the embryos secondarily separated from these latter and fused end to end. These deductions, although very plausible, are not really in accordance with the facts.

I have observed the oviposition of *Encyrtus fuscicollis*. Now, it does not lay its eggs in the month of May, but in the month of July only a few days after being hatched; moreover, its eggs are not deposited in the caterpillar but actually in the egg of the *Hyponomeuta* itself. The diminutive Chalcidian settles on a mass of eggs fixed for some hours, successively stroking with its ovipositor all or almost all the eggs in contact with it. I reserve the details of this operation, which I have observed at length, for a forthcoming memoir. I will only remark now, that the time occupied by the *Encyrtus* in laying its egg in that of the *Hyponomeuta* varies from half a minute to two minutes; almost as soon afterwards there follows another egg of the same laying, and so on for hours; then, when it has finished, it proceeds to another mass of eggs and begins the same manipulations again.

One important fact results from the foregoing observation. Given a limited quantity of eggs in the ovaries of an *Encyrtus*, it is practically impossible that, in the short time necessary for completing its oviposition, it should deposit in each moth's egg a number of eggs equal to that of the embryos composing one of the chains to which we have referred. A single egg must thus be laid in the egg of the *Hyponomeuta* and this single egg must split up into a great number of embryos.

This inference has been proved by direct observation. I have watched at the beginning of the evolution of the egg, and I have ascertained that from the first its enveloping membrane is constituted like that of other known Chalcidians; afterwards its cells multiply rapidly, and it lengthens in a way to form the epithelial tube. With regard to the cells which are found in the interior of the enveloping membrane, instead of resolving into a single embryo, as is the usual case, they dissociate in a way to give rise to quite a legion of little *morulae*, which later become embryos and arrange themselves in rows, as the envelope, increasing all the time, passes from the primitive vesicular form to that of a long flexuous tube. The whole product of the segmentation, however, is not devoted to the formation of embryos; from the beginning a cellular mass in the form of a crescent is seen on the periphery; this gradually increases in size and dissociates probably to form the granular mass which fills the enveloping tube and unites the embryos.

From the preceding observation therefore, there results the discovery, among the Arthropoda, of an entirely new method of reproduction to which I believe it is difficult to find anything analogous among the Metazoa. How then can this curious case of metagenesis be interpreted? Can the tube containing the embryos be considered as the parent, of which the soma may be represented by the epithelial tube, and by the internal cells which do not take part in the formation of the embryos? One cannot help thinking of the cysticercans and orthonectids; but such comparisons may be rash at present. We prefer to confine ourselves to the facts, waiting for their general interpretation until the observations which we are pursuing on different species furnish us with more copious data.

PAUL MARCHAL.

IV

The Smallest of Stridulating Spiders

MANY readers of *Natural Science* will doubtless remember Mr Pocock's interesting paper, published some three years ago,[1] on "Musical Boxes in Spiders." In that paper Mr Pocock summarised our knowledge of the various organs which, in spiders, are specially adapted for producing sound. In all cases the sound or stridulation is due to the scraping of sharp spines on roughened surfaces, or on a series of ridges, or on stiff, thickened hairs; but the stridulating-organ may be developed in very different parts of the spider's body. For example, we may find:—

1. "Westring's organ"—A semi-circle of teeth on the forward part of the abdomen, scraping a set of ridges on the hinder part of the carapace. (Males of *Asagena*, *Pedanostethus*, and some other Theridiidae.)
2. "Campbell's organ"—A set of ridges on the femur of the palp, scraping a similar set on the outer surface of the mandible. (Males of *Leptyphantes*; imperfect in female.)
3. "Simon's organ"—A set of spines on the femur of the palp, scraping a set of ridges on the outer surface of the mandible. (Both sexes of *Thomisoides*.) A similar organ, in which, however, the femur of the palp bears only a single strong spine, has been since discovered in both sexes of *Scytodes* by Mr F. Pickard-Cambridge.[2]
4. "Wood-Mason's organ"—A set of stout spines on the mandible, scraping a series of club-like rods (thickened hairs) on the coxa (basal segment) of the palp. (Both sexes of *Poecilotheria*, *Selenocosmia*, &c.) This organ has recently been re-discovered by Mr Pocock (it had been described in 1867 by Blackwall, who was, however, ignorant of its purpose) in the South American *Trechona zebrata*.[3] Mr Pocock has also found the organ in the Australian *Idiommata blackwallii*,[4] while it has been described and figured in another Australian species—*Phlogius crassipes*, by Professor Baldwin Spencer.[5] A closely similar organ is stated by Mr Pocock to occur in species of the Ethiopian

[1] Vol. vi., 1895, pp. 44-50.
[2] *Ann. Mag. Nat. Hist.* (6), vol. xvi., p. 371.
[3] *Ann. Mag. Nat. Hist.* (6), vol. xvii., pp. 177-9.
[4] *Op. cit.* (6), vol. xvi., p. 225.
[5] *Rep. Horn Exped.*, vol. ii.; *Zoology*, pp. 412-14. [See *Nature*, vol. li., p. 438.]

genus *Harpactira*.[1] He believes, therefore, that it must have been independently developed in various groups of the Aviculariid spiders.

5. "Pocock's organ"—A set of spines on the coxa of the palp, scraping a series of club-like rods on the mandible. (Both sexes of *Phormingochilus*, &c.)

Since Mr Pocock published his paper in *Natural Science*, he has described two new stridulating organs in spiders. It may be of interest to summarise their structure:—

6. A large heart-shaped tooth on the anterior sclerite of the pedicel (narrow "stalk" between carapace and abdomen) scrapes a set of six horny, arched ridges situated in a cave-like hollow beneath the base of the abdomen. (Males of the New Zealand agelenid *Cambridgea antipodiana*.[2])
7. A set of spines on the hinder surface of the coxa of the palp plays on a series of modified hairs on the front surface of the coxa and trochanter of the first walking-leg. (Both sexes of *Eumenophorus* and allied Ethiopian aviculariids.[3])

Having briefly reviewed our knowledge of these organs, it is now my privilege to give an account of a very interesting "musical box," whose construction differs widely from that of any of the above:—

8. The inner hinder corner of the coxa in the fourth pair of legs is drawn out into a sharp point, scraping over the surface of the "lung-book" cover, which is traversed by a complicated series of ridges and furrows. (Male of *Entelecara broccha*.)

The spiders on whose sound-producing organs Mr Pocock has specially worked, are the Aviculariidae, some of the largest members of the order. The species in which I have observed the organ whose structure is summarised above, is, on the other hand, one of the tiniest of spiders; measuring only about 1½ mm. in length. For some years past I have been working up the arachnid fauna of Ireland, and a number of friends have been good enough to collect material for me in their rambles in different parts of the country. One of the most energetic of these is Mr R. Welch, of Belfast (whose geological photographs must be familiar to many readers of *Natural Science*). And it was he who found in October last, on the summit of Slieve Donard, the highest point of the Mourne Mountains, some 2790 feet above sea-level, the little

1 *Proc. Zool. Soc., Lond.*, 1897, p. 771.
2 *Ann. Mag. Nat. Hist.* (6), vol. xvi., p. 230.
3 *Proc. Zool. Soc., Lond.*, 1897, p. 744.

spider which is provided with so interesting an organ of stridulation.

The spider is an addition to the Britannic fauna. It appears to be identical with a species described by Dr L. Koch from the Tyrolese mountains, under the name of *Erigone broccha,* and subsequently discovered near the Great St Bernard by Mr Simon, who [1] placed it in his genus *Styloctetor.* In his most recent revision of the spiders of this large and very difficult sub-family (Erigoninae), however, Mr Simon is disposed to reduce the number of genera, and now includes the species of *Styloctetor* in the large and comprehensive genus *Entelecara.*[2] We will refer to the spider therefore as *Entelecara broccha* (L. Koch). It is evidently a scarce alpine species, but there can be little doubt that it awaits discovery on other hills in our islands.

Probably the difficulty in determining this, to me, unknown spider led to the discovery of its stridulating organ, on account of the care-

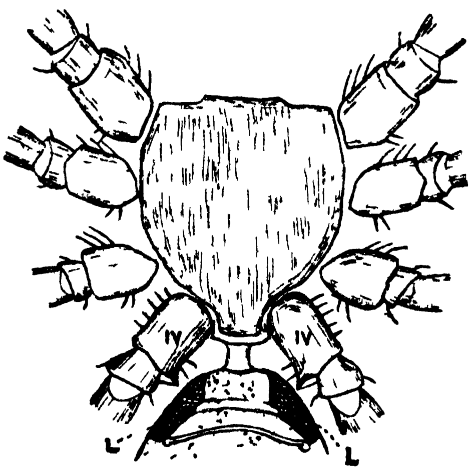

Fig. 1.

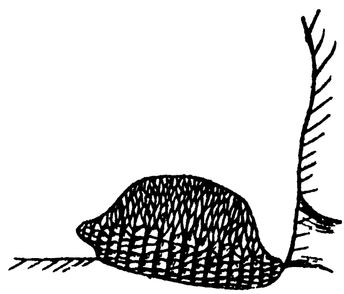

Fig. 2.

Fig. 1. *Entelecara broccha.*—Sternum and front portion of abdomen seen from beneath, showing the basal joints of the legs. The coxa (IV.) of the fourth pair produced inwardly into sharp spines which play over the ridged surfaces of the lung-book covers (L). Magnified.

Fig. 2.—Right lung-book cover seen from side, more highly magnified, showing parallel series of coarse ridges with transverse fine ridges (below), and irregular network of fine ridges (above).

ful examination of its structure which had to be made. When viewing the spider from beneath, the haunches of the fourth pair of legs were seen to differ markedly from those of the other pairs in the possession of a sharp point at the inner hinder angle (fig. 1). Such a sharp point is suggestive of a stridulating organ, but in this

[1] "Les Arachnides de France." Tome V. Paris, 1884.
[2] "Histoire Naturelle des Araignées." 2me edition. Tome I. Paris 1892.

position it could only play over some part of the hind-body or abdomen. Now, it is well known that in most spiders the hind-body is soft-skinned. The breathing organs or "lung-books," however, situated one on either side of the front end of the hind-body, are always covered by plates which are somewhat hardened. And microscopical examination soon showed that in the present spider these lung-book covers are completely chitinised, and that their surface is provided with a beautiful and complex arrangement of ridges and furrows. With the analogy of the stridulating organs of other spiders before us, there can be no doubt that the purpose of this apparatus is to produce sounds.

A more highly magnified view of a lung-book cover from the side is shown in fig. 2; and it will be seen that in the lower (ventral) region of the plate the ridges are somewhat coarse, and parallel, with numerous finer transverse ridges between them. But in the upper (lateral) region the ridges are all fine, and form an irregular network. It is likely that the note produced is relatively high or low, according as the fine or coarse ridges are brought into friction with the spine on the basal segment of the leg. Or it is possible that the irregular network may represent the primitive generalised sculpture of the surface of the lung-plate, and that only the parallel series of ridges are concerned in the production of sound.

Unfortunately, only a single male was obtained, so that it is impossible to say whether this organ is present also in the female. Its function is almost certainly to produce a sexual "call-note" or "love-song," for the apparatus is so minute that the pitch must be too high for the sound to be of use as a "warning-note." Many problems of interest are presented to the student of spiders by these stridulating organs. It is remarkable in how few forms comparatively they are present; and these by their structure and distribution are mostly seen to be ancient and decadent groups. How is it that species possessed of such wonderful and complex structures have been, to a great extent, thrust aside in the battle of life by rivals which have no such organs? GEO. H. CARPENTER.

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V

Parasites and Mess-mates in Ant-hills[1]

THE nests of ants, by their well-chosen situation, the great accommodation they offer, and the security they provide, for obvious reasons attract a certain number of animals which can in reality live perfectly well elsewhere. It is always necessary, in order that these animals may thus live associated with the ants, that they should not constitute a favourite food (*Porcellio*, *Lucasius*), or that they should possess some mechanical protection (*Glomeris*), or offensive glands as the myriopod *Blaniulus*, or secret glands like the hemipteron *Dictyonota*, or that they should be able to escape the ants by their small size or their agility (Thysanura).

The warmth in the nests, even during the hibernation of the colony, helps to attract a certain number of animals. It is no doubt on account of the warmth thrown off by the ants that one finds in Siberia, in the nests of *Formica rufa*, the same species of beetles as in Europe. *Cetonia floricola*, whose larva lives in the deeper parts of the nest of *Formica rufa*, spins its cocoon in the upper part of the dome in order to gain, during its nymph-stage, the heat which is there generated by the heat of the sun. *Quedius brevis*, which, during the fine season, feeds on *F. rufa* or *Lasius fuliginosus*, but which is compelled to hide itself to escape their pursuit, installs itself during the winter in the middle of the mass of torpid ants.

The vegetable nourishment to be found in ants' nests explains the presence of some animals, as, for instance, that of some Isopods. The woody material of which *F. rufa* and related races construct their nests serves as nutriment to certain caterpillars of micro-lepidoptera (*Myrmecocela ochraccella*). The same applies to the larvae of certain Coleoptera which live in the nests of ants, as, for example, the larvae of *Cetonia* or those of *Clytra*, which, utilising their excretions for the manufacture of a kind of cardboard, make of it protective sheaths for themselves.

But it is the animal nourishment in particular, furnished by the remains of insects devoured and by the carcases of ants themselves, which attracts to the nests a large number of myrmecophila. Such is the case with many Acarids (*Laelaps*) and Coleoptera of the

[1] Abstract of Charles Janet, "Rapports des Animaux Myrmécophiles avec des Fourmis" (Limoges: Librairie Ducourtieux, 1897. 8vo, pp. 100).

family Histeridae (*Hister*, *Hetaerius*, *Myrmetes*) and the Staphylinids *Stenus* and *Dinarda*. The progeny (eggs, larvae, and nymphs) which is sometimes so abundant in the nests, and which ants themselves, in case of scarcity, draw upon as if it were ordinary provision, can also be utilised by the myrmecophiles; wounded larvae or those bitten by ants are often devoured by the beetles *Platyarthrus claviger* or *Atemeles*.

The cloaca of the nests furnishes a good means for the development of certain Nematodes and for the nourishment of detriticolous Sarcoptides.

Generally, it is more often the assemblage of advantages, and not only one of those which are here cited, which has been the motive of introduction and definite installation of a myrmecophile in the home of a species of ant.

The advantages that a myrmecophile can find in the home of one species are not the same as can be found in the home of another species. For this reason we see *Atemeles* inhabit successively the nests of two different species: the nest of a *Myrmica* during the bad season until pairing time, then the nest of *Formica* to which they confide their progeny, and with which they themselves live to the end of their existence.

The nutritive liquid stored in the maw and worked up later with the addition of the product of the pharyngeal glands is a precious nourishment, nearly always available in abundance, and likely to be snapped up, in passing, whilst the ant disgorges for its comrades. This naturally attracts some animals. It is the case with the Lepismidae (*Lepismina polypoda*) which do not appear to be tolerated amicably by their hosts. More favoured than these Lepismids, the Acarids (*Antennophorus uhlmanni*) which support themselves on the same substance, have been accepted by the ants so far as to be permanently carried, and that in spite of their relatively large size.

It is this same liquid that is immediately and generously given from the ants to the myrmecoxies, as, for example, to *Atemeles*, which demand it by raising their anterior feet, or to *Claviger*, which strike with their antennae for it on the head of their host.

The product of the pharyngeal gland which plays an important part in the working up of the liquid, is the best nourishment for nematode larvae (*Pelodera*).

The viscous substance deposited on the surface of the eggs is perhaps used as nourishment by the nymph of *Laelaps oophilus*, which settles itself upon the packets of eggs in the nest of *Formica sanguinea* and *F. rufibarbis*.

A predisposition of ants to give to the progeny of certain myrmecophila the same care as to their own, has led to the definite maintenance in the nests of a certain number of forms. *Lomechusa* passes all its life with *Formica sanguinea*, which cares for the larvae of this beetle as much as for its own.

The search for the liquids secreted by Aphides does not usually constitute a true case of myrmecophily. But it is possible that a certain number of species of Aphidae have been so well cared for by ants, and have found such advantages from this care, that they have finished by no longer being able to live outside the ant nests, and have thus become veritable myrmecophiles.

It is probable that the search for certain coleoptera and the solicitudes with which they have been the object outside the nest, have been the origin of their myrmecoxeny (Thorictidae, Paussidae, Clavigeridae).

The presence of Aphides in the nests of ants attracts aphidophagous animals, which thus become indirectly myrmecophile (larva of the Coccinellid *Brachyacantha ursina*).

The blood of ants is sucked by Acarids (*Discopoma comata*) which perforate the abdominal membranes of their hosts, making slight wounds.

The incessant activity of all ants, the habits that certain of them have of making long journeys, have led a goodly number of myrmecophila to use them for transport.

The hairs of ants furnish to acarids who wish for transport an easy method. This is the case with *Uropoda cristiceps*, which, frequently enough, keeps itself firmly attached to one of these hairs on the extremity of the abdomen of *Formica fusca.*

The tibial comb, so important an organ to an ant, presents the interesting peculiarity of being used by an Uropode which, for this reason, bears the specific name of *philoctena.*

The stem of the antenna furnishes a convenient hold for a *Thorictus*, which crowds itself into a circular space formed by a hollowing of the epistome and by the mandibles, and is thus carried about as much in the interior of the nest as in the emigration from one district to another.

Myrmecophila, for purposes of transport, can also fix themselves upon the bodies of ants and hold on by various methods. *Claviger* fixes itself upon the back of *Lasius*, and gripping firmly with its claws, is, in case of danger, rapidly carried from the superficial to the deep galleries of the nest. An oniscid, noticed by Lund, gripped with its claws under the abdomen of *Myrmica typhlops*. *Antennophorus* remains permanently on the body of the ant, and adheres by adhesive caruncles terminating the six ambulatory feet. *Iphis equitans*, which is frequently upon the head of *Tetramorium meridionale*, does not remain permanently in that position, but descends quickly to the body and gets a fresh hold upon that. The nymphs of an Uropode near to *U. ovalis*, glue themselves with an anal mucus to the dorsal crest of the femur of the second foot of *Lasius mixtus*, in the same manner as the nymphs, and similar species, glue themselves upon other insects.

The transport of the progeny of ants is also utilised by certain myrmecophila, which are secure in fixing on to the eggs, larvae, nymphs, or cocoons, to remain near the ants and to be installed in a safe place and in the best parts of the nest. This is perhaps the case with *Laelaps oophilus*, which fixes itself on the egg packets: it is with that of *Claviger* which so frequently fixes itself upon the larvae, with *Antennophorus* which fixes sometimes upon the nymphs, with *Uropoda cristiceps* which can fix itself momentarily upon a cocoon by holding a fold closely between the coxa of its antenniform feet.

The means of defence of ants serve indirectly to protect animals which live in their nests. They serve, indirectly again, to protect those animals which mimic the ants, whether myrmecophilous or not. Myrmecoid animals can be protected by this resemblance, for example, against insectivorous birds which do not eat ants. This mimicry may be more or less pronounced. There are plenty of examples among the Arancids (*Salticus myrmicaeformis*, *Myrmecia vertebrata*), among the Orthoptera (*Myrmecophana fallax*), the Heteroptera, in which a certain number have the larvae at the same time both myrmecoid and myrmecophagous, and among the Coleoptera (*Clerus fornicatus* and *Myrmecomoea*).

Thanks to their means of defence, ants are not attacked by certain animals, or can defend themselves perfectly against them. Thus, even with animals which are powerfully armed, such as spiders, ants often come off victorious in a hand to hand conflict.

In spite, however, of the means of defence that they possess, ants by reason of causes which in other respects are a power to them, have become the normal prey of a great number of enemies. The length and frequency of their travels expose them to danger from the funnels dug in the sand by the larvae of Neuroptera (*Myrmeleon formicarius*) and by dipterous larvæ (*Lampromya miki*). They are also liable to be seized by beetle larvæ (*Cicindela*) hidden in vertical holes, and to become entangled by the antennae in the capture threads that certain spiders (*Theridion riparium*) attach to the ground. Their method of travel in single file, the hindrance in this case that is caused by the heavy burdens they carry, also exposes them to be carried off individually by spiders like *Zodarion elegans*, exactly as a man in a column of porters is sometimes carried off by a tiger. Again, at the time of egress, and especially whilst they are absorbed in the excitement of battle, ants are exposed to the attacks of certain Hymenoptera, of which the offspring are entomophagous (eggs of *Elasmosoma berolinense* are deposited in the abdomen of *Formica rufa*).

Sleep, so necessary to animals which expend so much energy, and the noctural torpor, occasioned by cold, put ants at the mercy of myrmecophagous coleoptera. One of these is *Myrmoecia fussi* of the nests of *Tapinoma erraticum*, which hides itself during the day to avoid the formidable poison of these ants, but which seizes and devours them during the night.

During their winter sleep ants are without means of defence. It is said that the bird *Picus viridis*, which supports itself chiefly on insects, digs up the nests of *Formica rufa* in the winter and eats the torpid inhabitants.

The great multitude of individuals, and the almost incessant renewal of the progeny, which contributes so much to the power of ants, allows them, on the other hand, to become an easily found prey for those animals which are stronger than themselves, or are so organised as not to suffer from their means of defence. Thus it is that ant-bears, birds, and toads destroy a considerable number of ants.

Ant larvae, in common with the larvae of so many other insects, receive the eggs of entomophagous Hymenoptera, which develop at the expense of the tissues of their victim and undergo their nymph-stage in the cocoon of the ant (*Euchoris myrmeciae*, the entomophage of *Myrmecia forficata*). The difficulty that the entomophagous Hymenoptera have in penetrating to the interior of the nests, explains the fact that the ant larvae do not more often fall victims to them.

Finally, the abundance of progeny which is to be found in ants' nests constitutes a choice of nutriment to every animal which seeks first. Thus *Solenopsis fugax*, thanks to its small size, is able to penetrate the nests of other species of ants to devour the nymphs.

Societies of ants are rendered powerful by the number of individuals that compose them; by their tenacity, courage, and complicated instincts; by the perfect means of attack and defence possessed by certain individuals; by the favourable medium and the protection that is afforded by their well-shielded retreats; and by a remarkable division of labour. On account of all these particularly advantageous circumstances, colonies of ants as a rule enjoy a long existence and a life of opulence.

VI

The History of the Mammalia in Europe and North America

THE story of the development of the races of warm-blooded quadrupeds, or Mammalia, in the northern hemisphere, during the Tertiary period, is one of the most interesting and important chapters in philosophical natural history. It is true that only the barest outlines of the subject have hitherto been revealed; but sufficient is known to arrive at some general conclusions which are not likely to be affected by future discoveries.

Mesozoic

The earliest known animals exhibiting any close approach to the Mammalia in the structure of their skeleton date back to the close of the Primary or Palaeozoic Era. They are five-toed quadrupeds, with most features which now characterise cold-blooded animals of the class of reptiles; but in several respects their skeleton resembles that of the lowest surviving mammals—the monotremes of the Australian region—while their dentition is often differentiated into cutting teeth in front (like incisors), grasping teeth at the corners (like canines), and grinding or crushing teeth at the sides (like molars). In allusion to the latter peculiarity they are usually known as Anomodontia. During the Permian and Triassic periods these animals flourished both in the northern and in the southern hemisphere; but before the dawn of the Jurassic they seem to have become extinct in all parts of the world which have hitherto been geologically examined. It is probable, indeed, that in some isolated region some of them passed into mammals about that time; for in the Jurassic rocks both of Britain and North America there are occasional remains of small mammals as large as rats, and the most plausible explanation of these is, that they were accidental escapes from some other land with a more advanced fauna, just as are the rats and mice of the present day in the comparatively antique realm of Australia. Another group of diminutive mammals of the Jurassic type occurs in the Upper Cretaceous (Laramie Formation) of North America; while quite at the base of the Tertiary Formations, both in North America and Europe, true placental mammals begin to appear, and very soon become the dominant animals of the land.

Base of Cainozoic or Tertiary

The members of this earliest Tertiary mammal fauna, so far as can be determined from fragments, are remarkably similar on the two sides of the Atlantic Ocean. Their remains are found in a lake-deposit, the so-called Puerco Formation of New Mexico, and in the Cernaysian Formation near Rheims, in France. Among them there are still some survivors of the typically Jurassic and Cretaceous mammals of the order Multituberculata—diminutive creatures, with jaws approaching those of the rodents in pattern, and with the crowns of the molar teeth bearing numerous tubercles for crushing. These mammals are now generally supposed to have had the same lowly organisation as *Ornithorhynchus* and *Echidna*; and if so they were the last survivors of the Monotremata outside the Australian region. The greater part of the Puerco and Cernaysian fauna, however, consists of small mammals with a diminutive smooth brain, which might have served very well for the ancestors not only of the modern placental mammals, but also of the lowly pouched mammals, or marsupials, now characteristic of the Australian region, and also met with in tropical America. Some of these ancestral types exhibit teeth and other features very suggestive of the lemurs, and might easily have been modified into the latter; others look a little like ancestral rodents; others are clearly at the base of the insectivorous and flesh-eating mammals of the orders Insectivora, Chiroptera, and Carnivora; while a large number are the little modified forerunners of the hoofed animals.

Eocene

The next early Tertiary mammal fauna, characteristic of the Lower Eocene period, is also nearly identical in Europe and North America. In England it has been found in the Thanet Sands, London Clay, and Woolwich and Reading Beds. In North America it occurs in lake-deposits termed the Wasatch Formation. A few diminutive typical marsupials, apparently almost identical with the opossums which still live in the forests of tropical America, occur in this fauna on both sides of the Atlantic. The ancestral hoofed animals, or Condylarthra, are now more varied and attain larger dimensions than they did previously; and some of them seem to have already evolved into odd-toed (perissodactyle) and even-toed (artiodactyle) members of the order. There are thus recognisable ancestors of the horses and the pigs. The most characteristic feature of this fauna, however, is the rise of a tribe of stout animals about as large as tapirs, with five short stumpy toes on remarkably small feet, hence named Amblypoda or Amblydactyla. *Coryphodon* represents this tribe both in Europe and North America.

It is harmless, exhibits a ridiculously small brain-cavity, and has a complete series of teeth adapted for feeding upon succulent vegetation. With the hoofed mammals, there are also the primitive smooth-brained flesh-eaters again, or Creodonta, as they are termed. There are likewise true rodents and undoubted examples of lemurs.

In the next, or Middle Eocene period, the lowly organised but overgrown Amblypoda seem to have become extinct in Europe; but they have assumed still larger proportions and fantastic shapes in North America. Here, at any rate in a limited area, they are now terribly horned animals, or Dinocerata, with three pairs of bony bosses on the top of the head, and a great pair of tusks hanging downwards from the upper jaw. They seem to have reached the maximum development for animals with such small brain-capacity, so they immediately become extinct. The true odd-toed and even-toed hoofed animals in which the brain gradually enlarges and begins to show complications, are much more flourishing and quite a conspicuous race. In North America, indeed, one family (Titanotheriidae) arises and soon includes animals as large as small elephants. The Creodonta are also more varied than before, and flying mammals or bats are now completely formed.

The Upper Eocene fauna is still more advanced, and the differences between the mammals of the Old and New Worlds begin to be more and more marked. The Creodonta are now of less importance than before, and true Carnivora much like modern dogs are met with for the first time at least in Europe (*Cynodictis*). The ancestral hoofed animals, or Condylarthra, are also few. The odd-toed hoofed animals, or Perissodactyla, are still numerous, and the most important new form is *Palaeotherium* in Europe; but the Artiodactyla attain the most remarkable development both in Europe and North America. On the former continent there are the pig-like *Chocropotamus*, and the primitive forerunners of the ruminants, *Hyopotamus* (*Ancodus*) and *Xiphodon*; while a remarkable short-lived family exhibiting three-toed spreading feet is represented by *Anoplotherium* and *Diplobune*. In America there are the Oreodonts (*Protoreodon*) and primitive camels (*Leptotragulus*), which must have been still more nearly ruminants. The Rodentia are now very numerous and varied; on the other hand lemurs appear for the last time on both continents. The opossums (*Didelphys*) are still abundant. Marine mammals occur for the first time, the primitive whale, *Zeuglodon*, being found in Europe, Northern Africa, and North America; while a single Cetacean vertebra of a more modern type has been discovered in the Upper Eocene of Hampshire. There is also some fragmentary evidence of Sirenia in northern Italy.

It must be added that the European Upper Eocene mammalian fauna is best represented in certain phosphate-bearing earths or phosphorites filling an extensive series of fissures in the district of Quercy, between Villefranche and Montauban, in the south of France. These fissures, however, were evidently open and being filled with earth and bones long after the close of the Eocene period, so that two or more successive faunas are mixed together, and it is not possible to determine with certainty the age of any particular fossil found in this anomalous deposit. A similar mixture of Eocene and Miocene mammalian fossils, though of a more fragmentary character, also occurs in several fissure-accumulations of iron-ore (the so-called bohnerz) in Switzerland (as at Egerkingen), in Würtemberg (as at Frohnstetten), and in Bavaria (as at Pappenheim).

Miocene

Between the Eocene and Miocene strata of Europe, stratigraphical geologists who base their conclusions on the marine deposits, recognise an intermediate formation termed Oligocene. So far as the vertebrate faunas are concerned, however, it does not seem possible to admit this division, and the so-called Lower Oligocene falls more naturally into the Upper Eocene, while the Upper Oligocene may be included in the Lower Miocene.

Adopting this arrangement, the Lower Miocene vertebrate fauna of Europe occurs in the Hempstead Beds which directly overlie the Upper Eocene in the Isle of Wight; in the lacustrine marl of Ronzon, near Puy-en-Velay, and of other districts in southern France; in other freshwater deposits near St Gérand-le-Puy, Allier; in the lignites of Rott, near Bonn, of La Rochette, near Lausanne, and of Cadibona, in Liguria; in the marine Rupelian formation of Belgium; in marine, brackish-water, and freshwater deposits in the neighbourhood of Mayence; and in other freshwater formations near Ulm, Würtemberg. A corresponding mammalian fauna in North America occurs in the White River Formation, which was deposited in an extensive series of lakes spread over Nebraska, Dakota, Colorado, Wyoming, and part of southern Canada. The Creodonta are now found for the last time both in Europe and North America, and seem to be represented only by one highly specialised genus, *Hyaenodon*. The true Carnivora are abundant and varied, including even a cat (*Eusmilus*) on both continents; but the viverroids are confined to Europe. Among hoofed animals the Perissodactyla include hornless rhinoceroses on both continents; while there is a considerable advance in the line of the horses in North America, and *Titanotherium* represents the highest and last development of the peculiarly American family of Titanotheriidae. Some of the primitive Artiodactyla attain a large size and become of importance, such as *Anthracotherium*, *Hyopotamus*, and *Elotherium*, both in Europe and America. The early ruminants are also represented in Europe by such genera as *Caenotherium* and *Amphitragulus*; while they occur in still greater variety in North America as Oreodonts (*Oreodon* and *Agriochoerus*), primitive camels (*Poebrotherium*), and strangely horned quadrupeds (*Protoceras*). The opossum (*Didelphys*) appears for the last time in Europe; so also do the modern American freshwater ganoid fishes, *Amia* and *Lepidosteus*. Among marine mammals, *Halitherium* and *Miosiren*, from the neighbourhood of Mayence and Antwerp respectively, are the earliest well-known examples of the Sirenia.

The Middle Miocene fauna is well known from numerous localities in Europe, and of these some of the more classical may be enumerated as follows: Sansan, Simorre, and Villefranche d'Astarac, in Gers, France; Léognan and Romans, Gironde; St Gaudens, Haute Garonne; La Grive-St-Alban, Isère; Montpellier, Hérault; Käpfnach, Chauxdefonds, and Elgg, in Switzerland; Georgensgmünd and Steinheim, in Würtemberg; and Monte Bamboli, in Tuscany. In North America the John Day Formation of Oregon and Montana is supposed to furnish a nearly equivalent, though distinctly more ancient assemblage of mammals. The most remarkable feature of this fauna in Europe is the sudden appearance of the elephants or Proboscidea (*Dinotherium* and *Mastodon*) and the true apes (*Oreopithecus*, *Dryopithecus*). It is also interesting as comprising the earliest known deer with antlers (*Dicrocerus*), and the earliest rhinoceroses with a horn. The Carnivora are noteworthy as including a series of genera intermediate between the dogs and bears (*Amphicyon*, *Hyaenarctos*). In North America there are not as yet any traces of the Proboscidea, or of deer with antlers; while the chief Carnivora of the John Day Formation are the sabre-toothed cats (*Nimravus*).

There is no clearly distinguishable Upper Miocene mammalian fauna in Europe, though the lacustrine deposits of Oeningen, Baden, may perhaps represent this stage. In North America, however, the mammals from the Loup Fork Formation are usually described as Upper Miocene; and the assemblage or fauna certainly exhibits a mingling of the types which are partly Middle Miocene, partly Lower Pliocene, in Europe. The Loup Fork series is chiefly of lacustrine origin, and very widely distributed between South Dakota and Mexico. *Mastodon* now appears for the first time in the New World, but it is not accompanied by *Dinotherium*. The rhinoceroses are still hornless. Besides other genera of primitive horses, *Hipparion* is found. Deer with small antlers are also met with (*Cosoryx*); while *Procamelus* makes a very close approach to the camels, which appear in India in the Pliocene.

Pliocene

The Lower Pliocene vertebrate fauna of the Old World is well known, not only from several widely-distributed localities in Europe, but also from parts of Asia and Algeria. Among marine mammals the Sirenian *Halitherium* and the primitive Cetacean *Squalodon* still survive; but the large majority of the remains found in the marine Antwerp Crag of Belgium, the Red and Coralline Crags of eastern England, and the equivalent sub-Apennine strata of Italy, represent genera of whales and seals closely similar to, or even identical with, those now living. The land-mammals are very numerous, and are best known from the freshwater deposits of Mount Léberon and Cucuron (Vaucluse) and Montpellier (Hérault) in France; from Concud in Spain; from the estuarine yellow sands of Eppelsheim, Hesse Darmstadt; from the Vienna basin and Baltavár in Hungary; and from a torrent-deposit at the foot of Pentelicon in the ravine of Pikermi, near Athens. Abundant remains are also found in a volcanic tuff on the Island of Samos in the Turkish archipelago; in a similar deposit at Maragha in Persia; and in the well-known Siwalik Formation of India. There are, moreover, traces of the same fauna in China, Japan, the Philippines, Borneo, and Java. In the European area true apes are represented by a single thigh-bone from Eppelsheim, and by numerous skeletons of *Mesopithecus* from Pikermi. Among Carnivora, *Felis* and *Hyaena* appear for the first time, associated with *Ictitherium*, which seems to be an ancestor of the latter; primitive types of bears (*Amphicyon, Hyaenarctos*) are also common, but *Ursus* itself is not yet found. *Dinotherium* is now met with for the last time, and *Mastodon* flourishes; but there is as yet no true *Elephas*. Antelopes abound, except in the comparatively northern region of Hesse Darmstadt, and there are several ancestral types of giraffe (*Palaeotragus, Helladotherium*). Typical pigs (*Sus*) are first found; and the very abundant *Hipparion* makes a close approach to the modern horses.

The North American fauna which seems to represent the Lower Pliocene is as yet very imperfectly known. One of the lacustrine deposits containing it, however, clearly rests unconformably upon the Loup Fork Formation, at least in one part of Texas. The hornless rhinoceroses now appear for the last time, species of *Mastodon* and true *Equus* occur, and there are also llamas and peccaries.

The latest (Upper) Pliocene mammalian fauna of Europe is nearly similar to that of the Pleistocene, most of the genera being identical and the species only slightly different. It is best known from freshwater deposits in the Val d'Arno, Tuscany; from a torrent-deposit at Olivola in the Carrara mountains; from Perrier, Ardé, Peyrolles, and other localities in the Puy-de-Dôme, France; from Roussillon and Montpellier (Hérault), where the Lower and Upper Pliocene faunas are not quite clearly distinguished; and from the Norwich Crag of Norfolk. There are still traces of the apes in southern France. Among the Carnivora, *Hyaenarctos* is just becoming extinct, and is being replaced by small bears of the genus *Ursus*; while the species of sabre-toothed tiger, or *Machaerodus*, are now very large. The true beavers (*Castor*) and other characteristic modern genera of Rodentia appear for the first time, and there is a large beaver-like animal, *Trogontherium*. *Mastodon* occurs for the last time, and *Elephas* now appears with the gigantic *E. meridionalis*. There are also true oxen (*Bos*, *Leptobos*) and deer of existing genera (*Cervus*, *Cervulus*); while a typical hippopotamus, as large as the recent *H. amphibius*, is not uncommon. The tapirs have already disappeared, but rhinoceroses survive (*R. etruscus*); and for the first time there is a completely evolved one-toed horse (*E. stenonis*).

The Cromer Forest Bed

At the close of the Pliocene period Britain was still connected with the adjoining continent, and an interesting remnant of the mammalian fauna of this part of Europe at the time is preserved in an old land surface and associated deposits now exposed in the cliffs of Norfolk, especially in the neighbourhood of Cromer. This is the so-called Forest Bed Series. The mammals found here are chiefly those of the typical Pleistocene fauna; but a few still survive from the Upper Pliocene, such as a gazelle, some deer with very large antlers allied to the *Cervus dicranius* of the Val d'Arno, *Rhinoceros etruscus*, *Elephas meridionalis*, and *Trogontherium*. The Forest Bed fauna is thus intermediate in character between the Pliocene and the Pleistocene; and a similar mingling of forms has been observed in certain local deposits both in France and Italy.

Pleistocene

The Pleistocene mammalian fauna of Europe is very remarkable. It does not include many forms which have subsequently become extinct; but throughout the central and north-central parts of the continent there is a curious mingling of northern, eastern, and southern types which it seems difficult to explain. The remains of this fauna are met with in the surface deposits and caverns of Britain, so far north as Yorkshire, quite as abundantly as on the continent; so that the bed of the North Sea and the Straits of Dover must still have been an open valley, and our island remained part of the mainland. The northern animals include the reindeer and musk-ox, which wandered as far south as the Pyrenees; the typically eastern animals comprise the saiga antelope, the jerboa, the tailless hare, and *Myogale moschata*; while among the numerous southern types may be mentioned the lion, leopard, hyaena, and hippopotamus. The principal forms which have since become extinct are *Machaerodus latidens*, *Ursus spelaeus*, *Cervus giganteus*, *Trogontherium cuvieri*, *Elephas primigenius*, *E. antiquus*, *Rhinoceros antiquitatis*, and the great remarkable Russian rhinoceros, *Elasmotherium sibiricum*. Of these latter, the so-called Irish deer (*Cervus giganteus*) is the only one which seems to have survived until the dawn of historic times.

The Pleistocene deposits of North America yield much fewer mammals than those of Europe; but there is a similar mingling of northern and southern types in the central region. It is also interesting to note that one of the commonest skeletons represents the latest known species of *Mastodon* (*M. americanus*). The mammoth, musk-ox, reindeer, elk, and bison are present; bears have now arrived, and the lion may also perhaps be represented; but *Equus* has already become extinct, and there are no traces of the woolly rhinoceros (*R. antiquitatis*), the cave bear, or the hyaena. In the surface deposits of the southern United States some of the typically South American edentates occur (*Megatherium*, *Mylodon*, *Glyptodon*), these having apparently wandered northwards on the emergence of the isthmus of Panama in the early part of the Pliocene period.

North American v. Old World Mammals

Having thus traced the series of mammalian faunas through the Tertiary formations both of the Old World and of North America, it is interesting to speculate as to where the various elements arose. At the base of the Eocene it is evident that the faunas of the east and the west were essentially identical. As they are traced upwards they gradually diverge.

The first noteworthy difference is the great development of the Condylarthra in America, and the rise in the Eocene of the large specialised Amblypoda, of which only a single genus (*Coryphodon*) has been found in the corresponding fauna of Europe. On the other hand, the still larger hoofed animals of the sub-order Proboscidea seem to have originated in the Old World, and did not reach America until the late Pliocene.

The Perissodactyla—the tapirs, rhinoceroses and horses—appear to have advanced on a parallel course on the two continents; though in America both the rhinoceroses and horses became extinct at the close of the Pliocene, the former without acquiring the characteristic horn.

Among Artiodactyla, both the deer and pigs seem to have been approximately parallel in their development on both continents, only differing in some minor branches which soon became extinct. The camels, however, are clearly American throughout, only wandering into the Old World by Asia in the Pliocene. It is almost equally probable that the oxen originated in the Old World.

Among Carnivora, the Creodonta are both American and European; but on the former continent they only pass upwards into the dogs (Canidae), weasels (Mustelidae), and the aberrant cats of the family Nimravidae, while in Europe they are succeeded not merely by these families, but also by the Viverridae, Hyaenidae, Felidae, and Ursidae. The viverroids and hyaenas never reached America, but the true cats and bears arrived in that continent at the close of the Pliocene.

Of the Primates, the primitive lemuroids appeared in the Eocene similarly on both continents; but in North America they soon became extinct, while in the Old World they were followed by the true apes, and still have some specialised survivors.

A. SMITH WOODWARD.

VII

Wachsmuth and Springer's Classification of Crinoids [1]

A LITTLE less than a year ago was published the largest book that has ever been issued all at one time on a crinoid subject. It represents a portion of the final results of over thirty years work by the late Charles Wachsmuth, in collaboration for the last twenty years with Frank Springer. These authors have long been recognised as the leading authorities on Palaeozoic crinoids, a position won chiefly by their most valuable " Revision of the Palaeocrinoidea," published by the Philadelphia Academy of Natural Science. That work, however, was but a paving the way for a greater enterprise, namely, the monographing of all the Palaeozoic crinoids of North America. This task can never be accomplished by them, though it is to be hoped that the survivor may find opportunity to give us the fruits of his knowledge in the remaining branches of the subject. The present volumes are occupied mainly with the systematic account of the North American representatives of those crinoids known as Camerata or 'vaulted' crinoids; but they contain also introductory and morphological parts, dealing with the Crinoidea as a whole. The points raised are so many, often so novel, and the treatment of them so important, that it would be impossible to discuss them adequately in a single article. On the present occasion I shall merely attempt a critical account of the authors' classification of the Crinoidea.

A crinoid reduced to its simplest elements consists of three principal portions—(i.) a theca or test enclosing the viscera; (ii.) five arms stretching upwards or outwards from the theca, sometimes single, sometimes branching; (iii.) a stem stretching downwards from the theca and attaching it to the sea-floor. The theca and arms together are called the 'crown'; that part of the theca below the origins of the free arms is called the 'dorsal cup'; while the ventral part above the origins of the arms, serving as cover to the cup, is known as the 'tegmen.' All these parts are supported by plates or ossicles of crystalline carbonate of lime, deposited in the meshes of the lower layers of the integument. In many cases the skeletal tissue or

[1] The North American Crinoidea Camerata. By Charles Wachsmuth and Frank Springer. Mem. Mus. Comp. Zool. Harvard, vols. xx. and xxi., 837 pp. and 83 pls. Cambridge, U.S.A., May 1897.

'stereom' forms by far the greater part of the whole animal; even the walls of the theca may be so strongly calcified that one marvels how the creature lived with so little space in which to pack its nerve-centre, stomach, and other vital organs. Some of the early cystid ancestors of the crinoids had no stem or arms, but merely a theca like an irregularly plated sac, affording little information as to the structure of the animal beyond the position of mouth, anus, water-pore, and generative opening. But in the crinoid, concomitantly with the development of stem and arms, a more regular arrangement of the thecal plates arose, and the various skeletal elements became more intimately connected with the organs of the body. It is not difficult for us, by comparison with recent forms, to get a fair idea of the internal anatomy of the most ancient crinoids. We can predicate the course of the nerves or the intimate structure of the tissues connecting the plates in any extinct type of crinoid, with no less correctness than the anatomist of Vertebrata can infer the position of the muscles in an Eocene ungulate; and it is doubtful whether our conclusions would be modified in any important point had the subjects of our study been preserved to us by the most approved laboratory methods instead of as petrifactions.

The cup, in its simplest form, consists of two circlets of five plates. Each plate of the upper circlet supports an arm and is called a 'radial'; the plates of the lower circlet, the 'basals,' rest on the stem and alternate with those of the upper circlet, *i.e.*, are inter-radial in position. Some crinoids have yet another circlet below these, and the constituent plates are called 'infrabasals'; they are radially situate. The tegmen in most primitive forms, as well as in the embryonic stages of the living *Antedon*, consists of five large triangular plates, alternating with the radials, and called 'orals,' because they roof over the mouth. Between one of the orals and the two adjoining radials there usually opens the anus, while this same posterior oral is pierced by one or more water-pores. The arms consist of a series of ossicles, called 'brachials'; each is joined to the radial by a muscular articulation, permitting of motion up and down, while each brachial is usually united to its successor by a similar articulation. The arms usually fork more than once, and the brachials after the radial and after each successive forking are termed brachials of the first order, of the second order, and so forth, or, as I have found more convenient, 'primibrachs,' 'secundibrachs,' 'tertibrachs,' etc. There passes along each arm, forking with it, an extension from each of the chief systems of the body; and to receive these the brachials are grooved on the ventral surface. We need only note here the axial nerve passing from the dorsal nerve-centre ('chambered organ') and innervating the arm-muscles; and the ciliated food-groove which sends a continuous current of water down the arms and over the tegmen to the mouth. The axial nerve-cord is dorsal in position, *i.e.*, it lies at the very bottom of the ventral groove, and may be shut off from it by an ingrowth of stereom, so as to lie in a distant 'axial canal.' The food-groove lies on the ventral surface; it is fringed by extensions from the ambulacral or water-vascular system, and is protected by small plates that can shut down over it, and are called 'covering-plates' or 'ambulacrals.' The stem or column consists of a series of ossicles, which may be circular, pentagonal, stellate, or oval in section, but always are pierced by a central 'axial canal,' which contains a prolongation from each of the five chambers of the dorsal nerve-centre.

The modifications of this simple type are numerous and in very different directions. Some of them are extraordinary, and structures have been produced that long remained unsolved enigmas. Moreover, many modifications, both of ordinary and extraordinary type, can be shown to have occurred more than once in groups of very diverse origin; for instance, the work under review describes a Carboniferous Platycrinid, *Camptocrinus*, in which the stem has undergone the same remarkable curvature and arrangement of cirri as affected the Silurian Heterocrinid, *Herpetocrinus*. Were all the crinoids that ever lived placed before us, with no indication as to their distribution in time or space, it could only be by an unlikely chance that anyone would hit on their true relationships; and to the classification of one author, that of another could always be opposed. Fortunately we do know something of the succession of this remarkable series of forms, and we can trace, with some assurance of correctness, a few lines of descent which it would be hard to controvert. The whole history of the attempts to classify the Crinoidea, shows the gradual recognition of these principles, and the gradual emancipation from the older habit of lumping forms together because they were alike in structure without considering how the likeness arose. This has been the history of all branches of systematic zoology, and if the latest classification of the Crinoidea does not attain perfection, it is partly because there are still too many gaps in our knowledge of the geological history of the class, a point admirably emphasised by Messrs Wachsmuth and Springer, partly because it is one thing to have a conviction, and another thing to have the courage to act upon it, and by so acting to overturn established and accepted beliefs.

For the purpose of this article it is necessary to consider only those modifications of the simple crinoid type that influence the classification, in other words, those modifications which are believed to indicate some affinity between the forms exhibiting them.

In the simple type the arms are freely moveable on the radials, they are distinct from the cup, *i.e.*, do not help to enclose the viscera, and are separate from one another, 'inadunate' as it is termed. Modification of this takes place in two directions, nearly but not quite the same, and both described as the incorporation of the lower or proximal brachials in the dorsal cup so as to support or surround the viscera.

In the one case this incorporation takes place by the intercalation of interbrachial pieces, and these and the 'fixed' or incorporated brachials are rigidly united by the firm mode of union known as 'close suture.' *Pari passu* with this incorporation of brachials in the cup there necessarily goes an incorporation of ambulacrals in the tegmen and their fixation by means of 'interambulacrals,' which are merely supplementary plates like the interbrachials. A further effect of this rigid fixing of the elements of the theca is that the ambulacrals of the tegmen cease to open, so that the food-grooves instead of being open gutters become closed tubes and pass beneath the orals to the underlying mouth. The process may even be carried further: the interambulacrals increase in size, and the ambulacrals may be squeezed down between them, so that all except a few of the larger ones are invisible from the upper surface of the tegmen; these larger ones become prominent and are called 'radial dome-plates.' Thus arises a form in which all the ambulacral structures, including the various extensions of the nervous, generative, and other systems of the body are absolutely subtegminal, and appear to be covered over by a rigid dome or vault of solid plates. So much is this the case that Wachsmuth, to whom the earliest complete account of the structure is due, supposed that the vault was actually a fresh formation that had somehow or other grown right across the normal ventral surface. That the vault is not really so anomalous a structure, but merely a modified tegmen, has only been proved within the last seven years by Wachsmuth and Springer, and is the view maintained in their present work.

In the second case incorporation of the lower brachials may be either by lateral union with those of adjacent rays, or by supplementary interbrachials, or by a finely plated integument; the difference is that the union is not rigid, but all plates above the radials retain some power of yielding or flexibility, even if they have not always the faculty of active motion. Of necessity correlated with the flexibility of the cup is the flexibility of the tegmen, which here gradually increases by decrease in size of the plates, or even by their complete disappearance; even the orals and ambulacrals have atrophied in some of the later representatives of this group. Another difference is the persistence of mouth and food-grooves as suprategminal, *i.e.* open on the ventral surface.

In accordance with these three types of structure it is possible to group the crinoids into three Orders. This is done by Wachsmuth and Springer, who call the first Order Inadunata; the second, Camerata; and the third, Articulata. But were these the only characters to guide us, we should often err in referring families to their respective Orders; or, to put it another way, a collocation of families by these structures alone would bring together forms that other evidence forbids us to regard as related. We know for certain that we should place the Pentacrinidae and Antedonidae side by side, because this is just what has always been done. Fortunately Wachsmuth and Springer have discovered a fresh character, which they believe is of considerable value. They maintain that in all undoubted Inadunata and Camerata new columnals are developed next the cup, so that the top one is always one of the latest formed, and continually moves from its proximal position as new columnals develop; but that in all undoubted Articulata the top columnal is not the latest formed, but a persistent element, for which I propose to use the term 'proximale'; it often fuses with the infrabasals. This discovery is one of the chief novelties in the present monograph, and it appears to be of great importance. There is, however, no great attempt to prove its universal application; in fact the whole question is disposed of in the few following lines. "In the young Comatula [*i.e.*, *Antedon*], in which the top joint subsequently develops into a centrodorsal, in the recent [*sic*] Mesozoic *Millericrinus*, and probably in the recent *Rhizocrinus* and *Calamocrinus*, and in all Ichthyocrinidae, so far as observed, the new nodal joints were formed beneath the top joint, and the latter remained permanently attached to the calyx. In *Apiocrinus*, in which for some distance the upper end of the stem is greatly inflated, and the proximal joints extremely long, it is possible that the nodal joints were introduced below the inflated part, for there appear to be no immature segments between the upper joints." This last sentence is not quite correct. One often meets with a specimen of the Bradford pear-encrinite (*A. elegans*, Defr.) with imperfect ossicles in the proximal cone; indeed similar incomplete portions of columnals may occur above the proximale itself, as in the British Museum specimens registered 46,234 and 34,520. Whether these imperfect portions are immature, in the sense that they might have become more mature and complete, may be disputed, but they certainly resemble the columnals which Mr Alexander Agassiz has, no doubt rightly, regarded as immature in *Calamocrinus*. In such genera as *Ichthyocrinus* and *Taxocrinus* it is by no means easy to satisfy oneself that there is a persistent proximale. Of course when the top columnal fused with the infrabasals, it must have been persistent; but it did not always so fuse. I do not mean to say that there is any objection to this statement by Messrs Wachsmuth and Springer; but when it is introduced by them as one of the three "most important characters for dividing the Crinoids into orders," one would have expected more direct evidence, more illustration, and more discussion of apparent difficulties, such as the abnormalities (for so I am content to regard them) met with in *Apiocrinus*, or the exactly opposite view maintained by some preceding writers concerning some of the genera mentioned.

Accepting the character as valid in the absence of actual disproof of its universality, we see that it enables a clear separation to be made between such a form as *Sagenocrinus* and certain Camerata, between *Pycnosaccus* and the Botryocrinidae, between *Millericrinus* and *Antedon* on the one hand, and the Pentacrinidae on the other. A proximale occurs in the former in each case, but not in the latter. But it does not follow from this that the presence of a proximale indicates genetic affinity. There can, it is true, be little doubt that all the Palaeozoic genera placed by Wachsmuth and Springer in their Order Articulata are allied; they can scarcely be separated even into genera. But it is by no means clear that they are the ancestors of the Neozoic genera placed in the same Order.

The Neozoic Articulata are distinguished from the Palaeozoic by the possession of 'pinnules,' which are defined by our authors as "small lateral appendages, given off alternately from opposite sides of the arms," to which should be added, that they have the same anatomical structure as arm-branches, but that they do not branch themselves, that their alternating arrangement is regular, and that when present the maturation of the generative products takes place only in them. I have long urged the view, in which I still firmly believe, that pinnules are derived from arm-branches, of which they are, "as happily expressed by Carpenter, repetitions on a small scale" (Wachsmuth and Springer). I am astounded to find that, in the opinion of the learned American writers, my "views respecting the pinnules are rather peculiar"; I fail to see any meaning in the argument that my "explanation is not satisfactory, as it would indicate that the smaller appendages are derived from the larger ones"; as for their belief that "every species of *Botryocrinus* [the genus on whose evolution I based my theory] has armlets, and that pinnules are not represented in any of them," I can only say that it can never be shared by anyone that has seen the type-specimen of *Botryocrinus pinnulatus*.

Our authors nowhere attempt to explain the origin of pinnules, so we are left to infer that they regard them either as independent developments, somewhat sudden in their first appearance, or as structures handed down from some Cystid ancestry. That they regard the possession of pinnules as a strong distinctive character, is shown by their further criticism of me on p. 161, where they say that I "cannot do" a great many things which I have done. When, therefore, they separate their so-called Articulata into the non-pinnulate Impinnata and the pinnulate Pinnata, two sub-orders between which no connecting links are known, they force one to demand the evidence that the latter are not derived independently from pinnulate Inadunata. If they will not admit the origin of pinnules from armlets, it is hard to see how they can maintain the descent of Pinnata from Impinnata. It must be remembered that the only essential difference between the Pentacrinidae (the type of Johannes Müller's Articulata) and the Articulata Pinnata of Wachsmuth and Springer, lies in the possession of a proximale by the latter. Now, why can this not have been acquired just as easily as pinnules? It was acquired once, we may suppose, when the Impinnata originated; why not a second time, when the Pinnata separated off? This question has not been discussed by Wachsmuth and Springer, although necessitated by their opinions far more than by mine.

As for the name 'Articulata' our authors themselves recognise that it may be objected to on the ground that it does not correspond with the Articulata of Müller, and they propose the name 'Articulosa' as an alternative. That name, however, had already been used by Dr Jaekel in a different sense, in 1894, and the very appropriate name Flexibilia was proposed by Professor K. von Zittel in 1895. This last, being free from confusion with other names, is the best to adopt.

Let us turn now to the Order Inadunata. These "represent the simplest form, their dorsal cup being composed invariably of only two rings of plates, or three when infrabasals are present. It has no supplementary plates, except an anal piece; but this is not represented in all of them. The arms are free from the radials up." This order is divided according to the structure of the tegmen into two sub-orders: the Larviformia, in which the tegmen consists only of five orals, completely covering the mouth; and the Fistulata, in which the posterior inter-radius is "drawn out into a sac or tube." Long ago I criticised this division. Now, in the first place, I repeat that it does not hold good on mere morphological grounds; the anal tubes of *Symbathocrinus* and *Pisocrinus* are quite as highly developed as that of *Herpetocrinus*. The presence of a tube in the last-mentioned genus or in *Heterocrinus* is no more proof of the presence of interambulacrals in the tegmen than it is in *Pisocrinus*. But as I do not wish to plough barren fields of controversy, I leave this for the more important consideration that, even if our authors' statement of fact be granted, it appears to be admitted by them that the structure of the Larviformia represents a primitive stage, and that from them the Fistulata descended. Dr Jaekel has shown, for instance, that *Mycocrinus* and *Catillocrinus* are final stages in a series of which *Pisocrinus* and *Calycanthocrinus* are the earlier terms. The former are 'Fistulata,' the latter are 'Larviformia.' I have myself tried to prove the intimate relation existing between *Pisocrinidae*, *Heterocrinidae*, and *Calceocrinidae.* In short, the Larviform and Fistulate types represent grades of structure, and not those fundamental divergences, on which sub-orders should be based. In a paper on the Inadunata of Gotland I attempted to show that the presence or absence of infrabasals was a far more deep-seated character. So far as I can see the arguments now brought against this are—first, that I was wrong in saying that *Cupressocrinus* and *Myrtillocrinus* had no infrabasals (even though I said so with a note of interrogation); secondly, that I attached undue importance to another character, namely, the horizontal bisection of certain radials. Even if this were to be admitted, it could not substantiate the remark, " All this is seriously in the way of making the presence or absence of infrabasals a subordinal character." The question is, whether anyone has ever proved a transition from a true monocyclic form (without infrabasals) to a true dicyclic form (with infrabasals), or *vice versâ.* This question is partially discussed in another chapter by Messrs Wachsmuth and Springer, and the answer to it is " No." This is enough to place the character above the varying development of the tegmen.

In their sub-divisions of the Camerata our authors appear to me to be even less fortunate. I do not propose to discuss the validity of the families, merely that of the larger groups. And at the outset it must strike everyone as peculiar that the two chief divisions should be into a Typical and a Non-Typical section. Why should one section be more ' typical' than the other? The Typical section contains those forms in which the lower brachial and inter-brachials form an important part of the dorsal cup, and includes the Reteocrinidae, Thysanocrinidae, Rhodocrinidae, Melocrinidae, Calyptocrinidae, Batocrinidae, and Actinocrinidae. In the Non-Typical section brachials and inter-brachials are but slightly represented in the dorsal cup. The families are Platycrinidae, Hexacrinidae, Acrocrinidae, and Crotalocrinidae. It appears to me that we have to deal here with four quite different sets of crinoids, that have all undergone modification to a greater or less extent along Camerate lines, but that are of quite different origin. The Crotalocrinidae, for instance, I have always regarded as intimately connected with the Cyathocrinidae, although the publication of the proofs accumulated in favour of this view has been long delayed; here I am glad to be in agreement with Dr Jaekel. The Platycrinidae and their allies, as I have before this attempted to show, are not far removed from the Monocyclic Inadunata, and were developed independently, long after the rest of the Camerata had come into a flourishing existence. As for the latter, is it not reasonable to suppose that the monocyclic Melocrinidae, Calyptocrinidae, Batocrinidae, and Actinocrinidae, all which have much in common, were derived from the monocyclic genera here referred to the Reteocrinidae, or at all events from simple forms like to them; and that the dicyclic Thysanocrinidae (or Dimerocrinidae), with their allies the Rhodocrinidae, were the descendants of the dicyclic Reteocrinidae? It does not seem at all necessary to lump monocyclic and dicyclic genera together in one family simply because their interbrachial plates are ill-defined.

Expressed in few words, an opinion as to the classification offered in this magnificent monograph might fairly say that from an anatomical standpoint it is by far the best that has yet been proposed, but that it serves as a key to structure rather than as an epitome of genetic affinity. The authors say: "We have not attempted to construct a genealogical tree for the Crinoids, or a branch of one for the Camerata, because such representations are generally unsatisfactory, and in this case the tree would have to be constructed too much upon imagination. Besides, our task is a humbler one. We have rather preferred to content ourselves in this respect with giving the general facts which our investigations seem to pretty well establish, and such interpretation of them as appears to us reasonably consistent therewith. Within these limits we have hoped that our generalisations may help to form a stable foundation upon which others may raise more ambitious structures." The absence of a phylogenetic tree is no cause for grumbling; but had the authors, when constructing their classification, kept phylogenetic principles more in view, they would probably have laid a foundation that would have needed less taking up and relaying by those who, in future times, shall complete the structure. Perhaps in the near future I may be allowed to indicate the plan that such a foundation might well follow.

There has been no attempt here to review the work as a whole, but while fault-finding may be postponed indefinitely, one can no longer delay an expression of gratitude for the large amount of information here collected and for the beautiful plates by Keyes, Westergren, Ridgeway, Liljevall, and others, that illustrate the work. Thanks also are due for the generosity of Mr Agassiz in producing these costly volumes as part of the *Memoirs* of the Museum of Comparative Zoology. But while expressing our thanks and our appreciation, we are repeatedly saddened by the thought that the senior author survived neither to receive the congratulations so justly his due, nor even to see this offspring of his maturity and full-garnered knowledge brought to the close that would have crowned his life's work. F. A. BATHER.

SOME NEW BOOKS

After Darwin

Darwin and after Darwin. By the late George John Romanes. Vol. III. Post-Darwinian Questions: Isolation and Physiological Selection. Cr. 8vo, pp. 181. London: Longmans, Green & Co., 1897. Price, 10s. 6d.

The third and last volume of "Darwin and after Darwin" deals entirely with post-Darwinian questions, and is practically a re-statement in rather clearer terms of the views of the author and others on isolation and physiological selection. Clearness has not hitherto been a characteristic of the small school of writers represented by Romanes and Gulick, therefore a statement of their case in terms which could be "understanded of the people" was really desirable. We cannot think, however, that, having been so stated, it is likely to win over more supporters from the ranks of the natural selectionists. The more clearly the theory of physiological selection is formulated, the less adequate it appears to produce the results claimed for it. It is a very long way after Darwin indeed. That great master began with facts, and only after years of patient accumulation of these did he think himself justified in enunciating as general laws the conclusions he drew from their study.

Mr Romanes' theory scarcely even pretends to be founded on facts; it is based on predictions which have as yet only received even apparent verification in a very few cases, yet the author himself admits that "the whole theory" must "stand or fall with the experimental proof of the presence or the absence of cross-infertility between varieties of the same species growing on common areas." The few facts he does adduce "as serving to corroborate" his theory are scattered at wide intervals through the volume, and are drawn chiefly from (1) a group of allied plants in one locality, and (2) "several genera" of land mollusca.

When we say that, on the authority of Le Conte, even the Steinheim snails (*pace* Weismann!) are requisitioned to add to this category, it will be evident that the physiological selectionists have not been able to produce an overwhelming amount of evidence from direct observation. Meanwhile, cases "making directly against" the theory are quietly dismissed in a note (p. 135) as "not numerous."

The strength of the position taken up by natural selectionists lies in the fact that their theory of the adequacy of natural selection to produce divergence is based on the universally-admitted fact of individual variability, and starts directly from this point; they do not claim anything more as material to work upon. All other theories constantly require something to be taken for granted at the outset which is not universally admitted. No argument, however closely followed out to its logical conclusion, can be really convincing unless it starts originally from undisputed facts; the weak point of Romanes' book is that he is continually basing upon premisses which are, to say the least, doubtful. Thus it is stated as a fact that hybrid plants are "rare in a state of nature." Botanists are by no means agreed on this point, and those who have made an exhaustive study of the flora of limited areas, with special reference to this question, tend more and more to the belief that hybridism is far from uncommon. On pp. 32 and 33, the whole argument is directed to showing that "we must henceforth cease to regard" natural selection "as in any instance the originating cause" of diversification in organic nature. Who could ever have begun to regard it as such? Individual variability is the originating cause, whether the "sustaining cause" be natural selection or physiological selection, or both, for it is admitted that they may act together.

Further, the method of argument is very imperfect. For example, geographical isolation is constantly referred to and used as an illustration of indiscriminate isolation, but it is not mentioned until the last chapter, and then only quite incidentally, that "it belongs to the very essence" of the author's view that "the efficiency of indiscriminate isolation as a 'vera causa' of organic evolution varies inversely with the number of individuals (*i.e.*, the size of the species-section) exposed to its influence." The reader, therefore, whose intelligence has enabled him to perceive that such a premiss was essential, has up to this point been obliged to set aside as valueless those statements from which it was omitted. This is a pity, but it may, of course, be partly due to the fragmentary nature of the book, of which only the first two chapters and the last were in type before the author's death, as the editor (Prof. Lloyd Morgan) explains in the preface. There are three appendices, which do not add materially to the value of the book. Altogether, we cannot think that "Darwin and after Darwin" strengthens its author's position with regard to his theory; the latter must still be regarded as not proven, though future evidence may modify this opinion. At present it cannot be said that good cause is shown for considering physiological selection as of paramount importance as a factor in evolution, or that the theory gains much by this exposition.

THE FRESHWATER FAUNA OF BOHEMIA

UNTERSUCHUNGEN ÜBER DIE FAUNA DER GEWÄSSER BÖHMENS. By Anton Fritsch and V. Vávra. Prag.: Fr. Rivnac, 1897.

DRS ANTON FRITSCH and V. Vávra have issued Part III. of their joint researches on the fresh-water fauna of Bohemia; this section deals with the animals inhabiting the lakes known as the Schwarzer See and the Teufelssee, as well as with the land-fauna and flora of the neighbourhood of each. Their results are evidence of the excellent work that may be done, without any very large outlay of money, by setting up temporary portable zoological stations in little-worked localities, and making thus a thorough study of the fauna of a limited area. Marine biological laboratories have already more than justified their existence in England and other countries, and we should like to see many more inland stations of the kind which Bohemian enterprise has made so successful. Especially is one needed in our own country, but nobody seems inclined to go to work. During four years' work in the neighbourhood of the lakes the investigations were very exact and exhaustive, and fresh-water fauna were for the first time studied with reference to their bathymetrical distribution. Full lists were made of all species observed; these might have been more numerous in a more fertile region, but the methods of investigation leave nothing to be desired.

The ingenious but simple apparatus (figured on page 15 of the publication) for catching small land-animals of various kinds is worthy of special attention, and might be used by anyone living in the country, with the result of greatly increasing our knowledge of the species inhabiting one district. A general summary of results would have added to the interest of the pamphlet, which consists of 74 pages, and is profusely illustrated.

Indo-Pacific Shells

Catalogue of the Hadfield Collection of Shells from the Loyalty Islands. By J. C. Melvill and R. Standon. Parts II. and III. 1897. Price 2s.

This pamphlet forms part of the series of handbooks issued by the Manchester Museum. Part I. was published in 1895, and the whole is merely a reprint from Vol. VIII. of the *Journal of Conchology*. The list will prove useful to future workers, not only on the fauna of the Loyalty Islands, but also of other localities in the Indo-Pacific. Many of the species have a wide range, and the authors call attention to "the cognate character in the molluscan fauna" of those islands and the Mauritius. That of the Loyalty Islands appears to be extremely rich, as many as 860 species being recorded by Messrs Melvill and Standon. Nearly all of the 106 new species described are very small, but many of them are very beautifully sculptured. It is questionable whether the authors are to be commended in such free use of Greek for the specific names. Mr Hoyle's introduction states that a few of the type specimens remain in Mr Melvill's cabinet, but the final paragraph of the paper says, "The original types are all in the Manchester Museum."

Life

What is Life? or, Where are We? What are We? Whence do we Come? and Whither do we Go? By Frederick Hovenden, F.L.S., F.G.S., &c. London: Chapman & Hall, 1897.

The legs and feet of this work are physical, and developed in accordance with principles which modern physicists (who are severely handled) regard as likely to render them weak-kneed and rickety. Its head and shoulders rise into regions of anti-theology. Its middle (concerning which alone we are here concerned) is mainly a piece of biological patchwork, to form which scraps from many authors have been collected with more diligence than discrimination. Apart from the patches, we have not discovered anything of biological value in the work. Here is a sample of the conclusions to which the author is led. "All living creatures, except the very lowest forms, are built up of cells and the secretions of these cells. All cells are built up of molecules. In the higher animals, including man, the cells are controlled by the central molecule, which is in its turn controlled by the fundamental atom." At death the constituents of the body tend to be distributed in the air. "From principally the air, the specific atom, which forms the initial fundamental molecule, again gets into the system of the human female, gathers to itself from the living organism the material to form the fundamental molecule, which is the initial formative power of the growing object—the human egg. And this egg, by the process already described, alters itself by adding to itself the material from the parent organism and forms the human being." Here we have a philosophy of generation and regeneration in a nut-shell.

DOWN WITH NATURAL SELECTION!

THE ARGUMENT OF ADAPTATION; OR, NATURAL THEOLOGY RECONSIDERED. By Rev. George Henslow, M.A., F.L.S., &c. London: George Stoneman, 1897.

APART from the arguments with regard to Natural Theology, the discussion of which may be left to others, this little volume sets forth the contention that natural selection is an "hypothesis based on an imaginary but baseless inference." Artificial and natural selection are not parallel processes, but diametrically opposite in character. "Not a trace of the innumerable unfavourable variations have [*sic*] ever been seen, whether in a plant or an animal." There, is, however, no adequate discussion of the evidence for variation under nature. And the assertions with regard to biological questions are so subservient to the main theological purpose of the work that it must suffice to state that these opinions among others are to be found in Mr Henslow's booklet.

INDIAN AGRICULTURE

THE FOUNDATIONS OF SCIENTIFIC AGRICULTURE. By Samuel Cooke, M.A., &c., Principal of the College of Science, Poona. 8vo, pp. ix. 268. London: Longmans, Green & Co., 1897.

AGRICULTURE is unquestionably the greatest and most important of all industries, the progress of which has been continually hampered by ignorance of the theory by practical workers. Scientific systems, when applied by rule of thumb, have only too often led to disaster owing to reckless waste of expensive materials and the lack of adaptation to local conditions. Hence every work which spreads a knowledge of scientific principles upon which some of the improved methods of farming have been founded, is to be welcomed. A considerable number of small text-books on elementary agriculture have appeared during the past few years. Prof. Cooke's work will rank among the best. It is especially intended for use in India, but may be read with advantage by English students owing to the freshness of the example quoted. The book is based on a series of lectures delivered to classes working at agriculture and forestry at the Poona College of Science. It consists of fourteen chapters. The first three contain the general introduction and the meteorological part of the subject. The soil is described in four chapters in which are explained the necessary facts of geology, mineralogy, crystallography, and chemistry. Then follows a chapter on the elementary facts of botany, and three with crops, manures, and on the agriculturalist himself. Finally comes a chapter on mensuration, a glossary, a list of examination questions, and three appendices.

The work deals with the elements of so many sciences that a few errors are inevitable. Thus in the table of British strata the coral marbles are assigned to the Lower instead of to the Middle Devonian; Silurian limes and fluxes are said to come from the Ludlow beds and only green slates from the Wenlock series. On page 3 we read of the *Pax Britannicae.* But slips like these do not materially lessen the value of the work which ought to have a wide circulation among Indian schools.

An American Astronomy

A New Astronomy. By David P. Todd, M.A., Ph.D. 8vo, pp. 480. With 6 coloured plates. New York, Cincinnati, Chicago: American Book Company. 1898. Price, $1.30.

We regret that the fascinating science of astronomy does not come within our scope, otherwise it would have given us great pleasure to have reviewed this elegantly illustrated little book, especially as the writing of the review would have given us very little trouble, for the publishers, with a business-like courtesy, have been at the pains to forward us both a short notice and a longer account in pamphlet form, both of which they would kindly permit us to reprint without acknowledgment or extra charge. We must content ourselves with drawing the attention of astronomically-inclined readers to the book.

Serials

Knowledge, for April, contains three very interesting articles. Prof. Grenville A. J. Cole writes on the Structure of Ireland, considering it from a broad standpoint as part of Europe. Mr R. Lydekker discusses the Sea-Otter and its extermination, giving a figure of its upper and lower jaws from a skull collected by Mr Barrett Hamilton. Mr Fred. Enock continues his paper on British Bees. Mr Enock's illustrations should be known to all; his text is equally clear and interesting.

In Vol. VI. of the *Studies from the Yale Psychological Laboratory* (1896), edited by Dr E. W. Scripture, there are researches on reaction-time, on Weber's law in illusions, and on voluntary effort. Perhaps the most generally interesting paper is that by the editor, in which he describes an elementary course in psychological measurements. This gives a good idea of the kind of laboratory work which is carried out at Yale under Dr Scripture's direction.

A short time ago we announced that *Timehri,* the Journal of the Royal Agricultural and Commercial Society of British Guiana, would cease publication in December last. On receipt of the December number we were pleasurably surprised to find that the magazine would be continued, with a reduced number of pages, at a price of 60 cents. (2s. 6d.) a copy. Somewhat appropriately, but we hope not ominously, the present number contains an instructive and amusing article entitled "Abortive Colonial Publications," by the Editor, Mr James Rodway. Mr J. J. Quelch writes on Boa Constrictors (Camoodies as they are called in British Guiana), while Mr C. A. Lloyd contributes notes on some Neotropical Birds.

In the February *Pearson's Magazine* the growing appetite of the public for popular natural history is met by an account of the trout-farm at Guildford. This is interesting reading, and, from our point of view, is more free from scientific errors than is usually the case in articles of this description. The illustrated development of the trout is pedantically described as its 'evolution,' and its yolk-sac is inaccurately placed. A two-year-old trout, we are told, passed an accidental sojourn in a 'fry-pond' for a few months, and lived up to his privileges to the extent of devouring 30,000 fry. Those who are prepared to accept this statement will, we do not hesitate to state, cry 'enough' when told that the weight of this 'two-year-old' increased during that period from 6 oz. to 5¾ lbs. The article is beautifully illustrated, and cannot fail to interest. We cannot say the same of the extraordinary literary effort on "Human Nails and Horns," preceding it. Here the author audaciously describes everything from a human wart to a deer's antler as a 'horn,' apparently considering that the authority of the *Boston Medical Journal* and the like is sufficient to allow his artist to give free vent to his imagination. He remarks: "The whole subject is one highly deserving of attention from the naturalist or the philosopher." Speaking from the point of view of the former, we think we may safely pass this article on to the latter!

We continue to receive copies of interesting papers from Mr T. H. Holland, now officiating Superintendent of the Geological Survey of India. The latest is "On a Quartz-Barytes rock occurring in the Salem district, Madras Presidency" (*Records Geol. Surv., India*, vol. xxx. pp. 234-242, pl. xviii., 1897). The rock forms a network of veins in gneissic rocks. The veins, which vary considerably in size, occupy fissures inclined at all angles to the horizon, and are composed of quartz and barytes in the proportion of 7 to 3. In the rock there are also found small quantities of accessory minerals, such as galena, pyrites, ilmenite, and hematite, which carry a small proportion of gold, 13 grains to the ton. The barytes is well crystallised, often forming large individuals, and this, as the author points out, is also the case even in veins of the smallest dimensions. The quartz is seen under the microscope to consist of an aggregate of irregularly interlocking crystals, which are sometimes quite microscopic. The author's conclusion is that both the quartz and barytes are original constituents, which have separated out from an injected mobile magma, the barytes being the first to crystallise. He rejects another possible explanation, namely, the derivation of the rock from a pegmatite (barytes replacing felspar by pseudomorphism), on the ground that the proportion of quartz to felspar in the graphic pegmatites is quite different; moreover, there are no traces of any material after which the barytes might be pseudomorphous.

The John Hopkins University *Circular* for November 1897, is melancholy reading, for in it Prof. W. K. Brooks has to tell the story of the students' expedition to Jamaica in the summer of 1897, and its tragical end. A party had been formed, under the charge of Prof. J. E. Humphrey, for the purpose of botanical and geological research, and they set up a temporary laboratory at Port Antonio. Here two months' good work was done, and Prof. Humphrey, with Drs Conant and Clark, and Mr Fredholm, had seen all the younger members of the party safely started for home, when the former was suddenly struck down by fever, and died on August 17th, after a few hours' illness. Dr Conant, left in charge, unselfishly refused to hasten his own departure and leave his friends behind; both he and Dr Clark were taken ill, and Dr Conant died on his way home. This number of the *Circular* is especially devoted to memorial sketches of Prof. Humphrey and Dr Conant, whose untimely deaths have caused an irreparable loss, not only to the biological department of the University, but to all who ever came in contact with these earnest and single-minded workers. The *Circular* also contains short papers by Conant, Clark, and others, on the viviparous *Synapta* of the West Indies, the follicle-cells in Salpa, the Cubomedusae, and other subjects.

Mr W. E. Hoyle has sent us a supplement to his "Catalogue of Recent Cephalopoda," including the 79 new species published between 1887-1896. Most of the novelties are from eastern seas, and are due to Drs Ortmann and Brock, and Mr E. S. Goodrich; but the most important systematic work of recent years is Dr Jatta's Monograph on the Cephalopoda of the Gulf of Naples, which was reviewed by Mr Hoyle in our own pages. Mr Hoyle, who of course writes as an authority, states that he has found scarcely any omissions from the lists given in the *Zoological Record.* This supplement, as was the original catalogue, is published by the Royal Physical Society of Edinburgh; the date 1897 is printed on the wrapper, and we learn that it is a reprint from the *Proceedings* (pp. 363-375); but we do not gather from the pamphlet before us what was the volume in which it originally appeared, or whether it is to be purchased in its separate form, and if so at what price. We mention these points merely because we were given to understand a short while since that the Royal Physical Society of Edinburgh was initiating some useful reforms in connection with authors' reprints.

FURTHER LITERATURE RECEIVED

Biomechanik erschlossen aus dem Organogenese, Mehnert: Fischer, Jena. Fossil Plants for Students, Seward: Cambridge University. Text-book of Botany, Strasburger and others, translated by Porter; Essays on Museums, Flower; Notes on Observations, Lupton: Macmillan, London. Mammals, Reptiles and Fishes of Essex, Laver: Essex Field Club. Practical Radiography, Isenthal and Ward: Dawbarn & Ward, London. Elementary Chemistry, Cheetham: Blackie, London. Anatomie Comparée, Roule: Masson, Paris.

Results of Swedish Research into Grain Rust, Eriksson: *Botan. Gaz.* Resultate der Tiefsee-forschung, Chun. Ratzel's History of Mankind, 24, 25: Macmillan. Princetown Contributions to Psychology, vol. ii. No. 3. 20th Rep. State Entomologist Illinois. Bergens Museums Aarbog, 1898. Witwatersrand Banket, Goldfields of S. Alaska, Becker: *Ann. Rep. U.S. Geol. Survey.* Kant as Natural Philosopher, Fractional Crystallisation of Rocks, Becker: *Amer. Journ. Sci.* Contents-Subject-Index to General and Periodical Literature, sections 3-6, Cotgreave. Ann. Rep. Raffles Museum, Singapore, 1897. Castration nutriciale chez Hyménoptères, Marchal: *C.R. Soc. Biol.* Entomologie appliquée en Europe, Marchal: *Bull. Soc. Nat. Acclimatation.* Cécidomyies des Céréales, Marchal: *Ann. Soc. Entom. France.* Univ. Wisconsin Agric. Expt. Station, Bull. 65. Existe-t-il une force vitale? Errera. Atoll of Funafuti, pt. 6: *Mem. Austral. Mus.* U.S. Dept. Agriculture Circulars, 2nd series, Nos. 22-28.

Scot. Geogr. Mag., April; Amer. Journ. Sci., April; Amer. Micros. Journ., March; Amer. Nat., Feb.; Victorian Nat., Feb.; L'Anthropologie, Jan. and Feb.; Botan. Gazette, Mar.; Feuille des Jeunes Nat., April and May; Irish Nat., April; Westminster Rev., April; Knowledge, April; Literary Digest, Mar. 12, 19, 26, April 2; Naturae Novit., Feb., No. 4; Scot. Med. and Surg. Journ., April; Naturalist, April; Nature, Mar. 17, 24, 31, April 7; Nature Notes, April; Naturen, Mar.; Photogram, April; Proc. Biol. Soc. Washington, xii. pp. 31-84, Mar. 24; Psychol. Rev., Mar.; Review of Reviews, Feb., Mar.; Revue Scient., Mar. 19, 26, April 2, 9; Science, Mar. 11, 18, 25, April 1; Scientific Amer., Mar. 12, 19, 26, April 2; Journ. School. Geogr., Feb., March; Riv. Psichologia, 22; Timehri, x. part 2.

OBITUARIES

JOHN ROBERT STREATHAM HUNTER-SELKIRK, who died on March 23, in the sixty-third year of his age, at his residence, Daleville House, Braidwood, near Carluke, Lanarkshire, was a well-known antiquary and geologist. From his rich collections he had made large donations to the Museums of Kilmarnock and Airdrie, and he also bequeathed some to different Institutions. For many years he had been a member of the Royal Physical Society of Edinburgh, and of the Geological Societies of Edinburgh and Glasgow.

FRANZ FIALA, Curator of the pre-historic and anthropological department of the National Museum of Bosnia-Herzegovina, died at Sarajevo on January 28, 1898, aged 36 years. He had published much on the flora of those countries and had also made numerous studies in their archaeology, especially on the pre-historic tumuli of Glasinatz in connection with Dr C. Truhelka.

GEORGE CHRISTOPHER DENNIS, for many years President of the York and District Field Naturalists' Society, died suddenly of apoplexy at York on December 22, 1897.

The deaths are also announced of:—JULES MIGNEAUX, a well-known natural history draughtsman, at Billancourt, aged 65; P. B. L. VERLOT, the botanist, at Verrières-les-Brusson; E. J. S. LINNARSSON, the botanist, a tutor in Skofde, Sweden; RAMON LISTA, the well-known Argentine naturalist and explorer, whose death occurred towards the end of last year in the forest near Mira Flores in the Grân Chaco; Dr A. ZIMMETER, Professor of Botany at Innsbruck, aged 49; A. J. HORACE PELLETIER, at Madon, France, in 1897, a lawyer who worked on noxious insects; JOSÉ D'ANCHIETÁ, who had made zoological collections and observations in the African possessions of Portugal, at Caconda in Angola, on September 14, aged 66; EMMANUEL MARTIN, lepidopterist, at Creil, France, in 1897; J. HOYES PANTON, Professor of Natural History at the Ontario School of Agriculture and a writer on fossil mammals, on March 2; at New York on November 21, aged 54, General ALBERT ORDWAY, who had written on the Crustacea, especially the genus *Callinectes*; the metallurgist, Prof. KNUD STYFFE, aged 74, for twenty-five years director of the Stockholm Technical College; Dr K. B. JACOB FORSSELL, the lichenologist, at Karlstad, Sweden, on February 11; Prof. KIRK, author of the "Forest Flora of New Zealand," and of other monographs upon the trees of those islands; ALPHONSE BRIART, the well-known Belgian geologist, on March 15, aged 73.

NEWS

THE following are among recent appointments:— Henry Hanna, to be demonstrator in biology, geology, palaeontology, of the Royal College of Science, Dublin; Dr H. W. M. Tims, to be professor of zoology at Bedford College for Women, London; Dr Hanson Kelly Corning, as professor of anatomy at Basle University; Dr Grafin Maria Linden, as second assistant in zoology at Tubingen University; Dr Paul Schiemenz, as director of the Biological and Fish-Culture Station at Muggelsee; Dr Johannes Thiele, of Strasburg, will succeed him at Berlin as assistant in the Zoological Department of the High School for Forestry; H. C. Bumpus, of Brown University, to be scientific director of Wood's Holl Station, U.S.A.; Dr Charles R. Barnes, of Wisconsin University, to be professor of plant physiology in the University of Chicago; Madame Lemaire, to be professor of drawing applied to plant study, at the Muséum d'Histoire Naturelle at Paris; Surgeon-Major David Prain, to be superintendent of the Royal Botanical Gardens, Calcutta, in succession to George King, retired; Dr Eugenis Serra, as assistant in the Botanical Gardens of Palermo; Professor Arthur Borntraeger, of the School of Agriculture at Portici, to be director of the Agrarian Station in Palermo; Dr H. F. Harris, as professor of bacteriology at Jefferson College; Dr J. L. C. Schroeder van der Kolk, as professor of mineralogy in Delft Polytechnic; Emil Wiechert succeeds in part the late Professor E. Schering at Gottingen University; the professorship of astronomy is now distinct from geophysics and earth magnetism, the section presided over by Dr Wiechert; L. V. Pirsson, to be professor of physical geology at Yale University, and not at Harvard, as stated in our April number.

THE Seventh International Congress of Geography is to be held at Berlin in 1899.

THE Museum of Bonn University has received the anthropological collection of Schaffhausen.

THE University of St Andrews intends to establish a professorship in Physiology, and in Anthropology and Anatomy.

PROF. G. B. HOWES has received the degree of LL.D. *honoris causa*, and Mr A. T. Masterman that of D.Sc., from the University of St Andrews.

THE collection of mammals made by the late Dr Harrison Allen was bequeathed by him to the Academy of Natural Sciences of Philadelphia.

DR HUGO BUCKING and Dr L. van Werveke have started for an eight months' expedition to the Netherland East Indies on behalf of a Dutch Society.

WE regret to learn that La Fayette College had a large part of its scientific equipment and all of the herbarium destroyed by fire on December 18th.

THE Eighth Annual Meeting of the German Zoological Society is to be held at Heidelberg, June 1st to 3rd, under the presidency of Prof. F. E. Schulze.

WE are glad to learn that Mr W. P. Pycraft has been elected an Associate of the Linnaean Society. Mr Pycraft has been busy of late in the preparation of a key to the osteology of birds.

THE Swedish Anthropological and Geographical Society has awarded the Vega Stipend to Mr J. Stadling, who has started on a journey through Siberia in search of possible traces of Andrée's balloon expedition.

MR EDOUARD FOA has travelled across Africa by the basin of the Zambesi, Lake Tanganyika, and the Congo, and has brought back numerous specimens of anthropological interest from the region of the great lakes.

DR S. SCHÖNLAND, the Director of the Albany Museum, Grahamstown, left on March 18th for a three or four months' trip to Europe. His address will be :—Per Adr. Herrn Schonland, Frankenhausen (Kyffh.), Germany.

THE ss. 'Belgica' with the Belgium Antarctic Expedition has grounded among the group of islands near Cape Horn, in consequence of which Lieut. Gerlache will not proceed further than the Shetland Isles in the Antarctic this year.

MAGDALEN COLLEGE, Oxford, announces that a fellowship in medical science will be given by the college next October. There will be an examination in sciences relating to medicine, and original writings may be submitted by candidates.

DR OTTO NORDENSKIÖLD and Dr Gunnar Andersson, of Stockholm University, with four scientific assistants, left Sweden at the end of March for Klondike, intending to be away about two years, and to make a thorough scientific exploration of the country.

DR KISHINOUYE, the representative of Japan at the Bergen Fisheries Exhibition, reached London in March from the United States. He has installed the Japanese exhibit at Bergen, and has returned to work at the Natural History Museum, London.

MR KRUPP of Essen has given 10,000 marks to the Berlin Geographical Society for a gold medal to be awarded yearly for geographical discovery. It is to be called after Gustave Nachtigall, the African explorer, and preference will be given to discoveries on the African continent.

IN recognition of the services rendered to the Zoological Society of Dublin by the late Dr Samuel Haughton it is proposed to erect a memorial building in the gardens of the Society. Subscriptions for this purpose may be sent to Prof. D. J. Cunningham, Hon. Sec. to the Royal Society, Dublin.

WITH reference to the resignation of Dr R. Semon, already announced by us, the *American Naturalist* remarks: "The monotremes and *Ceratodus* seem veritable 'hoodoos.' The material obtained a dozen years ago by Mr Caldwell, aided by Royal Society funds, is lying unused, and with no prospect of being studied."

AMONG forthcoming arrangements at the Royal Institution, we notice that on May 6th Mr E. A. Minchin is to lecture at nine o'clock on "Living Crystals," which those who know Mr Minchin's admirable work will readily interpret as the growth of sponges. On May 21st and 28th, at 3 P.M., Mr J. Arthur Thomson will lecture on "The Biology of Spring."

PROF. GRIMEAUX, of the *École Polytechnique*, Paris, has been deprived of his post for having given evidence at the trial of Zola; and Dr R. H. Warren, State Zoologist of Pennsylvania, has resigned because his political opinions differ from those of Governor Hastings. Democratic Government does not appear to be favourable to the scientific attitude of mind.

THE following are announced as Presidents of Sections at the forthcoming meeting of the British Association :—Physics, Prof. W. E. Ayrton; Chemistry, Prof. F. R. Japp; Geology, Mr W. H. Huddleston; Biology, Prof. W. F. R. Weldon; Economics, Dr J. Bonar; Mechanics, Sir John Wolfe-Barry; Anthropology, E. W. Brabrook; Botany, Prof. F. O. Bower.

THE Museum of Nantes has received the addition of a hall sixty metres long. Dr L. Bureau is attempting to display as complete a representation as possible of the fauna, flora, and geology of the West of France. In this attempt, says *La Feuille des Jeunes Naturalistes*, the richness of his collections renders success more than probable.

WE also learn from *La Feuille* that Mr Lennier, Director of the Havre Museum, intends to exhibit in the galleries of the second storey, which have recently been furnished, the ethnographical collections made by D'Entrecasteaux, to which he will add numerous specimens of similar character already possessed by the museum. He is only waiting for money enough to buy glass cases.

THE City of New York undertook to provide the New York Zoological Society with a site for a zoological garden on the condition that the Society raised the sum of $250,000 for buildings and collections, of which sum $100,000 had to be obtained before the 24th of March last. This sum having been obtained, chiefly by means of large subscriptions from millionaires of the city, the Society can now take possession of the site.

WE learn from *Science* that Prof. Nils E. Hansen, Professor of Horticulture at Brookings, South Dakota, who was despatched by Secretary Wilson of the Agricultural Department of the United States, to secure new, rare, and valuable seeds, has returned from a journey through Eastern Russia, Trans-Caucasia, Russian Turkestan, Western China, and Siberia, having obtained about three car-loads of seeds, which will be distributed to State experiment stations and others, chiefly for use in the arid regions.

THE following lectures, with lantern illustrations, are being delivered in the Whitechapel Museum :—On Tuesday, April 12th, at 8 P.M., "The Horse and Dog and their Relations and Friends," by Prof. Hobday; Tuesday, May 10th, at 8 P.M., "Butterflies," by Prof. W. F. R. Welldon; on Tuesday, June 7th, at 8 P.M., "A Piece of Wood," by Prof. Marshall Ward. Admission free by ticket, to be obtained in the Museum and Lending Library. The Museum is open to the public daily from 3 P.M. to 10 P.M., Sundays included, and on Saturdays from 10 A.M. to 10 P.M.

THERE has for some time been forming in New York what is termed a Scientific Alliance, that is to say a union of various scientific societies in the city, for various purposes, the chief of which is the erection of a building in which their meetings can be held. A design prepared by R. W. Gibson is published in *Science* for March 25, and an appeal is made to the public-spirited citizens of New York for financial support. Another way of promoting this excellent object was a dinner held at the Hotel Savoy, on March 16, with Mr C. F. Cox, President of the Council of the Alliance, in the chair.

THE following have been appointed on a Select Committee of the House of Commons to enquire into and report upon the administration and cost of the Museums of the Science and Art Department :—Lord Balcarres, Mr Bartley, Sir Mancherjee Bhownaggree, Mr John Burns, Mr Daly, Dr Farquharson, Sir John Gorst, Mr Ernest Gray, Sir Henry Howorth, Mr Humphreys-Owen, Mr Kendrick, Mr Platt-Higgins, Sir Francis Powell, Mr Woodall, and Mr Yoxall. The deliberations of this Committee's predecessor have had some effect, for the Government intends at last to complete the buildings at South Kensington, and are asking for that purpose a sum of £800,000.

WE learn from *Nature Notes* that under the title of Audubon Societies there have been formed in the United States no less than fourteen bodies, the main purpose of which is to discourage buying and wearing for ornamental purposes the feathers of any wild bird. The Massachusetts States Legislature has passed a bill prohibiting the wearing of song and insectivorous birds on women's hats, and the law is being vigorously enforced in the city of Boston. An official of the Boston Natural History Society Museum has published a list of no less than forty species of birds which he saw stuck on women's heads. The latest victim of fashion is said by the same official to be the grebe. The great crested grebe and the little grebe have been called the greatest ornithological ornaments of our inland waters. After this we suppose those waters will miss their ornaments. Andrée Goddard, writing in the *Revue Scientifique*, says, "It is high time, if not indeed too late, to add to the precepts of the Bible and the Koran, and to substitute for the poetic legends that have hitherto prohibited massacre, a scientific and social organisation for the preservation of bird species."

We learn from the *Scientific American* that a rival to Klondike has arisen in the village of Hadley, Warren County, New York. This is the centre of a district in which a large amount of gold is believed to occur. It is found in a fine state adherent to grains of sand. At some places the sand is found at a depth of 25 feet, and at other points still deeper. The gold is separated by the quicksilver process after the sand has been pulverised, and it is believed that the yield will be equal to $4 for each ton passed through the crusher. It is intended also to try a process called the combination cyanide-chlorination method. More than one company has been started, and claims have been made by thousands of prosecutors. It is also stated that there has been formed in the United States an Electrolytic Marine Salts Company for the purpose of extracting gold and silver from sea-water. It has of course been known for some years that those metals occur in minute quantities in the sea, but it has been supposed that they could not repay the labour necessary for extracting them. It is said that a large number of electrolytic apparatus are already in operation and that each obtains gold to the extent of £24 a day.

Prof. Seeley's Geological Field Class has arranged this year for a series of excursions, especially to illustrate the physical geography and geology of the Thames Basin. This is the thirteenth year of the Society's existence, and its continued growth testifies greatly to the interest aroused in the subject by the method of study adopted. The London area is not a particularly good centre for class field work, in spite of the extreme interest of its geological history. The absence of volcanic and plutonic rocks, and of any deposits earlier than the Middle Mesozoic, puts one serious limitation, and the great width of the suburban belt renders excursions lengthy and costly It is, therefore, all the more pleasing to find that Prof. Seeley's Field Class continues such vigorous life, for it no doubt means that during successive years a large number of students have profited by the course of studies. This year's subject will be illustrated by excursions along a line from the Upper Jurassic rocks of Aylesbury, across the Chiltern Chalk hills, over the Cainozoic deposits of the Thames synclinal to the North Downs, and finally to the Wealden beds around Cuckfield. The honorary secretary of the Society is Mr R. H. Bentley, of 43 Gloucester Road, South Hornsey, N.

At the Annual Meeting of the Norfolk and Norwich Naturalists' Society, held on March 29, Mr T. H. Gurney was elected president. The retiring president, Mr Arthur W. Preston, delivered an address, in the course of which he dealt with the weather of the past ten years, and its effect upon the leafing and blossoming of trees and plants. He handed round some carefully prepared tables showing the main features of the temperature and rainfall, the direction of the wind, and the dates of first leafing of trees, and first flowering of plants, both garden and indigenous, during the period. These illustrated the marked effect that temperature had on the development of vegetable life. In 1888, when the mean temperature of every month, from January to August inclusive, was below the average, the greatest number of latest dates of first flowering were recorded up to the conclusion of harvest, and the same occurred in 1891, though in a less marked degree. On the other hand, in 1893, when every month from February to August was above its average temperature, a large majority of the earliest phenological dates were recorded. In other years, when certain months were marked by extremes of temperature, but were followed by months of an opposite character, there was not much departure from the average of the dates of first flowering.

On June 2nd, 3rd, and 4th, the third Annual Congress of the South-Eastern Union of Scientific Societies will be held in the Town Hall, Croydon. On Thursday night, June 2nd, Prof. G. S. Boulger will give the Presidential Address. The following papers will be read and discussed :—"Entomology as a Scientific Pursuit," by J. W. Tutt; "Ancient and Modern Dene Holes and their Makers," and "Natural Gas in Sussex," by C. Dawson; "Photography in Relation to Science, with Lantern Illustrations," by J. H. Baldock; "The Place of Geology in Education," by Prof. J. Logan Lobley; "The Soil in connection with the distribution of Plants and Animals," by H. Franklin Parsons; "Life History of the Tiger Beetle, with Lantern Illustrations," by Fred. Enock; "New Methods of preparing Fossils, with Demonstrations," by Dr A. W. Rowe; "Ideals for Natural History Societies, and how to attain them," by J. M. Hobson; "Botanical Work still wanting Workers," by E. M. Holmes. There will be a conversazione on Friday evening. A meeting of delegates from affiliated societies will be held on Saturday at 10.30. Tickets (admittting to all meetings): Members of affiliated societies, 2s. 6d. each; persons unattached, 3s. 6d. each. Applications for these should be made to the Hon. Gen. Sec., Dr G. Abbott, 33 Upper Grosvenor Road, Tunbridge Wells; C. Poulett Harris, Hon. Sec. of Local Committee; and R. F. Grundy, Hon. Sec. of Croydon M. & N. H. Club.

The Lincolnshire Science Society, which has been the subject of so much correspondence in various papers, puts forward the following objects of study for its members during the ensuing year :—Geologists are asked to limit their attention to the Glacial Beds that occur in those portions of the Trent and Witham Valleys that lie within a ten-mile radius of Lincoln. The most important problems connected with these are the determination of their age by their topographical position, the collection of the mammalian and other remains that occasionally occur, and the recording of the erratic boulders of the district. Botanists should devote special attention to the careful recording of the Algae and Fungi found within ten miles of the city of Lincoln. The mounting of permanent specimens of the most uncommon species is essential. A list of flowering plants found within the same radius, arranged according to their natural orders, with the time when found in flower, and the exact locality, is also a desideratum. Notes on the insects found visiting the flowers would also be useful in forming a county record. The meteorologists are to study the rainfall of Lincolnshire in connection with mortality tables. Zoologists are requested to direct attention to the breeding localities of the more uncommon birds of the county. Special attention should be directed to the immediate neighbourhood of Lincoln. An investigation of the plant seeds, insects, &c., found in the stomachs of birds would be of great value from an economic point of view. Members will find some interesting information bearing on this point in some papers recently published by the Board ot Agriculture. Entomologists who, it is interesting to notice, are carefully separated from zoologists, are expected to verify all records of Lepidoptera in the separate divisions of the county. The photographers may make a photographic survey of the City of Lincoln, especially of features likely to be swept away in the course of street improvements. A programme of many interesting excursions to be undertaken during the summer has been sent to us.

CORRESPONDENCE

THE BRITISH MUSEUM CATALOGUE OF MOTHS

I HAVE read, in common with doubtless most lepidopterists, the circular relating to the publication of a series of volumes on the Lepidoptera Phalaenae with interest, in my case heightened by the fact that types of more than five hundred species described by myself are contained in the British Museum collection. It is owing to this interest, felt in the success of the undertaking, that I wish to protest against the resolution expressed in paragraph 10 of the circular, that the classification will follow that adopted by Mr E. Meyrick in his recent work on British Lepidoptera. May I suggest at once, that in a work like that proposed, a wise conservatism in the choice of the arrangement of the material will best subserve its purposes? In the present instance the choice of Mr Meyrick's classification is peculiarly unfortunate, since this rests upon speculations which the results of recent research stamp as improbabilities. From Mr Meyrick's work it appears certain that (1) it does not afford a working theory as to the evolutionary changes of the neuration upon which the classification is nevertheless apparently based, while Mr Meyrick fails to give sufficient or reasonable proof for the probability of his sequences, and also (2) that the figures of neuration with which Mr Meyrick's writings are interspersed are quite inaccurate and misleading so far as I have been able to compare them with the originals. This is not a matter of opinion but of ocular demonstration. I have published a plate in the *Illustrirte Wochenschrift fur Entomologie*, to which I invite the attention of British entomologists, and upon which I reproduce, by photographic process, Mr Meyrick's figure of the neuration of *Venilia macularia* as published in the *Transactions of the Entomological Society*. Side by side I give the actual neuration of the species, and it is sufficient to say of the two figures that any conclusions or any systems based upon their respective features must run diametrically opposite in all important particulars. Nor is this figure of Mr Meyrick's an exception or an especially unfavourable specimen. The figures in the 'Handbook,' of the diurnals especially, which I have compared, are caricatures in greatest part. The result of recent investigation, as a whole, far from lending itself to any such phylogenetic speculations as have been published by Mr Meyrick and are laid down in paragraph 10 of the Prospectus, runs often counter to any such assumed relationships between the groups. One of the most notable efforts of the present time is Dr Dyar's use of the position of the larval tubercles as a guide in defining taxonomically the larger Linnean groups. In brief, Dr Dyar shows us that the Sphingides, Saturniades and Bombycides (including Noctuidae and Geometridae) are on larval characters separate and homogeneous groups, and that only certain families, such as the Sesiadae, Anthroceridae, Cossidae, Psychidae, etc., are to be removed to the Tineides, their affinity with this latter group having been previously noticed by different observers from other characters than the position of the tubercles. Further, it becomes probable, through the researches of Dr Chapman, that the phylogeny of the larger groups above cited, and including the diurnals, leads independently to ancient forms, of which the Tineides are the modern, less changed survival. It seems thus that the day butterflies, the Hawk Moths and the Emperor Moths may have their separate origin in the Tineid group, while the taxonomic character revealed by Dr Dyar confirms their respective homogeneity. The supposed phylogenetic lines by which Mr Meyrick traces the descent, as given in paragraph 10, are drawn without reference to the characters brought forward by Dr Dyar. The most serious objection to their adoption lies, however, in their being insufficiently or not motived by Mr Meyrick. Indeed, it is easy to show that, in the diurnals of Mr Meyrick's 'Handbook,' the characters of neuration are not understood by the writer, in that the generalised form is made on occasion to proceed from the specialised, instead of the reverse.

But it cannot be my intention here, nor will it serve my purpose, to enter into a detailed criticism of Mr Meyrick's writings. It is sufficient to show that Mr Meyrick's arrangements are unsafe and should clearly not be trusted to in a work of the general character proposed by the British Museum. This work should recognise the main groups in the sequence adopted, not by Mr Meyrick, but by Linné and Fabricius, while any changes in the composition of these groups should be limited to such for which a large amount of evidence has been recorded. It is, philosophically speaking, certain that a linear arrangement is in itself unnatural, but the practical difficulty of reflecting the probable phylogeny in our books is not obviated by adopting Mr Meyrick's 'lines' in the present case. To trust to these will, it appears to me, endanger the undertaking and set the whole work adrift at the outset, while their employment will prevent many, through lack of confidence, from embarking and sharing in the labours of the voyage.

A. Radcliffe Grote.

Roemer Museum, Hildesheim.

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NATURAL SCIENCE

A MONTHLY REVIEW OF SCIENTIFIC PROGRESS

JUNE 1898

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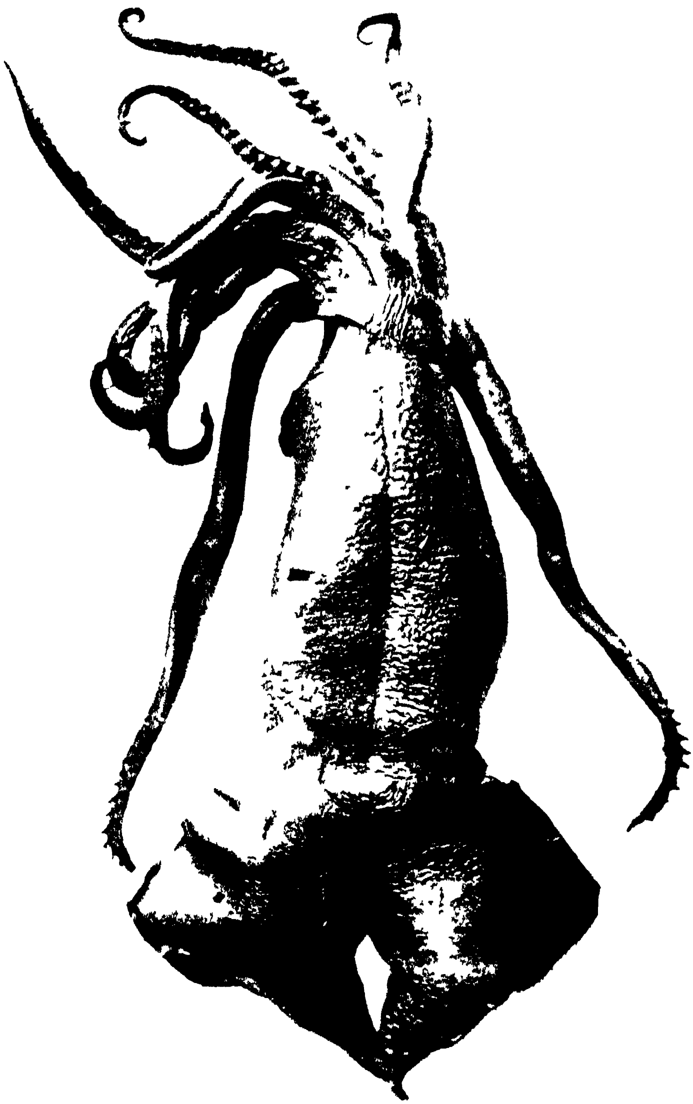

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NATURAL SCIENCE

A Monthly Review of Scientific Progress

No. 76—Vol. XII—June 1898

NOTES AND COMMENTS

The Museum of Practical Geology

The House of Commons Committee on the museums of the Science and Art Department has reported unanimously in favour of transferring the collections of rocks and fossils from the museum in Jermyn Street to South Kensington. The removal of the collections from the present headquarters of the Geological Survey has long been foreseen as inevitable. Those who know the conditions under which the vast accumulation of specimens, constantly being added to by the energetic officers of the Survey, is housed, and the difficulties of space, lighting, and so forth under which the scientific staff of that establishment has to work, as well as the great danger from fire, will not regret the removal of the collections and offices to some safer and more spacious quarters; least of all we imagine will such a change be objected to by the members of the Survey themselves. It is no doubt possible that some of the public, who find the present Jermyn Street Museum within easy reach, may complain of its removal to the wilds of South Kensington; indeed, protests have already been raised. But really it is more important that all the specimens should be accessible to a few than that only a few of them should be accessible to many.

But before expressing a final opinion upon this recommendation of the Committee, we should like to know what they mean by South Kensington, or even that portion of it which they designate as "on the west side of Exhibition Road." We will take it for granted that they do not mean the Imperial Institute, although even that establishment has found defenders lately. Neither do we suppose that it is contemplated to make a simple translation of the Museum of Practical Geology along with the offices of the Survey; for although there is ground still unoccupied at South Kensington, it is already appropriated for the future growth of institutions already on the spot. The intention, therefore, must be to incorporate the collections of the Survey with one or other of the existing museums at South Kensington, while keeping them available for constant and ready reference by the officers of the Survey. The Geological Survey, of course, has always been a branch of the Science and Art Department, and it might be supposed that the natural incorporation would be with what is usually called the South Kensington Museum, to which many of the models and appliances formerly at Jermyn Street have already been transferred. But it must be remembered that the scientific collections of the South Kensington Museum do not embrace objects of natural history, except in so far as they are definitely connected with education. The natural history collections of the country are preserved in the British Museum, in that branch of it now generally known as the Natural History Museum. It would therefore seem more fitting that the collections in question should be placed in the keeping of the Trustees of the British Museum.

We do not know exactly how the proposed alteration by the Committee will be received by those already in possession at the South Kensington Museum and at the Natural History Museum. We can imagine the existence of a little jealousy, counterbalanced by the fear of being overburdened with work; and we can understand that there should be considerable indisposition to yield valuable space without an equivalent return. With these matters an independent scientific Review has no concern. It is our part to consider what is best for the public and for the advance of science. Now, it is generally recognised that, in spite of the ardent efforts of the two or three hard-working gentlemen whose business it is to name fossils at Jermyn Street, the lists of fossils that are published by the Survey in its memoirs are not so reliable as they might be. We do not for a moment blame the gentlemen just referred to. They do as much as the difficulties of their position permit them; the fault lies with the system. What we want to see is the training of a school of broad-minded specialists, whether amateur or professional, and the co-operation of those specialists in the advancement of British palaeontology. Only in this way are the fossils of our strata likely to be identified with sufficient exactitude to be a true guide to the practical geologist and the miner; only thus shall we ever solve those wonderful problems of detailed stratigraphy and palaeo-geography, the very existence of which we are just beginning to realise; only thus will fresh light be thrown on the fascinating enigmas of the procession of life. It is obvious that the nucleus of such a band of specialists already exists at the Natural History Museum, and nowhere else; and that museum is already the head-quarters of many palaeontological specialists who are not on its staff. There, too, is to be found in greatest completeness that indispensable aid, a good library. Therefore, it seems to us that what is wanted is to bring those specialists and the facilities they possess into touch with the Survey collections. This, of course, would involve the increase of the staff of the British Museum, at least by the addition of the palaeontologists of the Survey, and, if good work be really desired, by two or three more.

We do not advocate, and we do not know anyone that advocates, the severance of the Survey collections from the surveyors, whose publications must always be based on the evidence of these particular rocks and fossils; and no one can be desirous of distributing the geological collections among those already in the British Museum, which latter are arranged primarily in accordance with zoological classification. Such a distribution would be a severe blow to the study of geology, and would quite do away with any advantage that might be gained by the change of site. No! the great need already at the Natural History Museum is for a geological and stratigraphical series. This gap might be filled by the Jermyn Street collections. We should like to see the erection of the eastern wing of the Natural History Museum, and the installation therein of a museum of stratigraphic and dynamical geology, chiefly illustrating the history of our own islands. We would have a stratigraphical series more complete and detailed than at present is possible at Jermyn Street. What an object-lesson it would be to see the faunas of every zone displayed in an ascending series! What an impulse it would give to more exact geological investigation, and how enormously this in its turn would benefit the students of the different taxonomic groups, both of animals and of plants!

There is, as we said at the beginning, little doubt that the recommendations of the Committee will be carried out in one way or another before very long. In what precise way and with what spirit remains to be seen. We believe that the greatest improvement will be effected by a change on the lines here sketched out, if only that change can be made without harmful friction.

Mr Herbert Spencer on Polar Bodies: a Postscript

To the article on Cell-Physiology published in our May number, Mr Herbert Spencer wishes to add a paragraph, containing (so he writes to us) "a clinching argument." It runs thus:—"A test fact remains. Sometimes the first polar body extruded undergoes fission while the second is being formed. This can have nothing to do with reducing the number of chromosomes in the ovum. Unquestionably, however, this change is included with the preceding changes in one transaction, effected by one influence. If, then, it is irrelevant to the decrease of chromosomes, so must the preceding changes be irrelevant: the hypothesis lapses. Contrariwise this fact supports the view suggested above. That extrusion of a polar body is a process of cell-fission is congruous with the fact that another fission occurs after extrusion. And that this occurs irregularly shows that the vital activities seen in cell-growth and cell-multiplication now succeed in producing further fission of the dwarfed cell and now fail; the energies causing asexual multiplication are exhausted and there arises the state which initiates sexual multiplication."

A Quilted Squid

The general zoological results of the Swedish expedition to Tierra del Fuego were described in our own pages by Dr Axel Ohlin. The collections made were entrusted to specialists, and the detailed conclusions are now being issued as they are completed in an octavo publication entitled, "Svenska Expeditionen till Magellansländerna." Of this we have just received Vol. ii., No. 4 (pp. 49-64, pls. iv., v.), which is an account of the Cephalopoda by Dr Einar Lonnberg. The species collected were *Octopus fontanianus*, d'Orb.; *O. patagonicus*, n. sp.; *Gonatus antarcticus*, n. sp., which appears to be the southern representative of the northern *G. fabricii*; and two almost complete specimens of *Onychoteuthis ingens*, E. A. Smith (1881), of which only the head was known previously. Through the courtesy of Dr Lonnberg we are able to present our readers with the portrait of one of these specimens (Plate XI.).

The chief interest of Dr Lonnberg's paper lies in its account of the integument of the last-mentioned species. The surface of the mantle and of the head presents a peculiar warty appearance, like a cobble-stone pavement. This is due to the presence of large subcutaneous papillae, between which the skin has sunk after being placed in formalin. The structure of the mantle is thus described, from within outwards: A thick coat of circular or transverse muscles; a thin layer of longitudinal muscles; a layer of connective tissue of about the same thickness, and containing a large number of cell-corpuscles, nuclei, and blood-vessels; on this are situated the papillae, which rise with steep sides from a broad base, and have a flat upper surface; between the papillae is delicate connective tissue; lastly, outside the papillae, comes the skin proper, consisting of connective tissue, muscles running in various directions, chromatophores, and epithelium. The papillae have a height of 1 millimetre or a little more, but vary in size and shape, some having a diameter of 2·5 millimetres, others as much as 5 millimetres. Each papilla is limited by thin but dense layers of elastic fibrils and fibrils of connective tissue. The main mass of the papilla is a network of thin, long, wavy, elastic fibrils, which enclose large irregularly elliptical meshes, in which are large round connective tissue cells; the structure is penetrated by blood-vessels and rather large nerves, and appears to be surrounded by circular muscles, which can contract the area but increase the height of each papilla. The connective tissue between the papillae contains large oval cells in a gelatinous matrix supported by a network of very fine fibrils; it contains many fat-corpuscles.

The function of this peculiar quilted padding is not obvious. The abundance of nerves in the papillae leads Dr Lonnberg to regard it as a sensory organ for noting changes of hydrostatic pressure, and he compares it with the 'terminal papilla' of *Spirula reticulata*. The oily fat stored up in the layer is probably to be explained as reserve material.

Nothing precisely like this papillose layer has ever been described, although there is a "thick glassy-looking subcutaneous layer" in *Mastigoteuthis lorimana*, and a gelatinous tissue with elastic fibrils and regulating muscles in *Alloposus mollis*, while certain "lenticular, glandular bodies" observed by Steenstrup in several species of *Ommatostrephes* may be similar papillae. There is, however, one form upon which the observations of Dr Lonnberg throw unexpected light. Not long ago Dr L. Joubin, under the name *Lepidoteuthis*, described a squid supposed to be covered with solid scales, forming "une véritable cuirasse qui donne à l'animal un aspect étrange, rappelant certains poissons Ganoides ou la carapace de divers fossiles." The specimen came from the stomach of a sperm-whale, and Dr Lonnberg maintains, with much show of reason, that the supposed scales are "only subcutaneous papillae from which the covering skin has been removed by the influence of the digestive fluids." In fact Dr Joubin's own description of the 'scales' accords exactly with Dr Lonnberg's description of the subcutaneous papillae.

CHEESE TO ORDER

WHILE Messrs Babcock & Russell, as explained in our March number (p. 151), have been working towards the conclusion that the omnipresent microbe has less to do with the maturation of cheese than is usually supposed, that, in short, the whole process can be accomplished without his aid, an energetic experimenter on this side of the Atlantic, Dr Olav Johan Olsen of Norway, has done something more than to rehabilitate the cheese-bacteria; he has actually put them into harness, and an account of his success is contributed by his assistant, Miss Thora Scheel, to the March number of *Naturen*. Hitherto, cheese has been left to do its own fermenting by means of such bacteria as its varying constituents, the temperature and moisture of its store-house, and various external conditions might permit. Dairymen had an idea of the proper course to pursue under the special circumstances of their locality; but they worked blindfolded, and their calculations were liable to be upset by unregarded variations of environment. Dr Olsen has changed all this. He has investigated various cheeses, and has caught and cultivated their microbes; then he has reversed the process, and used his cultures to produce the various cheeses from which he started. The kinds of microbes are not many, but by their combinations in different proportions, different results are obtained. First there are the common fermentation-microbes, common to all cheese, but replaceable, as Babcock & Russell have shown, by unorganised ferments. Then come the species that affect the diagnostic characters of the future cheese, its peculiar taste and smell. Cheesemaking now goes by the card. The milk is sterilised and heated to 70°-75° C., and the store-room is kept guarded against foreign microbes. Those that are desired are added in the requisite proportions, and their vigorous growth is of itself enough to overcome the influence of accidental strays. The production of the kinds of cheese is no longer an affair of the laboratory; but Dr Olsen will take your order for Gorgonzola, Stilton, or Camembert, and will furnish the precise description required at a cost satisfactory to your pocket and to his own. Norway is a land specially adapted to the industry of cheesemaking, and as Norwegian prophets differ from those of other countries in securing the hearty recognition of their countrymen, there is no doubt that Dr Olsen's discoveries will be rapidly taken up in practice. We wish therefore to draw the attention of the British farmer to them without delay.

Fishy Water

We referred in our last number to a paper by Messrs Jackson & Ellms on "Odours and Tastes of Surface Waters." We learn from *The Plant World* that the city of Brooklyn, N.Y., has had much trouble with its drinking water, complaint being made, especially during the summer months, of its objectionable appearance, strong oily taste, and what Trinculo would have called its "very ancient and fish-like smell." The causes of this condition have been investigated by Professor Leeds, of the Stevens Institute of Technology. He finds that the whole trouble is due to the presence, in immense numbers, of a diatom, *Asterionella flavor*. In some samples of water as many as twenty million individuals to the gallon were found. "Think of it; drink of it then if you can." This diatom is enclosed in the usual silicious skeleton, and has the power of secreting a substance in the nature of an oil, which possesses the peculiar taste and smell above described. This oily, taste-producing substance is volatile, and cannot be got rid of by distillation. The water itself was found to be colourless, the apparent colour being due to the suspended organisms. The problem of purification of the water is naturally an important one. Filtration was of no avail, and aerating it only tended to aggravate the mischief. The sole remedy that has proved effectual so far is that of excluding the light, and converting the reservoirs into subterranean basins; for *Asterionella* cannot live in darkness.

SAMPLING THE HERRING POND

DURING the trip to America in connection with the British Association meeting at Toronto last year, Prof. Herdman made a continuous collection of the little free-swimming organisms constituting the plankton, both on the outward and homeward journeys. The method adopted for this purpose was that of placing nets over a tap and over-flow pipe bringing water from the bottom and top respectively of a tank kept full of sea-water by a pump working continuously. The material strained from the water thus passing through the nets was collected twice daily and preserved for detailed study. The examination has since been made and the results published in the *Transactions* of the Liverpool Biological Society (Vol. xii. pp. 33-90). From the lists of species there given, a very good notion of the more important of the smaller kinds of organisms inhabiting the North Atlantic during the summer may be obtained. One can also perceive the changes which occur in the character of the plankton from point to point, although this would have been much more readily seen if the results had been issued in tabular form. The Copepoda, which form the most important constituent of the collections, have been specially worked out by Messrs I. C. Thompson and A. Scott, and include three species new to science. These latter, however, were taken not in the open Atlantic, but in the St Lawrence.

As remarked by Prof. Herdman, this method of collecting samples of the surface fauna, even from an ocean-liner going at full-speed, will enable naturalists to obtain, at very slight expense, a series of gatherings across the great oceans in every direction traversed by passenger or cargo steamers. The ship's surgeon, or any other officer who may be willing to take charge of a net and set of collecting bottles for a marine biologist, can now help in making an interesting series of observations which may lead to important conclusions as to the distribution of oceanic organisms. And as the Copepoda at least are edible (some were actually cooked and eaten during the trip) the cook has here the means of adding a new dish, or at any rate a sauce, to the bill of fare.

The Drift of the Channel

THE paper of most interest in the last number of the *Journal of the Marine Biological Association of the United Kingdom*, issued at the end of April, is a "Report on the Surface Drift of the English Channel and Neighbouring Seas during 1897," by Mr W. Garstang. It will be remembered that about a year ago the Director of the Plymouth Laboratory instituted an investigation by means of soda-water bottles, which were cast upon the waters, and which, we are glad to say, were in a large number of cases returned to him after many days. The locality where each bottle started upon its journey was accurately recorded, as well as the date on which it started. By means of a numbered post-card enclosed in the bottle, information as to the place reached by it, with some indication of the date, was returned to Mr Allen. The whole Report is of remarkable interest, but we must confine ourselves here to quoting the conclusions in Mr Garstang's own words :—

"Enough has been said, I think, to show that the method employed here for tracing the actual influence of the winds on the water is sufficiently accurate for practical purposes, and that by its employment, with proper precautions, the influence of the winds may be separated from that of other factors which operate in the production of surface currents. From this point of view the method may be of considerable use in the future for determining the existence of currents not produced by local wind-action. At the same time the method requires to be tested extensively before it can be used as a basis for conclusions. The present report pretends only to show that the relation between wind-action and surface currents is capable of quantitative study, and that the results obtained by the use of the methods here described are sufficiently accurate to encourage the further use of them. This we are doing during the present year on a larger scale, and the results will be set out in next year's report. It is very desirable that experiments should be made to determine the depth of the currents induced by wind-action, and we propose to attempt this work during the present year. A comparison of results obtained by bottles floating at the surface, and by other objects designed to come under the influence of lower strata of water, should yield results of considerable value. Until such experiments are made, however, it does not appear to be desirable to say too much upon the practical aspects of the experiments described in this report. We have obtained a general view of the movements of the uppermost layer of water, and we may be certain that similar, though slower, movements also affect the layers immediately subjacent ; but the actual depth to which this movement would be communicated under different conditions of wind and tide is a matter of too much practical importance to be left to mere guesswork. As Mohn has well said: 'Neither argument nor estimate, but carefully worked-out computations alone, can lead to a lasting result.'"

The Protection of Birds

Following on the action of the Massachusetts' State Legislature, noticed in our last number, the United States Senate has passed a bill for the protection of song-birds, providing that the importation into the United States of birds, feathers, or parts of birds, for ornamental purposes be prohibited, and prohibiting the transportation or sale of such articles in any territory of the United States or in the district of Columbia. Considering that the American lady can no longer bring in cheap sealskins or wear a *matinée* hat, this last blow will probably have the effect of seriously decreasing the female population of the United States.

At a recent meeting of the New York Academy of Sciences Mr W. T. Hornaday related the results he had obtained in reply to questions circulated by him in all parts of the United States with reference to the destruction of bird life. It appears that the most destructive agencies are sportsmen, plume-hunters, boys after eggs, pot-hunters, fire, and English sparrows; and through these it has been estimated that there has been a decrease of 46 per cent. during the last fifteen years. It is shown that game and edible birds are becoming scarce, and that song-birds are being used for food in their stead; that plume-birds are becoming extinct, and that destructive agencies are increasing. Mr Hornaday concluded with an appeal for more drastic measures in the game-laws, and for their careful enforcement.

Turning to our own country, we note that the indefatigable Secretary of the Society for the Protection of Birds has induced a score or so of prominent ornithologists to pen a series of brief but practical pamphlets intended for the man on the street, who has seldom much anxiety to preserve wild birds from the hand of the destroyer. The names of such accomplished naturalists as Messrs O. V. Aplin, H. E. Dresser, J. A. Harvie Brown, Howard Saunders, and T. Southwell are a sufficient guarantee that the majority of these papers are first-rate. They are published at the office of *Knowledge*, 326 High Holborn, London, W.C., and we wish all success to their circulation.

Desirous of bearing our part in the crusade against the 'Arrietts of 'igh-life, we hope to publish before long an article on "Milliners' Birds," written by a well-known ornithologist. This will afford some guidance to those who have sympathy but lack knowledge.

A New Hybrid Grouse

The irregular alliances contracted by the *Tetraonidae* have long afforded interest to students of the European game-birds. Even in Great Britain we meet with hybrids between the Black and Red Grouse, the Black Grouse and Capercailzie, and the Pheasant and Black Game, while in recent years the interbreeding of the Capercailzie with the common Pheasant of our Scottish coverts has been proved beyond all shadow of doubt. But it is in Northern Europe that the birds of this family are commonly recognised as prone to lapse from strict virtue and produce mule offspring, usually the hybrids between the Capercailzie and Blackcock, called 'Rackel-Fogel' in Sweden. The latter species occasionally mates successfully with the Willow Grouse; indeed, the interbreeding of these two species was pointed out by a Norwegian naturalist as long ago as 1795, and, at a later date, received a careful investigation at the hands of Prof. R. Collett. This fact increases the importance of the brief report which that distinguished naturalist has now drawn up upon a hybrid between the Blackcock (*Tetrao tetrix*) and Ptarmigan (*Lagopus mutus*), and published in *Bergens Museums Aarbog* for 1897. The specimen in question was secured in 1896 by the Bergen Museum, which previously contained other hybrids of great interest. We refer especially to a hybrid between the Willow Grouse and Capercailzie, acquired in 1889, and described by Mr J. A. Greig, whose paper was accompanied by a photograph of this remarkable specimen (cf. *Bergens Museums Aarsberetning* for 1889, No. 5).

Mammals of Norway

Another valuable paper that we have received from Prof. Collett is the third instalment of his "Remarks on Norway's Mammal-fauna," and deals with the years 1882-1897. (*Nyt. Mag. for Naturvidenskaberne*, vol. xxxvi. pp. 264-374.)

This memoir contains a clear and concise account of the forty-seven species of indigenous and three of introduced mammals which occur at the present time in Norway. The subject-matter is well arranged, the paragraphs on habits interesting but not over-lengthened, details of measurements are given, fortunately in millimetres, and the nomenclature (except in one or two trifling cases) is up to date—a recommendation which we had almost ceased to look for in local faunas. Altogether, we have little but praise to offer to this Fauna, and we can only regret that the language in which it is written must make it a closed book to many English naturalists.

The mammals of Norway must always be of interest to a wide circle of readers, since in that country the sportsman may still find the brown bear, the stag and roedeer, elk and reindeer, whereas the naturalist, content with smaller though to him not less interesting game, may trap quite a large number of the European Insectivora and Rodentia. To the Pinnipedia, too, Norway is a favoured home. But perhaps the most interesting of Norwegian mammals are the Norway lemming and the beaver—the former on account of its present restricted range and peculiar migrating habits, the latter because of its great rarity as a European mammal. Of both these animals, however, Prof. Collett has recently given full accounts, to which we have referred in previous volumes. The absence from Norway of such a wide-spread animal as the common brown hare is worth noting again, but this deficiency is compensated for by the presence of a numerous series of interesting Carnivora in the northern lynx, the glutton, the wolf, and the bear. Not by any means the least interesting parts of Prof. Collett's paper are the tables giving the number of these animals killed from the year 1846 onwards; and from these we gather that, whereas the number of wolves killed for the five years 1846-50 was 1120, the corresponding number for 1891 98 was only 273. Yet the figures given for the glutton would almost lead us to imagine that this animal had increased of late years, since the number killed during 1846-1870 is considerably less than that given for the years 1871-1895. Certainly the wild Carnivora, at least in Norway, are not becoming extinct at so rapid a rate as many people suppose to be the case.

Notes on Norwegian Geology

Prof. C. F. Kolderup makes three geological contributions to *Bergens Museums Aarbog* for 1897. In the first ("Ekersunds-Soggendalsfeltets bergarter og deres betingelser for anvendelse i stenindustrien"), he points out the practical applications of the rocks that he has previously described from the neighbourhood of Ekersund, in the extreme south-west of Norway. Among these are red, grey, and violet labradorite-rocks, with only trifling admixtures of hypersthene, biotite, and ilmenite; the red variety seems specially available for commercial purposes, lying as it does upon the actual sea-board. The norites do not stand the weather so well; they are grey in their fresh state, and include the well-known rock of Hittero. Now that Norwegian granitoid rocks are partially displacing the well-tried Scotch ones in the London market, and are already popular for tombstones, a paper of this kind is very timely, and may even help to assure men of business of the 'practical' bearings of geological mapping and research.

The second paper is still more to the point as a sermon to the 'practical' man; for it correlates the deficiencies in the bones of cattle in certain parts of the Ekersund area with the small amount of phosphoric acid revealed by the bulk-analysis of the rocks. As much as 2 per cent. is present in the region occupied by the norites and monzonites, and here the cattle have well-developed bones; the ·3 per cent. of P_2O_5 where the hornblende-granite series prevails is sufficient to check disease in some measure; but the bad district, from the farming point of view, coincides with that of the labradorite-rocks, which contain only ·002 per cent. It is almost quaint to find the appearance of apatite in microscopic sections employed as an argument for the choice of pasture-lands.

In a third paper ('Et orienterende niveau i bergensskifrene"), Prof. Kolderup describes a new fossil-locality at Aasen in Samnanger, which leads him to correlate the limestones of that locality, and their associated rocks, with the well-known altered Silurian rocks of Bergen.

Each paper is conveniently accompanied by an abstract in German.

RECENT WORK ON THE FORAMINIFERA

BEFORE enumerating the more recent papers on this group of animals it will be well to call attention to three papers which are likely to be overlooked, from the fact that they are all Inaugural Dissertations. The first, by Friedrich Sellheim, is entitled "Beitrag zur Foraminiferen-kenntnis der fränkischen Juraformation," and was published at Erlangen in 1893. This tract of 34 pages is accompanied by a plate in which fig. 17, *Frondicularia parallela*, is a new form. Many others are described as new species, but they do not seem to us to be worthy of such a position. The second paper "Geognostische Beschreibung des Rathsberger Hohenzuges" (Erlangen, 1896) is by Alfred Bettinghaus, and the Foraminifera occur as a list from the Amaltheenmergel (Lias δ). Karl Mittermaier, the author of the third paper, which is also published at Erlangen, 1896, writes on "Der Mikrofauna der oberen Kreideschichten von Transkaukasien." This paper calls for no special notice, except that his *Nodosaria subconstricta* and his *Glandulina panicea* are the same form, which seems to differ from anything we have seen before from the Chalk.

It is refreshing to turn to the concluding paper on the "Foraminifera of the Gault," by Frederick Chapman, which brings to an end a valuable and perfect monograph on the subject. We hardly know whether to congratulate the author or the Royal Microscopical Society the most, for the Society has most liberally seen the author through with a piece of work which will be a classic. The present paper deals with the Rotalines, and the odds and ends which have come to light during the progress of the work. There is a brief note on the Gault of Folkestone and its continental equivalents, followed by a bibliographic note, a summary of results on the zoological and zonal distribution, and a complete distribution table showing the occurrences of the Foraminifera throughout the whole of the Folkestone Gault at intervals of 5 feet. Mr Chapman has also found time to issue a note on the forms found in the Hartwell clay (*Proc. Geol. Assoc.*, July 1897); to show that the proper specific name of *Saccammina carteri* is really "*fusulinaformis*" of M'Coy, 1849 (*Annals Mag. Nat. Hist.*, March 1898); to join with Prof. Rupert Jones in a masterly resumé of the whole of that singular genus *Polymorphina* (*Journ. Linnean Soc. Zool.* 1896); and to write several other papers of considerable interest. He also undertook the section Protozoa in the Zoological Record, to which his contribution in conjunction with Dr Frazer Hume appeared in the volume for 1895.

Dr R. M. Bagg has given us a detailed description of the Foraminifera in the Tertiary and Pleistocene beds of the Middle Atlantic Slope (*Bull. Amer. Paleont*, vol. ii., Ithaca, N.Y., March 1898). American forms being so little known, this paper is the more valuable, and we hope Mr Bagg will dip further into the subject. The chief things to notice are a new *Spiroplecta*, *S. clarki*, and *Spirillina orbicularis*. Fifty-seven forms are enumerated.

Dr Carlo Fornasini continues his descriptive work, and among his papers are two in the *Rendiconti R. Accad. Sci. Ist. Bologna*, 1897, which discuss the work of J. B. Beccari and O. G. Costa respectively.

Mr J. J. Lister deals in the *Proceedings of the Royal Society*, 1897, with "a possible explanation of the quinqueloculine arrangement of the chambers in the young of the microspheric forms of *Triloculina* and *Biloculina*." As is well known from the researches of Mr Schlumberger, the young of the megalospheric form of *Biloculina* commences with a large chamber, the later chambers being disposed on either side of a single axis; while in the microspheric forms of the same genus, the young begins with a small chamber, and the later chambers are disposed on a rotating axis, that is to say, the plane dividing any single chamber symmetrically is not identical with the corresponding plane of the preceding chamber, but directed at a definite angle to it. It appears possible that, in the first case, the reproduction is asexual and in the latter sexual, but Mr Lister confines himself to suggestion for the present (*vide ante*, p. 58).

Finally, Dr Ludwig Rhumbler, of Göttingen, has a series of notes on the double-shelled Foraminifera, on reproduction and on structural peculiarities of Protozoa generally, which he published in the *Biologisches Centralblatt* early this year.

Flowers and Insects

Sir John Lubbock in a recent contribution to the Linnean Society's *Journal* (*Botany*, vol. xxxiii. pp. 270-278) adversely criticises some conclusions of Prof. Plateau on the relations between insects and flowers. Under the title "Comment les fleurs attirent les insectes" (see *Bull. Acad. Bruxelles*, 1895, 1896, 1897), Prof. Plateau described numerous experiments which in his opinion prove that insects are not attracted by the form or colour of flowers, but by their scent. Sir John argues that these experiments were incomplete, and would, moreover, often admit of conclusions exactly opposite to those drawn. For instance, single dahlias were used in several cases. Squares of coloured paper with a central hole were pinned over the head of flowers so as to hide the ray-florets, which are generally considered to form the attractive portion, while revealing the yellow honey-containing centre. Prof. Plateau did not find that the bees neglected the partly protected heads. But, as Sir John points out, the paper discs, of red, violet, white, or black, must have been very conspicuous, and the insects were, moreover, used to visiting the bed, and could, therefore, soon find the not inconspicuous yellow hearts of the partly covered flowers. When the whole heart was covered with the paper or with green leaves, and even when all the flowers were masked, the insects still came, which, Prof. Plateau says, proves conclusively that they were attracted by the smell. But against this it is suggested that, first, the flowers were only covered above, not completely hidden; secondly, that a bee which has got used to visiting a spot for honey will hunt about for the honey when it is hidden; and thirdly, that as dahlias have, so far as we can perceive, no scent, "it is somewhat illogical to assume that the bees are guided by the sense of smell, when we have no evidence that any scent is emitted." Even if it were proved in his experiments that the insects were attracted by smell, which undoubtedly they are in many cases, and that they did not see the ray-florets, this would not prove that they are not guided by the colour petals when they can see them. In reply, Sir John adduces some experiments of his own to test the respective attractions of scent and colour. A hive-bee was trained to come to a certain spot on a table for honey. The honey was then removed and on one side of the spot was placed a drop of honey on a glass slide, and the great brilliantly blue bracts plucked from a flower-head of *Eryngium amethystinum*; at an equal distance (one foot) on the other side was placed a similar drop of honey, and a far less conspicuous flower-head, which had been deprived of its bracts. During three successive days the visits of the bee were noted, and it was found that of ninety-three visits, it came sixty times to the honey near the bracts, and thirty-three to that near the flower-head. After each visit the head and bracts were transposed to eliminate any possible difference in the two samples of honey. These experiments go far to disprove Prof. Plateau's conclusion that the colours of flowers are not a source of attraction for insects.

Polyembryony in Seed-Plants

By a communication to the *Botanical Gazette* (April) Mr W. F. Ganong adds another to the already somewhat numerous instances of the production of more than one embryo in the embryo-sac. The case is that of the prickly pear *(Opuntia vulgaris)*. The plants had been growing and flowering luxuriantly in the Botanic Garden of Smith College, Northampton, Mass., for at least four years, and set seed each year in great abundance. About half of the seeds when sown produced more than one seedling, and there was the greatest variation in the number, size, and degree of union with one another of the seedlings. Investigation showed that the embryos originate as described by Strasburger for *Funkia*, and as has since been shown to be the usual method where polyembryony occurs, from the cells of the nucellus, the original tissue of the ovule. The egg-cell shrivelled and disappeared. The question arises as to the significance of polyembryony. The writer argues that it is too distinct and elaborate a process to be explained as mere budding, as Strasburger and Pfeffer suggest, and that the variety in the place of origin of the embryos from egg-cell, synergids, antipodal cells, or nucellus, preclude the idea of its being a relic of some older condition, or a case of apogamy. On the other hand, it may be the beginning of something new. "Its origin in several distinct groups and by several distinct methods seems to imply that there is some virtue in the development of the extra embryos, and that their appearance is controlled by that influence, whatever it may be, which is much more powerful than mere morphological inertia, and which elsewhere forms new structures from the most different morphological origins." Its independent appearance in distinct groups may be compared with the appearance of heterospory.

Vaccination in Japan

While vaccination is under the consideration of our legislators, it may be interesting to note that it is compulsory in Japan, and that re-vaccination must be undergone every five years. We learn from the *Revue Scientifique* that the process was introduced into that country in 1849 by a Dutch physician, Nagayo (which seems to us less of a Dutch than a Japanese name), but was first made official in 1871 by the establishment of a vaccination office in connection with the Medical College of the University of Tokyo. The lymph used at that time was derived from the human vaccine imported by Mohnike. But in 1879 a commission was sent to Europe to study the subject, and on its return an institution for the preparation of lymph twice a year was established. The progress which has since been made in the preparation of lymph in Japan is due chiefly to Dr Kitasato. This is a wonderful advance on the old days, not so old either, when the almost universal remedy for all ills was the mode of cauterization known as the 'moxa.'

The Moco

In a Note that appeared in our May number, and was headed "Geology at Oxford," we used the phrase "when the mocos have gathered round Mr Sollas' head."

It is strange that several of our readers should have been puzzled by this, and should have exposed their ignorance by writing to ask the meaning of 'moco.' One correspondent, who thought that we meant to say 'mokes,' irately protested against the application of such a term to the students of Professor Sollas.

The moco is an animal perfectly well known to all serious zoologists. It is sometimes called the rock-cavy, the Indian name is Hoké, while Moko, the original form of the word, is Portuguese. The correct scientific appellation is *Kerodon rupestris*. The species, which is larger than most other cavies, inhabits the interior of Brazil, and, according to Prince Maximilian who first described it in 1820, "it is confined to rocky districts, where it seeks its retreat in holes amongst the fragments of the rocks." As an adaptation to this habitat, it has thick soles, with short blunt nails. The appropriateness of our allusion in connection with a geologist will not be contested. Moreover, the moco is found near rivers, but always in the higher parts of their course; and its flesh is of a pleasant flavour. It is remarkable among cavies for its dense and soft fur. It has a large nail on the inner toe of the hind foot, and this, G. R. Waterhouse imagines, is used by the animal to clean its fur.

This explanation seemed necessary, for otherwise some readers might have suggested that 'mocos' was a misprint for 'snows.'

The Australian Snipe

This bird has been known to science for nearly a century under the name *Gallinago australis*. From the middle of August to the middle of the following March the bird is to be seen in Victoria, but during the intervening months it migrates to the northern hemisphere, and in consequence its nest and eggs remained unknown until last year. They have been found at last on the slopes of Fuji-yama, the sacred mountain of Japan, about 2500 feet above sea-level. The finder is Mr Alan Owston of Yokohama, but he appears to have been put up to it by Mr A. J. Campbell of Melbourne, as we learn from a report in the *Victorian Naturalist* for March 1898.

I

A Geographical Commemoration

THREE notable explorers have just been commemorated by the nations to which they belonged: the Portuguese Vasco da Gama, the Italian Vespucci, and the Russian Deschnev. They are the real or supposed discoverers of the routes to "east and western Ind" and of the north-east passage. Here are a few notes on them, giving the results of recent investigation.

VESPUCCI

THE third Congress of the Italian Geographical Association was held at Florence during the week beginning April 12th. The proceedings of the Congress included the celebration of the 400th anniversary of the discoverers Toscanelli and Vespucci, and were accompanied by much festivity. Vespucci's title to our recognition is that he has been regarded by some as the discoverer of the New World, and indeed the name America is supposed to be a slight modification of his own forename. As to that forename, however, there has been some dispute, for there have not been wanting people to take opposite views and to say that Vespucci's forename was really Alberrico, and that he changed it himself, or had it changed by his friends, to Amerrigo or Amerrico in order to make it resemble more closely the name Amerrique, which is said to have been the aboriginal name of a tribe of Indians living in Nicaragua and there found by Christopher Columbus. The whole question was very exhaustively discussed by the late Jules Marcou, who was a strong opponent of the claims of Vespucci, and had at the time of his death just completed a fresh paper on the subject, which, it is to be hoped, will see the light. Fortunately and appropriately at the present juncture the explorer's register of baptism has just been discovered in the Church of San Giovanni at Florence. It reads: "*Lunedi a di* 18 *Mars* 1452, *Amerigho et Matteo, di Messere Nastagio, di Messere Amerigho Vespucci, popolo Se Lucia Ognissanti.*" Besides the date, there follow from this two facts of some importance: first, that the future explorer had a twin brother; secondly, that he was christened Amerigo, a form of name not so much unlike America.

Vespucci's father was a notary in Florence, and the young Amerigo became a clerk in the great merchant house of the Medici, by whom he was sent to Spain about 1490, and was employed in the fitting out of both the second and the third expeditions of Columbus. Vespucci's claim to be the discoverer of America rests chiefly upon his own word, since, as we have seen, the evidence of his name alone is not very convincing. He said that he went on an expedition which left Cadiz about May 10th, 1497, and after stopping at the Canary Islands came "at the end of twenty-seven days upon a coast which we thought to be that of a continent." Unfortunately, contemporary history is silent regarding this voyage, and it has been proved that from the middle of May, 1497, for the next twelve months Vespucci was busily engaged at Seville and San Lucar in fitting out the fleet with which Columbus sailed on his third voyage. Vespucci, of course, did go to America with three subsequent expeditions, concerning each of which he wrote a narrative. It is said that the name America was first proposed for the Western Continent by Martin Waldseemüller in his "Introduction to Cosmography," published in 1507. In a MS. map of Henricus Glareanus, dating from 1510, the legend Terra America is placed against South America.

It is another remarkable coincidence that on February 3rd of the present year a portrait of Amerigo Vespucci was discovered in Florence, forming part of an altar-piece by Domenico Ghirlandajo in the Ognissanti Church. A reproduction of this portrait, with an admirable account, was given in the *Scientific American* for March 19th. The existence of this picture was well known to Vasari, Bocchi, and their contemporaries. The Vespucci Chapel, however, was whitewashed in 1616, and the painting, which was a fresco, was thus hidden. Search has been made for it before, but in vain, for two reasons: first, that there were two Vespucci Chapels in the church, and the one of them in which the fresco has at last been found passed to another family in 1616; secondly, because Vasari described the work as being over an arch, whereas it really is under an arch. It was eventually found to be in the Chapel of St Elizabeth, Queen of Portugal, behind Matteo Rosselli's canvas of the saint. The *lunette* of the altar-piece represents a standing figure of the Virgin, the broad folds of whose mantle, supported by angels, surround the members of the Vespucci family. Six women kneel on her left and six men on her right. Kneeling next the Virgin is the figure of a young man, who presents a three-quarter face view, and this is the head identified as that of Amerigo Vespucci. He must then have been about twenty years of age.

DESCHNEV

IN 1648 the Cossack Deschnev sailed from Kolyma past the north-east point of Asia down to the mouth of the river Anadyr on the

Asiatic shore of the Pacific, thus being, it is supposed, the first European to sail down the straits afterwards named after Bering, and to prove not merely the possibility of a north-east passage, but the more important fact of the separation of Eurasia from America. For many years the exploit of Deschnev was unknown to the Russian authorities. Thus in an atlas of Siberia by Remesov, completed in 1701, a cape east of the new Siberian Islands is found to have written over it the words "Okamvassbaere Cape," an inscription that was probably added by one of the Dutch councillors of Peter the Great, on the arrival of the map at Moscow. Now we learn that not only is a statue to be erected to Deschnev at Chabarowsk on the Amur, but that the name of the East Cape is to be changed into Cape Deschnev. The latter proposal will probably meet with less cordial acceptance than the former.

Vasco da Gama [1]

It was Bartolomeu Dias who first doubled the Cape of Good Hope, but it was Vasco da Gama who first made use of the knowledge thus gained to sail from Portugal round Africa to India. This was just 400 years ago. Dom Emanuel was then King of Portugal; he was twenty-eight years old, burning with ambition and with the desire of extending his power by geographical discoveries, as had his predecessors. He conceived the great idea of conquering India, and, in the face of opposite advice from his Council, he set about it.

Four ships, the largest of 120 tons, the smallest of less than 100, were fitted out under the superintendence of Bartolomeu Dias, and set sail from Lisbon in the beginning of July 1497. The commander-in-chief was Vasco da Gama, whose flagship was the 'San Gabriel.' His elder brother, Paolo da Gama, commanded the 'San Rafael'; on the 'Berrio' was Nicolas Coelho, while the fourth ship carried provisions.

Vasco da Gama was not an ideal explorer, and at St Helena Bay, between 32° 30′ and 30° S., where the first landing was made, he came to blows with the natives, an occurrence of which his hot and violent temper caused frequent repetitions. The Cape of Good Hope was passed without difficulty, but on the 13th of December the expedition met with a severe gale, during which mutiny broke out among the crew, who wished to force the commanders of the ships to return. Vasco da Gama, however, suppressed this with great firmness, declaring that nothing would induce him to return

[1] For much of our information we are indebted to the interesting account of Portuguese discovery contained in Baron Nordenskiold's magnificent work, "Periplus," Stockholm, 1897.

homewards before he had procured information concerning the route to India, to obtain which he had been sent out by the king.

On Christmas day a coast was seen, and for that reason the land was called Natal, the name which it still bears.

On January 6th, the expedition landed at the mouth of a river which was called Rio do Cobre. Here Coelho's ship was discarded and burned, while the others were repaired, for they had suffered much from the long voyage and heavy gales. Here, too, the mutineers obtained their liberty on the condition that they resumed their chains when presented to the king on their return home, not with any intention of harming them, but to the greater honour and glory of Vasco da Gama. This country was inhabited by Kaffirs, a race previously unknown to the Portuguese, whom they received so favourably that the country was called Terra da Boa Gente.

On January 22nd another large river was reached, on the banks of which was a richer and more cultured people than had hitherto been met with on the coast of Africa. Among them were some supposed to be a cross between Negroes and Moors; some of them even understood Arabic, and were richly dressed. The country was Mozambique, and Gama named the river Rio dos Boos Signaes. Notwithstanding this augury, a bad epidemic of scurvy broke out among the crew, this being one of the first occurrences of that disease of which we have any record.

Mozambique was reached on March 1st, Mombasa on the 7th of April, Melinda on the 15th. Thence Gama started on the 24th of April straight for India, under the guidance of an Indian pilot, whose name was Malemo Canaca, and whom Gama had procured from the ruler of Melinda through fraud and violence. It was on the 20th of May 1498, that Vasco da Gama anchored in Calicut, then an important city on the west coast of the Indian Peninsula, situated in 11° 15′ N. and 75° 45′ E. He stayed in India till the 5th of October, when he sailed westwards from Anchediva. The crossing to Africa, in consequence of contrary winds and calm, took three months, during which a fresh attack of scurvy carried off thirty men. It was not till the 20th of March that the Cape of Good Hope was rounded again, and not till the end of August or beginning of September 1499, that what remained of the expedition again anchored in the harbour of Lisbon.

It is impossible to forget that the career of Vasco da Gama was tarnished by a series of outrages which from the very first completely undermined the dominion which the Portuguese founded in India. Nevertheless, in its relations to the world at large, the achievement was one scarcely inferior to the slightly prior discovery of the New World. It forms an absolute turning-point in the commercial, economic, and political history of Africa and Asia. It is not to be wondered at that the event has just been enthusiastically celebrated at Lisbon from May 17-20, an exhibition being held there, to which charts and plans were sent from our own India Office. Considering, indeed, that our own country has proved the largest heir of Vasco da Gama's benefits, it was natural that a meeting should be held at the India Office, and that a special meeting, attended by H.R.H. the Prince of Wales, was held in the rooms of the Royal Geographical Society on May 16th. The Hakluyt Society, too, has signalised the occasion by publishing a translation, from the pen of Mr E. G. Ravenstein, of the 'roteiro' or log of the famous voyage which we have just described.

That voyage did not in itself effect any extension of our knowledge of the geography of India other than a more exact determination of the distance between Africa and India, and an improved charting of a short stretch of the west coast of the peninsula. But the enthusiasm with which Vasco da Gama's achievement was hailed in Portugal had for long an immense influence on the development of commerce and navigation. Not merely single vessels, but whole fleets, fully manned, were despatched to India by the route that Gama had opened, in order to make conquests there, and to procure strongholds on the coast, to force the natives into treaties of tribute and commerce, and, if possible, to abolish the Indo-Egyptian and the Indo-European trade. As a result, the wave of Portuguese exploration and geographical discovery passed rapidly through the Indian Ocean to Ceylon, the Sunda Islands, and Malacca; to Socotra and Ormuz, and thence to the interior of the Red Sea and Persian Gulf; and again, from Malacca to the Moluccas, China, and Japan. Among the chief conquerors and explorers we may recall the well-known names of Tristão da Cunha, Affonso D'Albuquerque, João de Castro, and Ferdinand Magellan; and not less immortal is the name of Camoens, who sang the story of his country's deeds while banished to the distant gardens of Macao.

II

Some Recent Progress in Root-Physiology

IN the early years of the present century some of the more fundamental questions in the life of the root were for the first time recognised and studied. Through the long tale of years which have since followed, these questions have never been lost sight of; and now, when we have entered upon the last decade of the century, the same problems are being attacked with unabated vigour. As the facts are slowly gathered in, we view the problems with greater clearness, and ask the questions in new and more philosophical forms. Without unduly extending this article with a full account of all the changes and advances which have taken place during the last few years, some of the more recent and more important additions to root-physiology may be briefly examined.

As is familiar to everyone, a root growing under equable conditions of moisture and the like will extend itself vertically downwards. If it be displaced from its normal position it will slowly curve round until its tip once more regains the vertical. This effect is due, on the one hand, to the force of gravity acting as a stimulus, and, on the other hand, to the inherent property of the living tissues of the root, by virtue of which they respond to such outward influences. The peculiar property of living tissues which enables them to respond to external influences or stimuli, is spoken of as their irritability, and the directive action of gravity upon the growth of plant-organs becomes effectual through that special form of irritability which is called geotropism.

In the action of any stimulus on a living organ of a plant, two events must be clearly distinguished: the perception of the stimulus, and the reaction to the stimulus. Between these two stages of the phenomenon a third may or may not be interpolated, viz., the conduction of the stimulus. In the animal body we have precisely the same series of events. A certain influence, such as a prick, a cut, or a burn agitates the end-organs of the sensory nerves lying in the skin of one's finger. The reaction, viz., the consciousness of pain, takes place in the brain, which is widely removed from the point of perception. Between the two events we have the conduction of the disturbance along the fibres of the nerves which connect the one organ with the other.

Turning once more to plant roots, it will be seen that it is an interesting question to determine whether or no the perception of gravity and the reaction which it evokes take place in precisely the same part of the organ. Is the perception of gravity spread over the entire growing (and at the same time potentially curving) region of the root, or is it limited to any particular spot?

The first light which was cast upon this subject was due to the observations of Ciesielski. He noticed, that when he removed the tips of the roots of certain seedlings, they never curved from the horizontal to the vertical position as did the unmutilated organs. As soon, however, as the tip had become regenerated, the normal geotropic activity was once more plainly visible. The root-tip, Ciesielski therefore concluded, was an important and necessary factor in bringing about the geotropic curvature. Charles Darwin[1] took up these experiments where Ciesielski had left them in 1872, and greatly extended and elaborated them. The important conclusion to which they led him was, that the root-apex is the point at which the geotropic stimulation is first received, whilst the zone of growth which lies just behind the apex is the region which carries out the reaction.

These experiments and views were much criticised, and led to numerous discussions. Some of the leading vegetable physiologists, and among them Sachs, altogether rejected Darwin's interpretation. In his "Lectures on Plant Physiology" Sachs writes, "In such experiments with roots not only is great precaution necessary, but also the experience of years and an extensive knowledge of vegetable physiology, to avoid falling into errors, as did Charles Darwin and his son Francis, who, on the basis of experiments which were unskilfully made and improperly explained, came to the conclusion, as wonderful as it was sensational, that the growing-point of the root, like the brain of an animal, dominates the various movements in the root." It must be admitted that these adverse criticisms were entirely justified, since the experimental proofs which were offered by Ciesielski and Darwin were far from conclusive.

So serious an operation as the 'decapitation' of the root might well be supposed to lead to changes in the irritability of the organ. The recent work of Rothert upon Heliotropism (1894) has taught us that removal of the apex of the cotyledon in certain Gramineae temporarily (1-2 days) paralyses their perceptive powers for light, notwithstanding the fact that the sub-apical parts of the cotyledon are also sensitive to light in the unwounded plant. Facts of this nature show us that there were good grounds for those who raised objections to Darwin's conclusions.

Matters remained in this highly unsatisfactory condition until quite lately, when Dr F. Czapek undertook a comprehensive series of researches on geotropism. The first-fruits of the work were com-

[1] Ch. Darwin "The Power of Movement in Plants" 1880.

municated, both in Germany and England, by Professor Pfeffer (10 & 11), in whose laboratory and under whose guidance the researches had been carried on. All who were present at the Oxford meeting of the British Association in 1894 will remember with what eagerness Professor Pfeffer's spirited address was listened to.

The plan which was adopted in this research was as ingenious as it was simple. The end of the root to be experimented on was directed into a small glass tube of suitable calibre, which had been bent on itself at right angles. The apex of the root was thus obtained at right angles to the part which carries out the curvature. Accordingly, if the apex is placed vertically the growing (and curving) part is horizontal and *vice versa*; we have, thus, a means to hand by which we can determine whether it is the 'tip' of the root or the growing region lying behind the apex which is perceptive of the stimulation due to gravity. Two batches of roots, which had been treated in this way but which were otherwise normal, were taken. The members of the one batch were placed so that their apices were vertical, whilst those of the other group were arranged so that the growing region was vertical. The result was that the first set with vertical root-tips grew downwards without exhibiting any curvature, whilst those plants which had their root-apices horizontal, curved in the growing region, so as to bring the apices into the vertical position which is normal to them, notwithstanding the fact that the growing region was itself displaced from the vertical by this movement. The long disputed question was at length set at rest by these experiments and it can no longer be doubted that Darwin's views were correct. The apex of the root receives the stimulation of gravity and transmits it to the growing region behind, which reacts to the stimulus by a curvature. The enthusiasm with which Professor Pfeffer's communication was received at Oxford was perhaps not altogether uninfluenced by a feeling of pleasure that our great naturalist had been in the right through these long years of controversy.

In the following year Czapek (1) published his "Untersuchungen uber Geotropism" in which the above-mentioned matters were set down at length, together with many other results of importance. The demonstration that the perception by the root-apex of the stimulus due to gravity was influenced by the various conditions of environment otherwise than was the reaction itself, is of great interest. Under a temperature too low for the geotropic curvature (reaction) to take place, the perception of the stimulus by the root-tip was nevertheless manifest. The curvature of the root, which is the end result of the stimulation of gravity, is directly dependent upon the growth of the organ. Stop the growth of the root and you at the same time stop the reaction.

The experiments of Köppen have shown that in the case of *Lupinus albus* the radicle ceases to grow at a temperature below 7·5° C., and Czapek himself found that at temperatures between 0° and 2° C. growth was inhibited in all the plants he used for these experiments. If, therefore, radicles of *Lupinus* and *Faba* were laid horizontally, at temperatures not higher than 2° C., the geotropic curvature was not carried out even after twenty-four hours induction. At the end of this time the still straight roots were placed on a clinostat (an instrument on which gravity is made to act equally in all directions, and on which, therefore, that force is practically eliminated as a stimulating agent) at a temperature of about 19° C., and in from four to five hours geotropic curvatures were clearly visible. In these radicles the perception of the stimulus (gravity) had taken place at the low temperature, although the reaction itself could not be accomplished under the circumstances; for as soon as the temperature was raised, and the conditions rendered suitable for growth, the rootlets carried out a curvature which could only be in response to gravity acting during their stay in the cold, since in the second and warmer stage of the experiment the stimulating action of the force had been eliminated. Although the perception of the stimulus is not entirely inhibited by a low temperature it is none the less affected by it. The lengthened time for which the force of gravity must act to evoke its result, shows that the perceptive faculty of the living cells of the root-apex is lowered by the lowness of temperature.

Another condition of environment which was carefully studied was that of an atmosphere containing no oxygen. The work of Kraus, Wortmann, and Correns was the first to give us any information regarding the effect of absence of oxygen upon geotropic phenomena. These experiments, however, left much still undecided and many points calling for a more extended examination. The time during which these botanists subjected the plant-organs to geotropic induction was not, in any case, sufficiently long to decide with certainty that the stimulation was not felt in the absence of oxygen. A longer period spent in an oxygen-free atmosphere, however, directly affects the life of the root, so that there were great difficulties in the way of obtaining conclusive evidence.

The ingenious application of Chudjakow's results by Czapek enabled the latter to overcome the difficulties which had invalidated the work of previous experimenters. Chudjakow, in carrying on his researches on the intra-molecular respiration of vegetable organs, had found as a side-result that the higher the temperature the more rapidly did the organ die in an atmosphere containing no oxygen. It occurred to Czapek to see, therefore, whether a root placed in a vacuum could not resist its usually harmful effect when the temperature was kept very low. Roots left for twenty-four hours in a vacuum at a temperature of 0° to 1° C. were quite healthy at the end of that time. On being placed once more in air at the ordinary temperature of the room, they resumed growth, and showed all the properties which are normal to these organs. In the actual experiments on geotropism, roots of *Lupinus* were placed horizontally for twenty-four hours in an atmosphere free from oxygen, and at a temperature of 0° to 2° C. During the whole of this time growth was entirely arrested in the organs, and no reaction could therefore be manifested by them. The roots were then placed on the clinostat, at the temperature of the room and in ordinary air. In a few hours geotropic curvatures were clearly shown.

From these results of Czapek and of the older workers it will be seen that, whilst the absence of oxygen inhibits the growth of the roots and prevents the display of a reaction, it does not arrest the perception of the stimulus by the root-apex. The previous work of Wortmann, Correns, and others had already shown that want of oxygen stopped the growth, even at ordinary temperatures, whilst the experiments of Czapek have now indicated that the perceptive faculty of the root is not prevented by the absence of this gas. As in the case of low temperatures, so also here the perception of and reaction to the stimulus are governed by different laws.

The effect of mechanical prevention of growth in geotropic organs was also examined by Czapek, but, beyond mentioning that in these cases the perception of the stimulus was not arrested at the same time, we cannot offer any comments in the present paper.

When a root which normally grows straight downwards is displaced from that position, its growing part bends round until the apex is once more in the vertical. A question which naturally rises to the mind in connection with this is, in what position is the reaction at its maximum? Sachs, partly on experimental and partly on theoretical grounds, believed that the horizontal position was the one in which geotropic reactions were most marked. As the root was displaced more and more from the vertical, the reaction became more and more vigorous, until when the horizontal direction was attained the highest point was reached; as the root was displaced above the horizontal the reaction gradually became less, until it completely died away when 180° had been passed through.

Czapek investigated this question both by studying the magnitude of the after-effect following inductions of equal length when the roots were placed at different angles from the vertical, and also by measuring the time-relations of the reaction. He found that the horizontal was not the position of maximum reaction, but that this was some 45° above the horizontal. From this point onwards, until the roots were once more vertical but in the reverse direction to the normal, the vigour of the reaction became less and less, but still remained higher than at 90°. When the tip of the root is exactly upside down, that is to say when it points accurately vertically upwards, it is theoretically once more in a position of equilibrium, and the geotropic reaction should equal 0. As a matter of fact the root curves round until the tip is once again directed vertically downwards. Sachs had already explained this as an effect of nutation, which carried the root out of the exactly upright position and so brought it under the influence of gravity again, and the experiments of Czapek tend to bear out this view.

The latter part of Czapek's work is occupied with an interesting account of 'autotropism,' and the elimination of geotropic curvatures by virtue of autotropism. By autotropism is implied the inherent tendency of vegetable organs to grow in a straight line unless they be compelled by outward influences (gravity, light, etc.) to bend aside from that direction. Moreover, having bent aside to the action of a transient external stimulus they subsequently straighten themselves out again through the agency of the same inherent quality. The reader, however, must refer to the original for these matters.

Up to this point we have only referred to phenomena presented by the main root. Both in the paper already mentioned and in other special ones Czapek (2 & 3) has studied the geotropism of side-roots. These apparently are governed by somewhat different laws; they do not grow vertically downwards like the tap-root, but occupy a position in which they form a greater or less angle with the vertical. Dealing with side-roots of the first order, Sachs was inclined to attribute their oblique position to a weaker geotropism residing in them. The angle they form with the vertical he called 'the angle of geotropic limitation.' In turning his attention to this subject Dr Czapek first satisfied himself that the only external force inducing the oblique position was gravity. Having settled this point by clinostat experiments, he next enquired whether the oblique position of the rootlets could be explained in the manner indicated by Sachs, viz., by a limited action of gravity, or whether a different explanation was needed. A number of plantlets of *Vicia Faba* were taken and divided into two groups. In the first group the side-roots were turned 60° above their normal position, whilst in the other set the rootlets were placed 60° below their usual direction. The experiment, which was carried on in darkness, showed that the rootlets of the first group returned to their normal position more rapidly than those of the second batch. The intensity with which side-roots, placed above their usual position, bend is therefore greater than that manifested by rootlets displaced below the normal direction. Czapek believes that these facts point to a two-fold geotropic action: to a positive geotropism common to the side and the main roots alike, and to a transverse geotropism which tends to give the organs a horizontal direction. In the case of the roots which are turned above their 'angle of geotropic limitation' the horizontal and positive geotropisms act together, whilst in the opposite case in which the displacement is below the normal angle the transverse geotropism acts in direct opposition to the positive geotropism. In this way Czapek seeks to explain the oblique position of the rootlets and the other phenomena which were noticed in his experiments. What Sachs called the 'angle of geotropic limitation' is on this view the resultant of the action of two forces—positive geotropism and transverse geotropism.

In the same memoir Czapek includes his important work on the directive forces acting on rhizomes and underground stems.

Quite lately further interesting observations on the behaviour of side-roots have been published by Alfred Schober (13). He ranges his experiments in two series, according as the main root is compelled by growth to bend on itself at an angle and thus bring the normally oblique side-roots into a vertical position, or as the main root is merely placed in a slanting position sufficient to bring the rootlets into the vertical. The experiments were carried on in one of Sachs' cases with a glass side, and the seedlings were so arranged that their side-roots lay against the glass. They could be watched in every phase of their growth and all changes faithfully recorded on tracing paper. The plants used for these observations were *Pisum sativum*, *Phaseolus multiflorus*, *Cucurbita Pepo*, and *Vicia Faba*. It was found that when the rootlets were placed in a vertical position they lingered for a shorter or longer time in this direction, and then usually bent aside so as to bring their apices into an oblique position. In nearly every instance the curvature took place in a plane passing through the side and the main root. In a few cases, as for instance in one recorded for *Cucurbita Pepo*, the rootlet when placed in the vertical line continued to grow in this direction throughout the twelve days that the observation was kept up. This behaviour is, however, rare, and only serves as the exception which proves the rule. It was noticeable that, although the curvature of the side-roots from the vertical nearly always took place, they seldom bent sufficiently to attain their normal 'angle of geotropic limitation.' The lingering, too, in the vertical was also sometimes very noticeable; Schober mentions the cases of *Cucurbita* and *Vicia Faba*, in which the rootlets continued to grow for 2 cm. in a straight line before they manifested the characteristic curvatures.

These experiments bring out another interesting point, viz., that the direction of the curvature is partly fixed and partly irregular. It may be said, in general terms, that the rootlets springing from the hypocotyl (the part of the stem below the seed-leaves) and from the adjoining base of the root curve upwards towards the shoot part of the plant—as Czapek expresses it; whilst the side-roots arising nearer the root-apex are indefinite, and may bend towards the shoot or towards the main root. In one and the same individual rootlets may be seen bending in both directions.

Another manifestation of irritability by roots has lately received renewed attention from Professor Spalding (14). In his work on the "Power of Movement in Plants," Charles Darwin first examined the effect of attaching small pieces of cardboard and the like to the tip of the root by means of shellac or gum-water, or of touching this region of the organ with dry silver nitrate (caustic), or of slicing off a small fragment from one side of the apex. The usual result was that the root executed a curvature, in the growing region, of such a nature that the convexity was on the side of the object or cut. Darwin concluded that the phenomenon was one of irritability and that the apex was sensitive to the stimulation of touch. These experiments of Darwin led to a long discussion, which, although fruitful in many respects, yet left the main issues undecided. It was clearly shown by Wiesner that the curvature was due, not to simple contact, but to injury. Detlefsen believed, on the grounds of his researches, that these curvatures—often called 'Darwin's curvatures'—had no connection with the irritability of the organ but were purely mechanical results of the injury. Wiesner, likewise, favours a mechanical interpretation, but many of his experiments and statements may equally well be taken as upholding the opposite view, which invokes the aid of protoplasmic irritability. Pfeffer, on the other hand, regards mechanical explanations as insufficient, and he suggests the name of 'Traumatropism' for the phenomena resulting from such one-sided injuries. By this term he brings the processes in question in line with the manifestations of geotropism and heliotropism.

In 1894 Spalding carried out a careful research on these traumatropic curvatures, and justified the name by bringing forward good evidence for the belief that the phenomena were due to irritability. Most of his experiments were made by branding the root-apex with a small, heated, glass rod. A root so treated executes a curvature in the growing region, with the convexity towards the injured spot, and also exhibits a second bend directly over the damaged region but in the opposite direction to the first curve. Darwin had, likewise, observed this second bend and rightly—as Spalding points out—attributed it to mechanical actions. Root-tips touched on one side with silver nitrate or metallic copper behaved in a similar manner.

Detlefsen connected his mechanical interpretation of Darwin's curvatures with the altered tension of the root-cap. In fact the root-cap was, to his mind, alone responsible for the phenomenon.

Spalding shows that injury to the aerial roots of *Anthurium*, etc., although they are destitute of root-cap, still induces strong manifestation of traumatropic curvatures. Moreover, roots in which the injury is to the root-cap alone, without at the same time affecting the apical meristem or actual growing-point, do not carry out traumatropic bendings. The whole evidence, in fact, points to the growing-point as the only region sensitive to cuts, burns, or other injuries, and suggests that these agencies act as true stimuli which are conveyed to the elongating region lying behind.

These results show a close correspondence with those already mentioned in the case of geotropism, and in yet another respect do we find a similarity between the two phenomena. The latent period, viz., the time between the perception of the stimulus and the resulting reaction, can be greatly extended both in geotropism (as we have seen) and in traumatropism. A seedling of *Lupinus albus*, for example, had its root-tip branded with a heated glass rod. The rootlet was immediately afterwards confined in a plaster cast, so that its growth, and therefore also its reaction, were prevented. After eight days the cast was removed, and the root allowed to resume its growth. It at once executed a traumatropic curvature, which could only have been in response to the injury received from the hot glass more than a week before. This shows that in the case of stimulation by uni-lateral injuries the latent period can be even more protracted than in geotropic actions.

Quite a different aspect of root-physiology has been touched upon by Czapek (4) in his paper "Zur Lehre von den Wurzelausscheidungen." In this paper the substances which are excreted by the root into the surrounding medium are subjected to a careful and searching examination. In the present article it is impossible to enter at any length into the numerous important facts and conclusions which Czapek has set down in the memoir quoted, we can only pick out one or two significant points, and shortly deal with these.

The older teachings of Liebig and others, that, in the excretions of the root, substances are present which alone are capable of laying open to the plant the nutritive store of the soil, has, with the course of time, been modified gradually, and taken with ever increasing reserve. The fact that roots excreted acids, capable of permanently reddening litmus-paper and of corroding a smooth surface of marble, had, however, been long believed in by botanists. The researches of Czapek have now thrown considerable doubt upon this point. In no instance could he satisfy himself that any of the stronger acids passed out from the root in the free state. Acid salts and carbonic acid could alone be detected, and all the phenomena upon which the idea of free acids in the excreta depend, can certainly (as Czapek points out) be explained by the presence of these two substances.

The most important of the acid salts is the mono-potassium phosphate, and this is quite capable of permanently reddening litmus-paper. The other pillar which upholds the view of an excretion of free acids by the root is the fact, first indicated by Liebig and Sachs, that when roots are grown on a polished slab of marble they eat this away in their progress, and mark it out in curious corrosion-figures. By using artificial slabs of known composition (*e.g.*, calcium carbonate, calcium phosphate, aluminium phosphate), Czapek has been able to bring forward very strong evidence that carbonic acid is the only acid responsible for the corrosion-figures. Substances not dissolved by carbonic acid are likewise unaffected by the excreta of the root.

Whilst accepting these results so far as they go, Pfeffer (12) points out that the question can only be regarded as determined for the particular plants investigated, and for the particular conditions under which the observations were carried out. The fact that we are acquainted with certain fungi which excrete organic acids, and that this formation of free acid is largely dependent on outward circumstances, speaks in favour of this view. "We should not be surprised," Pfeffer remarks, "if certain flowering plants were yet discovered which made use of the undissolved ash-constituents by means of an energetic secretion of acids."

Of the other substances present in the excretions of the root, the most interesting, perhaps, is formic acid, which had previously been detected by Goebel (5). This acid, Czapek believes, does not occur in the free state, but in combination with potassium. The acetic acid which Becquerel believed he had found in root-excreta could in no case be seen, either free or combined, by Czapek, neither could Boussingault's lactic acid. Oxalic acid, although not generally found, could be clearly demonstrated in one instance, viz., *Hyacinthus orientalis*. Enzymes were not found with any regularity in the excretions of the root, and in this respect Czapek's results differ from those of Molisch (7).

The excretion of the root is generally supposed to pass solely through the root-hairs. Czapek points out that this conclusion is based upon insufficient grounds, and that we even have evidence against it. Roots of hyacinth grown in water are without hairs, and yet excrete oxalates; the formic acid is given out by the youngest parts of the root below the region of the root-hairs.

Another interesting contribution to root-physiology is that of George Peirce (8) on the penetration of roots into living tissues. Pfeffer (9) in a previous research had shown that the roots of the different plants he examined exerted a considerable pressure in their growth. Thus, in one case (*Vicia Faba*), a longitudinal pressure of 7-10 atmospheres was found to be attained by the growing root. These observations suggested the idea that the roots of various earth-inhabiting plants might be induced to penetrate living vegetable tissues. Peirce, in examining the matter, took potato-tubers and split them in half. In small slits on the one half he inserted seeds of *Brassica napus* or *Sinapis alba*, and then tightly bound the two parts of the tuber together again. He left these manipulated potatoes for twelve days in moist sawdust and then unbound them. He found that nearly all the seeds had germinated, and that whilst a few had grown along the line of the cut in the tuber, others had penetrated the substance of the potato. In some cases growth had been so vigorous, that not only had the parenchyma of the half-tuber been penetrated, but the root had actually made its way through the cork-layer on the outside and grown into the sawdust. Other experiments, rather differently arranged, showed that *Pisum* and *Vicia Faba* could likewise penetrate living tissues. Not only did the roots of these plants grow into tubers of potato but also into the stem of *Impatiens sultani*, leaves of *Echiraria*, *Aloe* and other plants. One of the most interesting and significant results which Peirce obtained was to grow specimens of *Pisum* as parasites upon other plants from the seedling stage until flowering. *Impatiens sultani* formed the most satisfactory host. In *Pisum*-plants grown under these peculiar conditions the root-system was formed in a fairly normal manner, although the rootlets were destitute of hairs; the stem, however, was stunted, but bore leaves and a few flowers. The importance of this experiment to the student of parasitism will at once be evident.

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III

The Study of Variations: A Rejoinder

PROFESSOR HENSLOW has apparently completely misunderstood my paper in this Magazine for April. My object was, leaving the whole question of facts and observations out of consideration, merely to point out some reasons for the well-known difference of opinion on this subject among present-day biologists.

In referring to Professor Henslow's theory I endeavoured to show that another explanation, quite as plausible and equally in accordance with the facts adduced by himself, could be advanced. Thus there would be two theories in the field, both explaining the facts, and neither absolutely negatived or directly supported by these facts. Consequently, to test the merits of the rival theories a fresh series of data would be required. The ultimate decision might be in favour of Professor Henslow and the Lamarckians, and opposed to the Darwinians, or *vice versâ*, or might show that both theories were partially correct. I did not doubt the facts, or even assert that the conclusions drawn were incorrect, but argued that this possibility of double interpretation proved the conclusions to be inadequately supported. Professor Henslow says he does "not quite see, if the conclusions be correct, how the argument can be faulty." This statement must surely be due to an oversight, for it is notorious that right conclusions may accidentally be arrived at through fallacious arguments But I did not suggest that his conclusions were either correct or incorrect; my position is, that in either case the facts on which they are founded would equally support other conclusions.

On page 313, paragraphs 1, 3, 4, 5; and again on page 314, paragraph 1; and page 315, paragraphs 3 and 4, he makes a series of statements in reference to the Neo-Lamarckian position, which position he considers I have misunderstood. (1) ". . . The purport of Neo-Lamarckism is precisely that embodied in the words of Darwin quoted, or the discovery of 'the primary cause of modification.'" (2) "Neo-Lamarckism is only concerned with tracing out the causes which originate or bring about the variations themselves." (3) "'No selection except from general strength.' In this last [kind of variation] I seem to recognise my own position."

I find it difficult to reconcile these three statements. The meaning of the first position appears quite plain, 'the primary cause of modifications' being the sole quest of the Neo-Lamarckians. If this is so, they are working on a part of evolution which Darwin confessedly found difficult to elucidate, but which can yield nothing opposed to his views. Indeed, as Mr Henslow himself has insisted by means of an admirable quotation from Darwin, ". . . Natural Selection . . . has no relation whatever to the primary cause[1] of any modification of structure." It follows that there can be no antagonism between two positions so distinct. In fact, the Neo-Lamarckian is included in the other larger, Darwinian position.

But the second definition involves something more than a study of the primary causes of modification, since causes which bring about, as well as those which originate, variations are here included. In the third position Mr Henslow accepts for himself the views which I formerly ascribed to him, and eliminates natural selection as a species-forming, though admitting it as a possible species-separating, force. Which of the three positions he would have us accept, it is hard to infer, either from his reply or from his works.

He further says: "There is no necessity for all the individuals to be equally, though they be all similarly, modified. Natural Selection need not enter so timidly as he imagines, but may boldly kill off as many as it pleases, and thus 'become a factor of some importance'; but this has nothing whatever to do with the primary cause of the origination of the definite variations. These being now known, it is at once seen that Natural Selection plays no part at all in causing them." Mr Henslow here seems to hold that variations, when occurring in any given variety, may vary in degree, but that this degree will remain constant in each; that, given a certain number of variations from a certain point, each one of these will continue to vary at the same rate whether other variations are present or not; and, consequently, that "Natural Selection thus applied only separates varieties and makes them more distinct for the benefit of the classifier," but plays no part directly or indirectly in forming these divergences; it only eliminates the unsuccessful intermediate varieties when formed.

Now the objection that I raised to this position was, that as there are at least as many eminent authorities who hold opposite views and are not convinced by the facts adduced in support of this contention, it is not enough to assert that it is so; it must further be shown that where Natural Selection is not able to act, varieties have diverged and adapted themselves as rapidly as similar varieties have in a control experiment, in which Natural Selection has been given full opportunity of acting. Again, when I questioned the validity of assuming that definite variations are necessarily opposed to Natural Selection, he, in reply, mentions what I already knew, that

[1] Spaced type mine.

Romanes regarded indiscriminate variations as essential to the theory of Natural Selection. Had Romanes and other biologists thought otherwise, it would have been unnecessary to raise this point, and it was only because I had not seen my position taken up elsewhere that I put it forward.

Professor Henslow may be told by many that his books do prove that variations are definite, but, unfortunately, for the present state of the question there are at least an equal number who think otherwise. But were all biologists unanimous on this point my position would be unaffected, since it maintains that definite variations can be easily accounted for by Natural Selection alone. The fact that Darwin and Wallace were both unaware of this, does not invalidate the theory in the slightest degree, since they could not possibly be expected to have foreseen every development of their views. Mr Henslow finds my theory on this subject to be 'offered without a particle of fact' to support it; but no fact is here needed, the position being that a theory is offered in support of Natural Selection, leading naturally from indefinite to definite variations in the course of evolution. It is not a question of facts, as at present collected; it is simply a question of two competing theories, which would both explain the definite variations in nature, if they exist. It will therefore be only from a careful restudy of variations that we can hope for a more settled view of this question.

The difficulty that Professor Henslow finds in this to me obvious deduction astonishes me; I can see nothing complicated in the assumption that Natural Selection, as it necessarily must eliminate the unfit, will as necessarily leave behind to grow and reproduce with each other the more or less fit; and that with each succeeding generation the variations must tend to become increasingly fit, and, consequently, more or less definitely adaptive. Indeed it seems to me to be a much greater assumption to say that variations are adaptive on the present data, since a biologist's knowledge would be great indeed if, even on any given single variety, he were able to prove all its variations useful, and much more so were he able to establish the same result with reference to species.

Lastly, I am asked to give half-a-dozen examples of plants and animals living in a wild state, which I can place within the first three groups of my classification of variations. As the whole object of my paper is to point out that no instance can be adduced by any biologist which will be accepted by others holding opposite views, it seems a little extraordinary that I should be expected to give six instances, or even three or one, when I believe none are to be found in the present state of our knowledge on this question.

Weismann, in *The Contemporary Review* for September 1895, states this difficulty very clearly: " An essay by Herbert Spencer is always interesting reading, even when one does not agree with him. He can defend his thesis admirably; and the most destructive arguments that are brought against him seem to turn, in his skilled hand, into supports for his views. I am quite convinced that it would be impossible for me to adduce any evidence on my side to which he would not have some reply to make, and so we might prolong this conflict of opinions indefinitely."

The disputes which have occurred in the last ten years are surely sufficient evidence of this unsatisfactory condition of affairs.

To recapitulate. I started with the well-known difficulty that biologists have in coming to anything like a unanimous conclusion on this subject. I then endeavoured to prove that more or less equally plausible, though conflicting, theories could be advanced to explain the facts at present collected, leaving entirely out of the article my own views on heredity, and taking up a neutral position; lastly, I pointed out that this unsatisfactory state of things might be due to a too indefinite use of various terms and too hasty generalisations, and, therefore, that more precise methods were needed. I do not see that any one of these positions has been affected.

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IV

The Migration of the Right Whale
(*Balaena mysticetus*)

Part I.—In the Greenland Waters

WE are so accustomed to regard systematic migration as an attribute peculiarly distinctive of the birds of the air, and only shared with them to a limited extent by a few terrestrial animals, as to be in danger of overlooking the fact that this mysterious instinct (?) is also conspicuously developed in marine mammals. Some members of the Order Pinnipedia especially afford striking examples, but in none does it exist with greater regularity and persistency than in the Cetacea. Dr Nansen, in the "First Crossing of Greenland," remarks that "whales have evidently their migrations, though we know little or nothing about them," a reproach which this imperfect contribution to the subject is an attempt in some degree to remove. Since little has been written specially devoted to this interesting feature in their life-history, my object is to present in concise form the information on the subject, at present so widely diffused that its full significance is not apparent. So far as I know, the most important papers bearing on the migration of the Cetacea are those of Eschricht and Reinhardt[1], and Dr Robert Brown's valuable papers as reprinted with additions in the "Arctic Manual" (1875, Part I., Biology, pp. 1 and 69). Much is to be gleaned from the voyages of the early Arctic discoverers, and something from the many less pretentious expeditions in more recent times; but too often the vague mentions of 'whales' seen leave the species so uncertain as to be valueless for any useful purpose. This is excusable when we consider how very difficult it is to identify them when seen perhaps only for a brief period, and from the deck of a vessel in perpetual motion. It thus happens that the chief and most reliable information is to be derived from those whose business it is to pursue and capture these animals in their summer haunts. Here again caution is necessary, for intelligent as some of our whalemen have been—and I need only mention Scoresby and David Gray in the Atlantic, and Scammon in the Pacific—it must be borne in mind that their main object is the

[1] "Recent Memoirs on the Cetacea," by Profs. Eschricht, Reinhardt, and Lilljeborg. Edited by W. H. Flower, pp. viii., 312, 6 plates. 4to. Ray Society, London, 1866.

capture of these valuable prizes, and not the study of their habits, except in so far as such a knowledge would conduce to that result; and tradition has a very strong hold upon them. It unfortunately happens, too, that these men, who could tell us so much and are perfectly ready to impart information, owing to their very familiarity with the subject, and perhaps to their regarding much really of scientific value as too trivial to be worthy of mention—are extremely difficult of approach, and it requires no little previous knowledge of the subject to elicit at such interviews all the information possible. Nevertheless, I have to acknowledge with gratitude the invariable kindness I have received from the whaling captains, and the value of the information resulting from many conversations and much correspondence with them extending over many years.

In so imperfect a sketch as this must of necessity be of so wide a subject, it would be impossible to deal with Cetacean Migration generally, I propose therefore on the present occasion to confine my remarks to one species, the Arctic Right Whale (*Balaena mysticetus*), an animal which has been more or less under observation ever since the first decade of the seventeenth century, when it was discovered by Henry Hudson frequenting the seas west of Spitzbergen in great numbers, and became the object of attack by men of various nationalities. At first it was hunted from the shore, but as, gradually driven from the bays and fjords, it became scarcer and more wary, its pursuers became more and more enterprising, and followed it farther and farther into the ice-fields. And here I may perhaps be allowed once for all to dispose of the popular idea, so oft repeated and so difficult to eradicate, that the Greenland Right Whale formerly frequented the temperate waters of the Atlantic Ocean, and that it has been driven north by persecution, or has been exterminated in the more southerly localities. The true *B. mysticetus*, greatly reduced in numbers it is true, and only an occasional visitor in some localities where once it was common, still frequents precisely the same waters in which it has ever been found, and from the nature of its habits must continue to do so, so long as it exists as a species. The whale of the genus *Balaena* formerly found in considerable numbers from the Bay of Biscay to the North Cape, according to season (for it also was a regular migrant), and still occasionally met with in the waters of the North Atlantic, is a perfectly distinct species, a fact fully established by the investigations of Eschricht and Reinhardt.

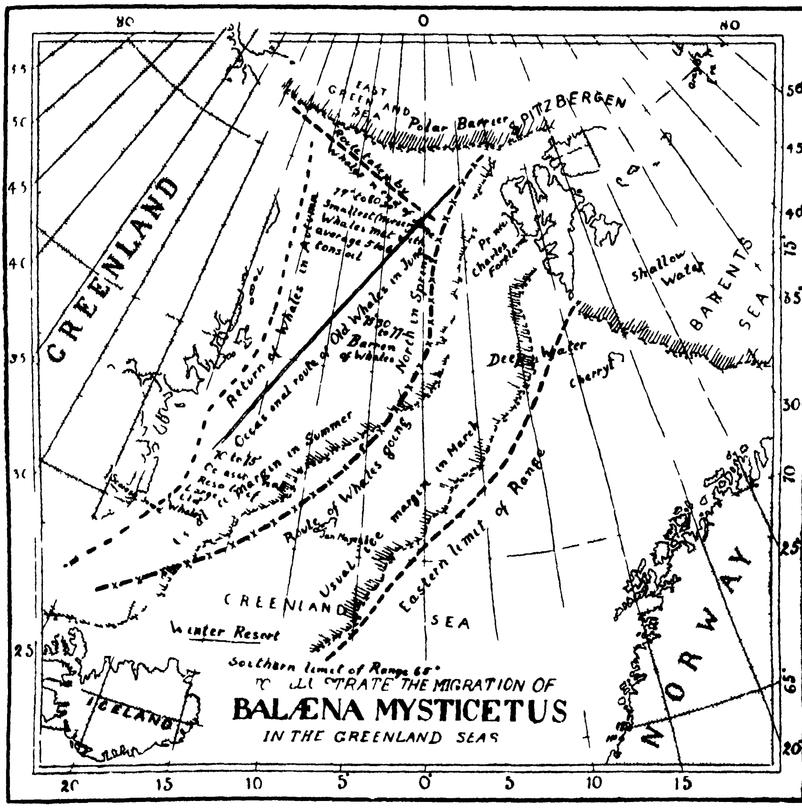
It is not my intention to follow the so-called 'Greenland' Right Whale into the Pacific, where it is also found; but the two resorts with which I propose to deal are the northern extensions of the Atlantic Ocean, lying to the east and west of Greenland, reaching on the east side of that frozen land to the western shores of Spitzbergen, and on the other westward to the waters of the Arctic archipelago forming the much broken extension of British North America, and comprising Davis Strait, Baffin Bay and the many straits and sounds lying still further west, but to what precise extent is not fully known. On the east side of Greenland, *Balaena mysticetus* winters in about 65 N. latitude, and has been found as far north as the barrier presented by the border of the permanent polar pack. Its range on the west side will be discussed later on, since it is intended at present to confine ourselves to the Greenland seas. In all its wanderings it rarely quits the ice-edge, and may be said to inhabit "the region of the loose ice and open water-spaces, bounded on the one side by the sea, and on the other by the edge of the solid ice, nearer by choice to the former than to the latter" (Gray).

I will now endeavour to trace the migration of the Right Whale in the Greenland seas, but first it should be explained that when migrating the behaviour of the whale differs materially from its mode of proceeding at other times Dr R. Gray (*in lit.*) tells me that a whale 'on passage' may be readily recognised, "it goes steadily onward with considerable rapidity, with the upper jaw from the tip of the nose backward as far as the blow-holes above water, the body submerged, and, except for the eddy caused by the action of its tail, apparently motionless. It continues thus for some five or ten minutes, blowing at regular intervals all the while, then, throwing up its caudal fin it leaves the surface and before re-appearing may have performed the distance of a mile. Swerving neither to the right nor to the left, it continues its onward journey and no ordinary ice-field causes it to alter its course." Scoresby says that a "run of whales of a particular tribe passing from one place to another has been traced in a direct line from south to north, along the edge of the western ice through a space of two or three degrees of latitude, where it has entered the ice to the north-west and passed beyond the reach of the fishermen."[1] On such occasions they usually associate in small flocks or 'schools,' but have been observed in considerable numbers. Dr Brown was informed by Dr M'Bain, R.N., that a little north of Pond's Bay he saw a continuous flock of several hundreds pass north, and a few days after they were followed by a herd of walruses, the numbers of which were "beyond all computation," all seeming intent on reaching the opening to Lancaster Sound. Dr Brown pertinently asks, where could such a number of these huge animals come from?

Not much is known with regard to the sojourn of the whales in their winter quarters. As the ice becomes disrupted in the early spring they pass along to the northward, availing themselves of the

[1] "An Account of the Arctic Regions," by W. Scoresby, jun. 2 vols. Edinburgh, 1820. Vol. ii., p. 214.

open waters and floes. It might be expected that at this time they would be met with by the vessels which, in the months of March and April, search the ice-edge for the young seals, usually to be found to the north-east of Jan Mayen, between 70° and 73° N. latitude and 1° to 4° E longitude, in accordance with the lie of the ice, but as the whales keep well within the margin of the ice they are rarely seen *en route* at that time; when seen, however, they are always passing northward. Large numbers of these 'spring



TO ILLUSTRATE THE MIGRATION OF
BALÆNA MYSTICETUS
IN THE GREENLAND SEAS

migrants' were observed by some Dutch sailors who wintered on the Island of Jan Mayen in 1633-4, they first appeared off the island on the 27th of March, and were seen almost daily in larger or smaller numbers till the end of April, the poor fellows did not survive to tell the sad tale which their diary disclosed The next appearance of the whales is on the north-west fishing ground in about the latitude of Prince Charles' Foreland, where they are met with by the whalers in the middle of May, and continue in their summer resort till the end of June, awaiting it is presumed the breaking up of some ice-barrier which bars their further passage.

Certain it is, however, that about that time they suddenly disappear for the season in a north-westerly direction. After the middle of June, whales are generally found in about 72° to 75° N. and 13° W. longitude off the east coast of Greenland. This is known as the south fishing ground, and from here the vessels usually take their departure for home early in August; but some very large whales have been killed in the end of August and early in September, indicating the return autumn route of the whales to be along the land-water of the east coast of Greenland. The old males (not so often females) frequently remain off the coast all the summer till the return of the females and young or growing whales; they approach the shore in autumn, when they join forces and work south together to their winter quarters.

The movements of the whales frequenting the Greenland seas are subject to considerable uncertainty; the vast extent of ice with its varying and irregular distribution, the influence of prevailing winds, and the presence or absence of food-supply render it very difficult to predict the precise locality in which they will be found at any given period of any particular year. There are also many other disturbing influences, some of which are not at present well understood; but the above is, I believe, a fairly accurate outline of the usual routes and times of migration of the Right Whale in the Greenland waters. It is traced on the accompanying chart.

A few of the many interesting features with regard to the migration of the Right Whale may here be mentioned. Not only is it probable that a separation of the sexes takes place at certain seasons, but also that to some extent the individuals of various ages form themselves into separate communities and occupy stations of their own. It has been observed that the older whales, especially the males, frequent more open water than the females and growing whales, the latter preferring the vicinity of the fast and solid ice. This would account for the presence of the large whales in 70 to 75° N. already mentioned; but in former times some very large whales, known as 'blue-water' whales, used to be captured far from the ice; some of these have yielded as much as twenty-eight or thirty tons of oil, but they are now very rarely met with. The last I have heard of was taken in 1875, and its longest slip of 'bone' (baleen) measured 12 feet 6 inches. These must have been very old whales, and it is possible that they may have been deposed from their leadership, perhaps by some more vigorous members of the herd, as occasionally happens with individual sperm-whales. The assortment, as it were, of the whales, according to size or age on the various feeding grounds, is a fact well known to the whalers, and Captain Gray tells me that the largest whales are taken between 70° and 75° N.; between 75° 30′ and 77 N. the sea is usually barren of whales; 77° to 78° 40′ N. produces 'second-sized whales' averaging from ten to twelve tons of oil; whilst from 79° to 80° 20′ N. are found only 'nursery whales,' averaging from five to ten tons of oil each. The following statistics will illustrate this:—In 1814 Captain Sutter made his celebrated capture of forty-four whales in 79° N.; they averaged only 5¾ tons each. In 1886 fourteen whales, killed by two vessels fishing together in the same latitude, averaged the same. In 1884 Gray killed four whales in 77° 30′ N., which averaged thirteen tons each; and the same year three whales in 73 N., which averaged seventeen tons of oil. In 1863, also in 73° N., he killed eight whales which averaged 16½ tons of oil. Scoresby, in about the same latitude, on the 15th August killed three whales, and a companion vessel a day or two later, a fourth; these averaged twenty tons each. Lastly, on the 21st June 1887, D. Gray, in 73 40′ N., killed a female whale which measured 57 feet in length, and yielded twenty-seven tons of oil; the longest slip of 'bone' of this monster measured 11 feet 4 inches, and weighed when cleaned 9 lb. 5 oz. Scoresby inclines to the opinion that these three classes of whales represent distinct 'tribes,' and that they adopt separate lines of migration; the latter is undoubtedly the case, but that they differ from the other whales, except in age or sex, is unlikely. Reasoning from analogy it seems not improbable that, if the adults of both sexes form themselves into different 'schools,' individuals at other stages of growth may do the same. We know that in the case of the Eared Seals the 'bachelor' or non-breeding seals separate from the breeding portion of the community, and it may be that the adult and non-breeding Greenland whales herd in separate flocks habitually, a still further sexual division of the adult flock taking place at certain periods. Scoresby (*l.c.*, p. 292) thinks that the females and young retire into the interior of the bays and sounds in the summer, which he considers is the commencement of the period of gestation; but even this, supposing it to be the case, would not account for the disproportion of 'suckers' to young growing whales, much more conspicuous now than in his day, and especially so in the Greenland seas. Females accompanied by suckers are now rarely met with, and it follows that they must either have a secure hiding-place in which to remain until the young are able to take care of themselves, or that reproduction has for many years been greatly arrested, or has almost ceased, the young whales found in latitude 80° N. being the last of their race. The age of these whales would fairly coincide with the introduction of steam, and it may be that the old whales have since been so harried at a period when rest and seclusion are absolutely essential, that they have ceased to perform their natural functions.

There is proof that individual whales resort to the same localities year after year. Captain Gray informed me that in 1856 his men observed a whale with a distinct white mark across one side of its nose; three years later they saw it again in the same place, and struck and lost it. In 1867 he chased a whale "with a growth like a bee-hive on the left side of its tail"; in 1872 he killed this same whale, and almost on the same spot. Writing in 1886, he said that in 1880 he chased a whale with a large white splash on its back, and that he had seen it every year since. He also states that whalers come to know strongly-marked individuals, and recognise them from time to time, thus showing that the whales follow the same line of migration for many successive seasons.

From east of Spitzbergen I can glean very little information with regard to the occurrence of the Right Whale; nor, when we consider the requirements absolutely essential to its well-being—ice of great extent, and of a sufficiently open character, deep water, and an abundance of the minute organisms which are consumed in such vast quantities to nourish its mighty fabric—is it reasonable to expect that the shallow waters of Barents Sea should be largely visited by it. It is true, as pointed out by Eschricht and Reinhardt (*l.c.*, pp. 25, 26), that the ancient Dutch whalers speak of a whale which they distinguished as the 'South-ice' whale, and believed that it came from the east round the south coast of Spitzbergen; but very little seems to be known about it, and that little does not affect our present enquiry, although, as these authors remark, it ought not to be totally disregarded, whether a separate species or not. But it must be remembered that Stephen Bennett and Jonas Poole, walrus-hunters, who frequented Cherry Island from 1603 to 1609, say no word about whales being found there at that time; and seeing that it was the same Jonas Poole who in 1610 brought home the news of the great abundance of whales off the west coast of Spitzbergen, which led two years later to the establishment of the successful whale-fishery from that shore, they would hardly have remained silent had they made so important a discovery. Eschricht and Reinhardt state that "it has been proved by K. E. v. Baer [1] that the whale has not been seen near the coasts of Nova Zembla." The difficulty of determining the species of 'whales' recorded as seen by modern travellers here presents itself, and it is impossible to say with certainty to what species certain whales mentioned by Nordenskiöld [2] belonged. Speaking of the Right Whale by name, however, the last mentioned author says, "thus during our many voyages in these waters we have only seen one such whale, which happened on the 23rd June 1864, among the drift ice off the

[1] *Wiegmann's Arch. fur Naturgeschichte*, vol. i., p. 168.
[2] "Voyage of the *Vega*," vol. i., p. 169.

coast of Spitzbergen in 78° N.L.," which is the usual hunting-ground.

Nordenskiöld further states that fragments of the skeletons of whales, thrown up in such quantities on the shores of Spitzbergen, are not in his experience to be found on the shores of Novaya Zemlya, or on the coasts of the Kara Sea and north coast of Siberia, between the Yenisij and the Lena at which he landed. Colonel Feilden also tells me that bones of the White Whale were the only cetacean remains he saw on the shore of Novaya Zemlya, either on the Barents or the Kara Sea side of the islands. Should the Right Whale stray to the eastward, it would seem more likely that it should do so by a passage north of Spitzbergen, and the only certain records known to the writer of the remains of this species having been observed in such longitudes are the following. In 1897 Mr Arnold Pike "on the summit of the basaltic ridge, say 150 feet, which juts out from Cape Hammerfest [in one of the islands of Wiche Land, east of Spitzbergen] found old whales' bones, mostly very much decayed."[1] In the more northerly parallel of Franz Josef Land, Leigh Smith,[2] on his first visit in 1880, found portions of the skeletons of "two whales" on the shore near Eira Harbour; these, Mr W. S. Bruce of the Jackson-Harmsworth expedition tells me, are of very ancient date. With regard to the two Right Whales said to have been seen by Mr Leigh Smith passing out of Gray Bay, Mr Bruce kindly informs me, on the authority of the experienced ice-master of the 'Windward,' who was with Mr Leigh Smith in the Eira, and saw the whales in question, that they were not of the species under consideration. Mr Bruce also says that nothing was seen of Right Whales during the time the expedition remained at Cape Flora. The whalers, 'Balaena,' 'Active,' and 'Diana,' which visited Franz Josef Land in 1897, were also unsuccessful in their search. No Right Whales appear to have been seen by De Longe from the 'Jeannette.' Weyprecht, in his paper on the scientific work of the 'Tegethoff,'[3] expressly says that the only species of whale met with by them was the White Whale, "near the coast, but pretty often." Of the supposed occurrence of this species east of Spitzbergen, I have not met with a single recent instance that has borne investigation, and I doubt whether in the present day it passes in that direction beyond longitude 20° east.

The Right Whale thus appears certainly not to extend its migrations regularly to the eastward of Spitzbergen, and is probably absent altogether from the Kara Sea and the Siberian waters till we reach Cape Schelagskoi in 171° E. longitude. West of this Von

[1] *Geogr. Journ.* April, 1898, p. 368.
[2] *Proc. Roy. Geog. Soc.*, 1881, vol. iii., p. 136.
[3] *Journal Roy. Geog. Soc.*, 1875, xlv., p. 32.

Wrangel, the Russian explorer, in 1823, says they are not to be found; but passing to the eastward they become more abundant as Bering Strait is approached, and to this he attributes the increase of population he found along the shore in that direction.

We may therefore, I think, dismiss from our minds any idea of the Right Whale habitually visiting the seas to the eastward of Spitzbergen; or in the present day of any interchange or overlapping of the individuals of this species inhabiting the Greenland seas and those to the north of Bering Strait by means of a north-east passage or the reverse, whether by westward drift or voluntary migration. I shall, however, later on have something to say with regard to the probability of the Bering Strait whales visiting the Atlantic by a west-to-east migration.

The whale-fishery in the Greenland seas has, as an industry, almost reached a vanishing point, and the vast fleet of costly vessels hailing from many ports, which formerly visited these waters every summer, had in the past season of 1897 dwindled to three vessels only, from the one port of Dundee; by these only two whales, one of which was killed, were seen. The absence of whales is not absolute proof of their non-existence, but may depend on various causes, some of which at least are well known to the whalers, as I have endeavoured to explain from time to time in my annual "Notes on the Seal and Whale Fishery," published in *The Zoologist* (1884-98). Here I can only say that they depend mainly on the condition of the ice; but there is no doubt—more especially since the introduction of steam—that the race, if for a few years longer saved from absolute extinction, will owe the extension to its acquired habits of greater caution;[1] and to the expensive nature of the outfit required for its pursuit rendering the business unremunerative.

PART II.—WEST OF GREENLAND

The difficulties encountered in an attempt to trace the periodic migrations of the Right Whale in the narrow seas to the west of Greenland are by no means of the same nature as those experienced when following its movements in the vastly more extensive area of the Greenland Seas proper. Not only is the field of observation more restricted, but although the pursuit of the whale in these waters is comparatively a new occupation, it has been more thoroughly and systematically worked, for, in addition to the records of the Danish factories on the west coast of Greenland, there have been voyages undertaken by men of considerable scien-

[1] In the year 1697, 188 vessels killed in that one season 1959 whales off Spitzbergen. That these too confiding animals soon forsook bay after bay of these blood-stained waters for more secure quarters is not surprising.

tific training, if not for the sole purpose of studying the habits and economy of the whales, certainly with very excellent results in that direction. Scoresby, of course, takes the leading place, but Dr Robert Brown's observations (before referred to) are of great value, and the routine of a modern whaling voyage has been admirably narrated by Captain (now Admiral) A. H. Markham from his personal experiences on board the 'Arctic' whaler in 1873.[1] In addition to these there are many references to the subject in the journals of the voyages undertaken for Arctic discovery, and in the reports of the whalers themselves, many of whom are men of great intelligence.

It may be well first briefly to sketch in outline the course pursued by the whales in their annual migrations, leaving the details, more especially the autumn movements of the females and young whales, for fuller consideration later on. To the westward, the southern limit of the Right Whale's winter resort in the present day appears to be about the 57th or 58th parallel of north latitude off the Labrador coast. In April and May they are met with off the entrance to Hudson Strait and Resolution Island; the old males enter Davis Strait, and as the ice retreats make their appearance in the neighbourhood of Disco. Here they bear to the west, and crossing Baffin Bay join the female and immature whales which have arrived before them (coming north by a route through the heavier ice nearer the western shore), and wait the breaking up of the ice in Lancaster Sound, which generally takes place in July.[2] They then pass into the Sound, and disperse into Prince Regent's Inlet and other ramifications of the deep channels intersecting the Northern Archipelago, and spend the summer in these waters. When the ice begins to form in autumn, and it becomes necessary to beat a retreat, the whales commence their southward journey in a leisurely manner; and this also appears to be performed in two sections, the old male whales returning along the west shore of the bay and the females and young ones by a more circuitous route, which I shall endeavour to trace in due course. These routes I have laid down on the accompanying chart.

Dr Robert Brown is of opinion that the Right Whales which frequent Davis Strait in summer pass the winter and produce their young all along the broken water off the coast of the southern portion of that Strait, also in Hudson Strait and Labrador. The when and the where of the reproduction of this species is a very interesting question, and too large an one to enter upon here. It is certain,

[1] "A Whaling Cruise to Baffin Bay, &c., in 1873." London, 1874. The s.s. 'Arctic' in which Captain Markham sailed was lost in Davis Strait the next season, and her successor, a fine vessel of 522 tons, was so severely nipped in Fox Channel in 1887 as to render her abandonment in Cumberland Gulf necessary.

[2] These dates are only approximate; so much depends upon season, prevailing winds, and the varying condition of the ice.

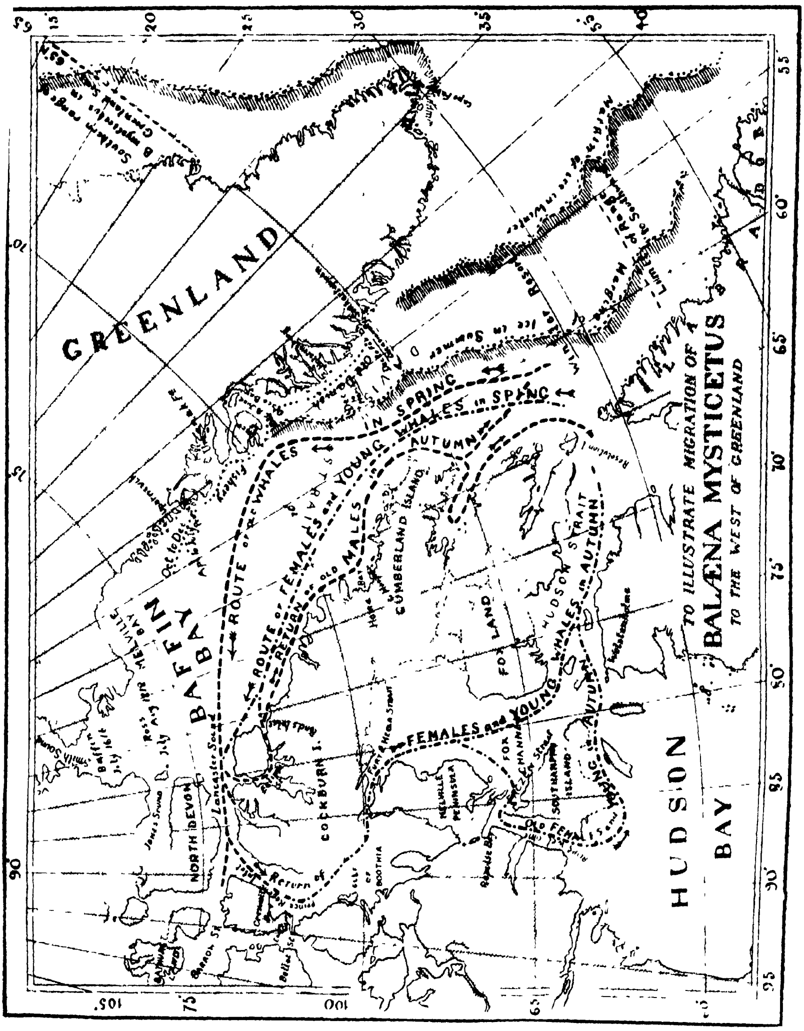
however, whether or not this function is performed at that season, that the whales are found in the localities just indicated early in the year, and they have doubtless wintered there. Off Resolution Island the 'Arctic,' in which Captain Markham was a passenger, killed her first whale on the 23rd of May 1873; but, owing to the unsettled weather which usually prevails at this season, and to the dangerous nature of the ice, this, which is known as the 'South-west Fishing,' is not much frequented by the whalers. This is doubtless the starting-place of the most southerly contingent of the migrating army of whales in Davis Strait, and from this point the whalers, or such of them as have been to the south-west fishing, take their departure for the north at the end of May, and as a rule see no more of the whales till they reach the fishing-ground off Ponds Bay or Lancaster Sound. On their journey north the immature whales with the females, Captain D. Gray assures me, keep close along the west side of the straits, finding breathing-room in the cracks and lanes of water always found amongst the ice, and arrive at or near Eclipse Sound about the 15th of June.

We will now proceed to the east side of Davis Strait, where a totally different disposition of the ice, and consequent distribution of the whales will be found. Fortunately, there exists a very remarkable set of records in connection with a whale fishery established here by the Danish Government, and these have been analysed by Messrs Eschricht and Reinhardt (*op. cit.*). These Stations or 'Factories' extend along the shore of West Greenland for a distance of 200 Danish miles, from the 60th to the 73rd degree of north latitude, in a direction almost due north, but only part of this extent of coast is, or was, visited by the Right Whale, the most southerly station being Sukkertoppen, in latitude 65° 25′ N., and the most northerly, Upernavik, in 72° 48′ N. At these factories the fishery was prosecuted from the shore by means of boats, a look-out being kept. When a whale was discovered, weather and ice permitting, the boats put off, and, if successful, towed the dead whale ashore, where it was flensed. Accounts were kept and returns made to the Colonial authorities at Copenhagen, not only of every whale killed but also of those seen from the look-outs. These records extend from 1780 to about 1839, and from them it appears that the whales were seen at Sukkertoppen in the months of December, January, and February, when they entered the larger sounds and fjords in very unequal numbers, in greater numbers the more severe the winter and the more the sea was filled with floating ice. Bearing in mind that the winter condition of the ice in the parallel of 65° on the east side of the Strait would about correspond with that on the Labrador coast five degrees farther south at the same time, and the fact of the whales appearing in greater and less numbers in accordance with the severity of the weather, this more northerly wintering station is quite in conformity with the habit of these creatures of at all times frequenting the loose ice on the margin of the pack. At Holsteinborg (66° 56′ N.) the whales, as recorded in the journals, made their appearance generally in the first half of December, and remained along the coasts or in the fjords till March. Further north, in Disco Bay (about 69° N.) they appear about the same time as at Holsteinborg, but stay longer, generally having departed by the middle of June;[1] whilst in Omenak Sound (71° N.) "they are found not only through the whole month of June," but even as late as the beginning of July. Finally, between Proven and Upernavik, situated between 72° and 73° N, "the whales make their appearance considerably earlier than at the more southern parts of the coast. They have been regularly observed there by the month of October, and in some instances even at the end of September. They are then seen through November and some part of December, and again towards spring from April to July," evidently indicating a double migration. These writers further observe that, so far as can be ascertained by records dating back to 1721, although the numbers are vastly reduced, there has been absolutely no change in the time or route of the migrating whales. It must be understood that these observations all refer to the east side of the Strait, where the extent and condition of the ice differ entirely from those prevailing in the same latitudes on the west side.

Before following the whales to the westward, it will be well to ascertain, if possible, how far their range extends in the direction of Smith Sound. Baffin, who was the first to penetrate to this northern latitude, in the year 1616, saw many whales in Wolstenholme Sound, also in 78 N., at the entrance of Smith Sound; Ross, too, in the year 1818 met with whales between 75 and 76° N. in July and August, during which months, it will be remembered, they are absent from the coast along which the Danish Factories are situated; but I am inclined to think these instances are quite exceptional and that the Right Whales seldom penetrate farther north than latitude 75° or perhaps occasionally to the entrance of Jones Sound. That they are to be looked for with success in the extreme north appears very improbable, and on this point Captain (now Colonel) Feilden, the naturalist to the voyage of discovery under Sir G. S. Nares in 1878, speaks very decidedly; he says[2] "I am, however, quite satisfied on one point, and that is, no whale could inhabit at the present day the frozen sea to the north of Robinson Channel. To penetrate thither from the north-water of Baffin's Bay would be a too hazardous

[1] Here whales are sometimes taken by the whalers on their way north in the month of May.

[2] "Narrative of a voyage to the Polar Seas during 1875-6, with Natural History Notes," by H. W. Feilden. 2 vols. London, 1878. Vol. ii., p. 197.


TO ILLUSTRATE MIGRATION OF A
BALÆNA MYSTICETUS
TO THE WEST OF GREENLAND
GREENLAND
BAFFIN BAY
HUDSON BAY
HUDSON STRAIT
CUMBERLAND ISLAND
FOX LAND
COCKBURN I.
NORTH DEVON
MELVILLE PENINSULA
SOUTHAMPTON ISLAND
ROUTE of WHALES
WHALES in SPRING
IN SPRING
AUTUMN
ROUTE of FEMALES
RETURN of OLD MALES
FEMALES and YOUNG
WHALES in AUTUMN
LABRADOR

task for this great animal, and in this opinion the experienced whaling quartermaster who accompanied the expedition coincided. We may dismiss from our minds the idea or hope that nearer the pole, and beyond the limits of present discovery, there may be haunts in the Polar Sea suitable for the Greenland whale; . . . and I see no hope of Arctic discovery increasing our knowledge of the range of this animal." That it is impossible for the whales to pass more than a very limited period in ice of too close a texture is certain, as it would not only impede their breathing but also prevent their feeding, for both of which purposes open spaces are necessary. That whales are subject to the risk of suffocation is beyond doubt, and Eschricht and Reinhardt (*l.c.*, p. 12) quote an instance of its actual occurrence from an old MS. account by a Mr Geelmuyden, who states that in the year 1750 "the masses of ice in Disco Bay were uncommonly large, staying there until the year was far advanced; that the fishing therefore, generally speaking, had been very bad, but that the Greenlanders had been fortunate enough near the Dog's and Whale's Islands, at the outlet of Disco Bay, to find no less than fourteen whales, 'which had perished by themselves beneath the ice,'" evidently suffocated for want of breathing spaces.

It was not until the year 1817 that the whalers ventured to enter the pack in Melville Bay, and until the introduction of steam the passage through this dreaded obstruction was by no means certain. Now, however, having reached the 'north-water' of Baffin Bay some time in June, they are accustomed to find the whales waiting the disruption of the ice at the entrance of Lancaster Sound.[1] As soon as that takes place all the whales pass up the Sound, and entering Prince Regent's Inlet, or it may be some of them continuing into Barrow Strait, remain until the autumn ice compels them to return southward. How far west the whales extend their wanderings in this plexus of Straits and Sounds it is difficult to say, but it seems probable that they do not pass in that direction much beyond Prince Regent's Inlet or a short distance into Barrow Strait, seldom so far as to meet the whales from the Pacific side, which probably also do not penetrate far into the narrow ice-bound channels between the islands of the Northern Archipelago. The only Cetaceans which I have found mention of in Jones Sound are White Whales, and these appear to be very numerous in that locality.

It has been observed that in the return or southward migration along the west side of Davis Strait in autumn, there is an absence of female and immature whales, and the question arises, by what route do they return? This I will now endeavour to trace. Sir John Ross, on the 15th of August 1829, when off Bellot's Strait, said "many whales of a light colour [a sign of immaturity] came close to

[1] The 'Arctic' took four small whales here on 15th of June 1873.

us, appearing to be quite indifferent to the presence of the ship"; also on the 20th "many whales," and on the 21st two "large whales," and finally in 70° 54′ N. on the 22nd a "large whale," after which I do not find mention of any others being seen. Markham, whose journey extended to Cape Garry,[1] says the whales were all heading along the Inlet to the Gulf of Boothia, where Captain Adams had at one time thought of following them through Fury-and-Hecla Strait and Fox Channel, or through Frozen Strait into Hudson Strait, an intention he unfortunately abandoned; but he evidently had a shrewd idea that such was the course taken by the whales. Ross, in the appendix to his second voyage, mentions that the natives of Boothia told him the Right Whale "is rarely seen either on the east or west side of the Isthmus . . . only two were seen by us during the three years we were frozen up in that neighbourhood." This seems to indicate that they cross the head of the Gulf to the unexplored coast of Cockburn Island and pass through Fury-and-Hecla Strait, for Dr Richardson tells us, in the appendix to Parry's Second Voyage (p. 336), that they are frequently seen near the shores of Melville Peninsula, in Frozen Strait, and in Hudson Strait; whilst at p. 510 of the narrative of the same voyage, it is stated that the natives meet with most whales on the coast of Eiwillick, where the Hudson Bay Company once carried on a whale-fishery, but had then (1825) abandoned it for some years. It is worthy of note, as indicating the direction in which the whales were travelling, that during Parry's stay off the coast of Melville Peninsula the only month in which he saw these animals was August, and the earliest dates are in localities farthest north; thus, about the 5th of August (1822) they saw Black Whales off the east entrance to Fury-and-Hecla Strait; on the 17th of August (1821) a "great number" of Black Whales were seen playing about the beach off Frozen Strait; on the 22nd more were seen in Repulse Bay; and on the 28th of August three Black Whales were seen off Rouse Island in Hurd Channel; these latter were probably on passage to Rowe's Welcome. No whales were seen going north in the earlier part of the year, and indeed the late date at which the ice breaks up in these narrow straits would render it quite impossible for them to be used by the whales on their spring passage north, and it is not unlikely that this is one of the reasons for their going north by Davis Strait.

We have thus traced the whales through Lancaster Sound into Prince Regent's Inlet, have found them abundant in the neighbourhood of Cresswell Bay and Bellot Strait, rare on the east coast of Boothia, owing, it is suggested, to their crossing the Gulf at this point

[1] It is worthy of mention that the 'Arctic' killed an old female and a small male whale at this spot on August 9th and 11th respectively, during which month they are not often met with in Lancaster Sound.

to pass through Fury-and-Hecla Strait, at the east entrance of which we again find them abundant, as also all along the shore of Melville Peninsula into Hudson Strait. A detachment goes west through Frozen Strait into Repulse Bay and Rowe's Welcome (where the 'Perseverance' has wintered for the three past seasons, securing six whales), and finally, either winters in that locality or passes round by Southampton Island, and rejoins the main body to winter in Hudson Strait or thereabout. The route will be found marked on the accompanying chart (Pl. xii.), for which I am mainly indebted to Captain David Gray, and however unlikely it might seem for the whales in any number to pass through so narrow a passage as Fury-and-Hecla Strait, there appears every reason to believe that this is really the route chosen by a definite section.

It is a much more simple matter to follow the old whales on their return journey along the west side of Baffin Bay and Davis Strait. At the approach of winter, when the young ice begins to cover the bays, the old whales turn southward, many of them passing through Eclipse Sound and Pond's Inlet. They travel along the west side of the strait, being due at Home Bay and Cape Hooper about the 15th of September. From their habit of hugging the shore (a habit which it will be remembered they share with the whales found under similar circumstances on the east coast of Greenland), these whales are known as 'rock-nosers.' They continue working south till they reach Cumberland Gulf, which is their last resting-place before the accumulated ice drives them into the more open water farther south. There they remain until the returning spring enables them once more to commence their northward migration along the broken margin of the ice trending towards the east side of the strait. So well are these different lines of migration recognised by the whalers, and so constant are the characters of the individuals using them, that, as in the Greenland seas, they are by some regarded as different 'races,' and called after the localities in which they are found, as 'middle-icers,' 'rock-nosers,' and 'Pond's Bay fish,' but, as Dr Brown remarks, these distinctions do not designate separate "species, or even varieties, but express a geographical fact and a zoological habit" (*l.c.*, p. 80).

As mentioned when speaking of the whales inhabiting the seas to the east of Greenland, there is also abundant evidence of the Davis Strait whales likewise frequenting the same locality year after year; perhaps the most recent instance of this is the most remarkable. In September 1894 Captain M'Kay of the 'Terra Nova' killed in Davis Strait an unusually large whale, in the blubber of which he found a harpoon, the steel of which was quite bright, bearing the name of the 'Jean' of Bo'ness, dated "forty years back." The 'Jean' of Bo'ness was lost in Davis Strait "thirty-seven years ago"; the whale must therefore have frequented Davis Strait for at least thirty-seven years.

It is an open question whether an interchange of inhabitants ever takes place between the regions lying to the east and west of Greenland, and, if so, by what route it is effected. Dr Brown is strongly of opinion that the whales of Spitzbergen never as a body visit Davis Strait. Sir William Flower, in a lecture delivered at the Royal Colonial Institute,[1] says that "though found on both sides of Greenland, and passing freely from one side to the other, it is never found so far south as Cape Farewell." Whilst quite agreeing with the latter part of Sir William Flower's statement, such being admittedly the case, the southward passage not being available for them, I could wish that this eminent authority had indicated by what other means the interchange might take place. Supposing the whales to pass round the north of Greenland,[2] the heavy ice in the upper reaches of Kennedy and Robinson Channels would be fatal to their reaching the west coast, and Cape Farewell for many reasons is out of the question. When it was believed that a passage existed through Mid-Greenland from sea to sea, no insuperable barrier to such an interchange appeared to present itself, but it seems impossible that it should be accomplished by means of any passage known to us in the present day Let us look at the evidence. The only instance of the supposed passage from Davis Strait to Greenland seas with which I am acquainted, is on the authority of Captain D. Gray, who admits that he has found little evidence of such an interchange. He says, however, that the 'Kate' of Peterhead, in 1866, in 80° N., killed a small whale in which was found an Esquimaux harpoon; this he considers conclusive proof of its Straits origin, "there being no natives on the east coast of Greenland;" as, however, he is wrong in this supposition, the occurrence is of no value. Some of the stories of such visits of Greenland whales to Davis Strait are very circumstantial; I will only give two which appear to be the best substantiated. The first is from Eschricht and Reinhardt (*l.c.*, p. 24), and they rightly remark that "if it can be completely depended upon, it is deserving of peculiar mention." It is derived from an account given by Paul Egede, to the effect that the commander of a whaling expedition (in 1787) in Davis Strait found a whale drifting with a harpoon sticking in it, which he recognised as that of his brother, and which, on his return, he learned had been put into the whale near Spitzbergen only two days before he found the animal dead in Davis Strait. The second instance I give from

[1] *Journ. Roy. Col. Inst.*, February 1895, p. 163.
[2] The most northerly known point of North Greenland extends to 83° 24′ N., and Peary thinks it likely it reaches even to 85° N.

Dr Brown's "Notes on the Cetacea of Greenland," as reprinted in the "Arctic Manual" (p. 76). "I was told," says Dr Brown, "by the late Captain Graville of the s.s. 'Diana,' a proverbially experienced and truthful man, that a whale was struck near the entrance of Scoresby's Sound, on the east coast of Greenland, by the father of the late Dr Scoresby (with whom Mr Graville was a fellow-apprentice); but, being lost, it was killed next day near the entrance of Omenak Fjord, on the east coast, with the harpoons freshly imbedded in its body. This was adduced in proof of the existence of an inlet in former times (as, indeed, represented on old maps) across Greenland between these two points. Unless the whole story was founded on a misconception (an event even less likely from the searching investigation which took place at the time), we can scarcely believe that the whale could have reached the west coast by any other means; for, even allowing the greatest credible speed, it comes scarcely within the limits of possibility that it could have doubled Cape Farewell and reached 70° N. latitude within the interval mentioned." Here I must leave these wonderful but apparently well-accredited stories, simply remarking that with our increased knowledge of the geography of Greenland, and of the habits of the Right Whale, they seem even more wonderful than ever.

When considering the migration of the Right Whale in the Greenland Seas, I briefly stated the difficulties, if not the impossibility, of their passing from the Pacific to the Atlantic, or the reverse, along the northern shores of Europe and Asia. It may be well to say a word in passing with regard to the probability of such an interchange along the coast of North America. The 'Bowheads' (by which name the Northern Right Whale is known to the Americans) wintering in the North Pacific are seen by the natives of Alaska, as stated by Captain Macguire, of H.M.S. 'Plover,' to make their appearance off Cape Hope in April and May, when the ice breaks up into fields, and most of them have disappeared before the ships are able to follow them. They then pass through Bering Strait, a portion going westward, but to no great distance (see *ante* pp. 404-5); but the bulk bears to the east, where they are pursued by the natives of Alaska, and followed, as soon as the ice permits, by the American whalers, who, since the year 1848, when they first passed the Strait, have established a lucrative fishery in the neighbourhood of Point Barrow. Probably, owing to the difficulty of passing this icy promontory, they do not follow the whales much further east, but the Esquimaux pursue them as far as Cape Bathurst, their season commencing on the 7th of May, and continuing till the month of June, at which time the whales disappear, to return on their way to their winter quarters in August and September; in July and early August they are seldom seen. On the 19th of September the 'Investigator,' whilst struggling in the ice in Banks Strait, between the north of Banks Land and Melville Island, saw two Right Whales going to the westward, apparently on their return journey to Bering Sea. Thus there seems every probability that the Pacific whales do visit the Polar Archipelago and return to their native seas; but they reach this, the limit of their journey in an easterly direction, so late in the season that it seems unlikely they should mingle with, or, perhaps, even more than occasionally meet with, the contingent from Baffin Bay.

There are several apparently well-authenticated instances on record of harpoons having been found in whales killed on the Pacific side of the globe, which were identified as having belonged to the Greenland (West Greenland?) whalers.[1] G. Brown Goode, in the volume of the Census Report of the U. S. Commission of Fisheries for 1887 (p. 97), says that whales have been taken in the neighbourhood of Point Barrow with harpoons in them bearing the marks of vessels that had been pursuing the fishery in the vicinity of Davis Strait, and gives the details of such an occurrence. Scoresby also mentions some instances of supposed Pacific whales having been killed in Greenland; but I do not attach much importance to them, since the stone lances and bone harpoons on which he relies, and which are attributed by him to the Esquimaux of the Pacific coast, might almost as well have had their origin, perhaps many years previously, seeing how long lived these cetaceans are, on the shores of Baffin Bay. Admitting the instances quoted to be genuine, they merely go to prove that should an interchange of Atlantic and Pacific whales take place it is much more likely to be by way of the 'North-west Passage' than along the shores of Asia and Europe. That the Right Whales inhabiting the two seas are specifically distinct there is no reason to suppose; some racial difference there may be, but even this has never been clearly defined.

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[1] See Eschricht and Reinhardt, p. 46.

SOME NEW BOOKS

THE TEACHING OF BOTANY: A CONTRAST

ELEMENTARY BOTANY. By Percy Groom, M.A. 8vo, pp. x + 252. With 275 figures. London: Bell & Sons, 1898. Price, 3s. 6d.

LESSONS WITH PLANTS. Suggestions for seeing and interpreting some of the common forms of vegetation. By L. H. Bailey. With delineations from nature by W. S. Holdsworth. 8vo, pp. xxxi + 492. New York: The Macmillan Co., 1898. Price, 7s. 6d.

MR GROOM has produced a very serviceable little text-book for beginners in botany. It is an ominous sign that a man trained at Cambridge and writing from the Oxford botanical school should be the author of a botanical book for the proper understanding of which the use of a compound microscope is quite unnecessary. Such, however, is the case, and though, in our opinion, no aid to the imparting of knowledge should be despised, we welcome a recognition of the fact that a very great deal may be learned about plants with the help of a sharp penknife and a pocket-lens. The microscope is fascinating, but the beginner is apt to be bewildered by overmuch detail, and lose sight of the broad principles of general form and function. In recommending this book to teachers of elementary classes, we suggest that an occasional and discreet use of the microscope will be a gain.

The work, which deals only with seed-plants, falls into the usual three parts—general morphology, classification, and physiology. It is extremely difficult to clothe the bare facts of morphology so as to make them interesting, and Mr Groom fares neither better nor worse than many other authors; one chapter of the thirteen devoted to this section, that, namely, on pollination, may perhaps be read. For the rest, Part I. excellently fulfils its function as a text-book, that is a complement to the lesson and practical class. Similarly, Part II. is an excellent guide to the study of about thirty of the more important natural orders; but it is a pity to repeat in an elementary book the fable of the 'distinct disc' in Geraniaceae; it is as distinct as the perigyny in British Leguminosae. *A propos*, also, of the general table of classification, we notice that the ovary of Liliaceae is said to be inferior—an obvious slip. The same, however, cannot be said of the inclusion of Iridaceae under the head 'flowers actinomorphic'; the zygomorphic *Gladiolus* is so well-known a flower and the second largest genus in the order, that some qualification of the clavis character is advisable. Part III. (Physiology) is somewhat brief, but up-to-date and accurate. Finally, a word of praise is due to the profusion of illustrations, which are clear and helpful without being in the least elaborate.

Perhaps it is hardly fair to compare the second book before us with the one just noticed. Mr Groom's is an elementary text-book, good, but without pretensions to literary excellence or striking originality. Prof. Bailey's is a series of lessons learnt from the plants, but learnt as a child learns, gradually and by intimate association with the objects themselves. And it is also a book to read, for Prof. Bailey has a style of his own, bright, easy, and readable; we almost forgive him his unfamiliar spelling. It would be an interesting experiment to compare the results of teaching on the plan of these two books respectively. The text-book might perhaps score most in examinations with a restricted syllabus, though we are not certain even of this, but for mental development and the fostering of a love for plant-knowledge Prof. Bailey's 'Lessons' would be *facile princeps*. The best description of the book is its title. Its use to the teacher is to suggest what material to put before his students, and what lesson to let them imbibe from it; only, as the author insists, one lesson at a time. If the teacher is worth his name, he will find no difficulty in extending the scope of the book on the same lines, and he will also find that he is learning as much as his pupils. When the student has not the advantage of a teacher, Prof. Bailey puts him well in the way of helping himself. But neither teacher nor student must rest satisfied with the pictures with which the book is so lavishly supplied. They will help wonderfully to elucidate the living specimens; but if they are used instead, the object of the book will be frustrated. It is worth the price of the volume to get almost for every page a new illustration. Dear old Sachs & Co. were very useful, but the blocks have got much worn of late. But it is impossible to review this book. One keeps looking at the pictures and reading bits, and the editor wants copy to-morrow. Suffice it to say that it is divided into seven parts: studies of twigs and buds; of leaves and foliage; of flowers; of the fructification; of the propagation of plants; of the behaviors (*sic*) and habits of plants; and of the kinds of plants; with an appendix containing many useful suggestions on the collecting and preserving of plants, on books, on how to build a school-house and lay out the garden, and a glossary. Long live Professor Bailey!

The American Bureau of Ethnology

Four volumes of the "Annual Reports of the Bureau of Ethnology" are to hand, being the fourteenth (two volumes), fifteenth, and sixteenth Reports, 1892 to 1895. The usual high standard of this important ethnological publication has been amply maintained, both in the subject material and in the style of production. As hitherto, the illustrations are very numerous and excellent.

The fourteenth Report (1892-3), Part I., forms a bulky volume containing two important and lengthy papers. The first is a "Monograph on the Menomini Indians," by W. J. Hoffman, M.D., a detailed descriptive account of a tribe of Indians of Algonquian stock, nearly related to the Ojibwa, and located on a reservation in the N.E. part of Wisconsin. This tribe has been referred to in print under upwards of eighty synonyms, which are quoted in a list. The paper deals in detail with the form of government, the various societies associated with special cults, mythology and folk-lore, everyday life and manufactures, describing the various appliances used in the various occupations. There is no lack of illustrations, and a vocabulary is appended. The arrangement of the paper is systematic, and is thus well suited to quick reference, the list of contents and headings to sections rendering easy the search for special points. These monographs upon special tribes are of great value to the ethnographer, and it is much to be desired that their general plan of arrangement should be as uniform as possible.

The second half of this volume is devoted to an elaborate historico-ethnological paper by G. P. Winship on "The Coronado Expedition of 1540-42." The narrative of this, together with the earlier expeditions of Francisco Vasquez Coronado, forms a chapter in the history of the Spanish conquest of America. The Spanish text of Castañeda's narrative is given in full with a translation, and translations of several other accounts, communicated by other writers in the form of letters to various people, are also given. The reproductions of a number of important sixteenth century maps add much interest to the historical information conveyed in these narratives, while the excellent photographs of the modern Pueblo Indians, their dwellings and occupations, serve admirably to illustrate the incidental passages which have reference to the manners and customs of the native inhabitants of New Mexico in the sixteenth century. The early ethnographic observations are of value as giving an indication of the amount of change which the native culture has undergone since the Spanish invasion of the country.

Part II. of the same report is a volume devoted to a very exhaustive paper upon the "Ghost-Dance Religion and the Sioux Outbreak of 1890," by James Mooney, a writer eminently qualified, by his long personal acquaintance with the customs and ritual of the Indian tribes, to act as chronicler of the curious circumstances which for a while created so much stir and trouble but a few years back. Mr Mooney has mapped out the area of the ghost-dance, showing a wide distribution in the Central and Western States of North America. He treats his subject historically, dealing with the relations between the Indians and the Whites, and the causes of the various outbreaks of the former in their endeavour to throw off the yoke of the latter. Most, if not all, of the Indian tribes have held belief in the coming of a Messiah or Deliverer who will restore them to their original happy state, and numerous prophets have arisen who have taught the religious dances, which may collectively be classed under the term Ghost-Dance, and which are elaborately symbolic, relating to the doctrine of the future blissful state. The history and acts of the celebrated 'Messiah' Vovoka are gone into in detail, together with the causes of the recent Sioux outbreak. A mass of material of both historical and ethnological interest is brought together in the paper. The individual campaigns are described and illustrated by maps. Of special interest to ethnographers is the minute description of the ceremonials of the ghost-dance, and the extensive collection of myth-songs recorded.

The Fifteenth Report (1893-4) contains a paper by W. H. Holmes on "Stone Implements of the Potomac-Chesapeake Tidewater Province," in which the author makes a detailed and plain statement of his views as to the nature of the 'quarry-workshops' and their products, reasserting his belief in the necessity of attributing them to the agency of the historic Algonquian Indian instead of referring them to a remote period and a different culture. Much, of course, is repeated from his former papers on the subject, but so plain and straightforward a statement of his views, which are very convincing, must facilitate discussion and tend to focus it. His description of the steatite working is of much interest.

This is followed by a general paper on the "Sioux Indians" by W. J. M'Gee, describing briefly their general culture and beliefs, and serving as introduction to a posthumous paper on "Siouan Sociology" by J. O. Dorsey, dealing with the tribal divisions of the Dakota.

J. W. Fewkes furnishes a paper upon "The group of Tusayan Ceremonials called Katcinas." The complex symbolic performances, in which masked figures impersonating mythological beings play a large part, are treated very fully, and illustrations of the performances and appliances embellish the paper, which is not the least important of the published results of the Hemenway expedition.

A paper describing the work upon the "Repair of the Casa Grande Ruin" by Cosmos Mindeleff terminates this volume.

The Sixteenth Report (1894-5) commences with an elaborate memoir on "Prehistoric Trephining in Peru," by M. A. Muniz and W. J. M'Gee. The subject of trephining as exhibited by primitive peoples has been kept very much alive of late years, and this finely-produced and well-illustrated paper is a welcome addition to the literature. It refers to the practice in Peru in pre-Columbian times, and evidences several methods by which trepanation was effected, chiefly or wholly produced by operation with stone implements. Post-mortem trephining and 'cranial amulets,' as described by Broca, are not evidenced in the Peruvian collection. The number of trephined skulls averages 2 per cent. in a collection of one thousand brought together by Dr Muniz; one example exhibits triple trephining.

The next paper, by Mr Cosmos Mindeleff, deals with "The Cliff Ruins of Canyon de Chelly, Arizona." The extraordinary cliff dwellings and villages, in which the caves and rock shelters of the canyon have been supplemented with masonry to a greater or less extent, are very fully described, and, although seemingly belonging to a specialised condition of culture, they are none the less, in the writer's opinion, to be regarded as, for the most part, "subordinate structures, connected with and inhabited at the same time as a number of larger home villages located on the canyon bottom." He urges that they served not so much for purposes of defence as for outlooks, whence the adjacent cultivable areas could be viewed. They present advanced methods of construction, and cannot therefore be regarded as primitive, and in many cases must be referred to comparatively recent times, within the historic period. First-rate reproductions of photographs and plans illustrate the paper.

The "Day Symbols of the Maya Year" by Cyrus Thomas follows, and the volume ends with a description by Dr Fewkes of the "Tusayan Snake Ceremonies." Since the publication of Captain J. G. Bourke's well-known book on the snake dances in 1884, many fresh localities, where these ceremonies obtain, have been discovered. The rituals throughout are closely related, but local divergences are observable, and a careful comparison of these will tend to throw light upon the origin of the ceremonial. The details of the Tusayan dances are very interesting, and the comparative method of treatment of the subject is very instructive, though the elucidation of the true significance and symbolism of the cult is one presenting great difficulties. H. B.

Serials

The March number of the *American Naturalist* is almost entirely devoted to an account of Louis Agassiz, who, in March 1848, began his course of zoological instruction at Harvard University. Articles are contributed on various aspects of the work of the great naturalist, as follows:—"The Philosophical Views of Agassiz," by A. S. Packard; "Agassiz and the Ice Age," by G. F. Wright; "Agassiz on Recent Fishes," by David Starr Jordan; "Agassiz's Work on Fossil Fishes," by C. R. Eastman; "Agassiz's Work on the Embryology of the Turtle," by Gertrude C. Davenport; "Agassiz at Penikese," by Burt G. Wilder. The *American Naturalist* was started by pupils of Louis Agassiz, and indeed most of the eminent zoologists of the United States were his pupils, so that it is only fitting that it should commemorate in this manner so important a jubilee. An excellent photograph of Louis Agassiz is reproduced as the frontispiece.

Nature for May 5 contains, as the thirty-first instalment of its well-known series, "Scientific Worthies," an account of Prof. Albert von Kolliker, by Mr W. F. R. Weldon, illustrated by an admirable portrait.

Further Literature Received

Students' Text-book of Zoology, Sedgwick: Sonnenschein. History of Fowling, Macpherson: Douglas, Edinburgh. Flora of Perthshire, White: Blackwood, Edinburgh. Garden-Making, Bailey: Macmillan, New York. De Danske Barkbiller, Lovendal: Schuboth, Copenhagen.

Contents-Subject Index, 7, 8, 9, 10, Cotgreave. Problems of Plant-Physiology, MacDougal: *Science.* Summer School of Biology on Illinois River, Forbes. Bulletin U.S. Dept. Agriculture, Entomology, N.S., Nos. 10, 12, and 13. Report Museums Assoc., 1897. Contribution to Theory of Warning Colours, Nos. 3 and 4, Finn: *Journ. Asiat. Soc. Bengal.* Notes on Introduction of Brown Hare into Ireland, Barrett Hamilton: *Irish Nat.* Ninth Ann. Rep. Missouri Botanic Garden. Catalogue of Land-Shells of America, N. of Mexico, Pilsbry & Johnson: *Nautilus.* Wing and Larval Characters of Emperor Moths, A. R. Grote: *Proc. S. London Entom. Soc.* Maryland Geol. Survey, vol. i. Second Ann. Rep. New York Zool. Soc. Reports on Water-Supply of Goldfields in Western Australia, Govt. Geologist. Trans. Oxford Univ. Jun. Sci. Club, N.S., Nos. 3 and 4. Report Rugby School Nat. Hist. Soc., 1897. Rep. Cheltenham Coll. Nat. Hist. Soc., 1897. Case of Protective Coloration in House Mouse, Jameson: *Linn. Soc. Journ. Zool.* Proc. Biol. Soc. Washington, vol. xii., pp. 85-129. Metric Equivalents of Imperial Weights and Measures: *Pharmaceut. Journ.* Mem. Soc. Cient. Antonio Alzate, vol. xi., Nos. 1-4. Habits and Economy of Birds, Finn; *Proc. Asiat. Soc. Bengal.*

Amer. Monthly Micr. Journ., April; Scot. Geogr. Mag., May; Scot. Med. and Surg. Journ., May; Amer. Journ. Sci., May; Amer. Nat., Mar.; Victorian Nat., Mar.; Annot. Zool. Japan, vol. ii., pt. i.; Botan. Gazette, April; Irish Nat., May; Westminster Rev., May; Journ. Essex Tech. Lab., vol. iii., Aug.-Dec., 1897; Journ. Marine Biol. Assoc., V., No. 2; Journ. School Geogr., April, May; Knowledge, May; Literary Digest, April 9, 16, 23, 30, May 7; Naturae Novit., Mar., Nos. 5, 6, April, No. 7; Naturalist, May; Nature, April 14, 21, 28, May 5, 12; Nature Notes, May; Naturen, Mar., April; New Age, Feb.; Plant World, Mar., April; Psychol. Rev., May; Review of Reviews, April; Rivista Quindicinale di Psichologia, April 1, 15, May 1; Revue Scient., April 16, 23, 30, May 7, 14; Science, April 8, 15, 22, 29, May 6; Scientific Amer., April 9, 16, 23, 30, May 7.

OBITUARIES

HENRY LEWIS

BORN AT WALWORTH, 1834. DIED AT WANDSWORTH, 10TH APRIL 1898.

ON Easter Day there died one whose name is perhaps but little familiar to our readers. Henry Lewis was what would be called an uneducated man, in a small way of business as a bootmaker. But with him business was subordinate to the pursuit of knowledge, his special delight being the collection of facts concerning pre-historic man. We are indebted to Mr W. J. Lewis Abbott, himself a well-known worker in the same field, for an enthusiastic appreciation of Lewis' work. Space forces us to be content with a few extracts.

"For several decades he has been tramping the country from Suffolk to Barton, obtaining thousands of interesting specimens, which throw a flood of light upon Bronze, Neolithic, Palaeolithic, and Plateau man, neglecting the more lucrative following of his trade, and denying himself almost the necessities of life. Without a word of help or encouragement he was content to plod along, working at every pit or opening likely to yield the objects of his quest For many years he was one of the best known collectors in any pit in the Thames Valley, and men got to like him so thoroughly for his straightforward earnestness, and his willingness to impart his little knowledge, that they would let him have implements on credit; and although months elapsed before they were paid for, notwithstanding his weekly visits, we have heard rough-handed, square-hearted labourers remark, 'Oh, he's all right, he'll pay when he gets some money.' Perhaps no man set more labourers to work to find implements than Lewis, and thus it is impossible to calculate the effect of his life-work. Upon hearing of Skertchley's finds at Botany Bay, and the discredit with which they were unjustifiably received, he set off to the scene of dispute, and there secured not only far better implements than Skertchley had found, but bulbed and worked flakes also. For some years we have directed him to numerous so-called preglacial gravels to hunt for striated bulbed flakes or implements, and the number of these he obtained will surprise most people when an account of them is published. Although he never wrote very much, his labours have been made known by Mr Arthur Evans in connection with the interesting bronze finds at Aylesford. We have seen him taking bands of working men round the British Museum galleries. Bloomsbury knew him well, but Sir Augustus Franks, despite his unparalleled generosity and deep learning, cannot be said to have been unduly prejudiced in favour of older pre-historic man. Twice during his struggles Lewis was visited by fire, which practically made a total wreck of him, and his implements had to be sacrificed to meet claims. From the effects of the last disaster he never recovered, and his business subsequently became of restricted dimensions.

"At last with the creeping on of old age and the effects of continued saturations and exposures, to which real field-workers are subject but from which arm-chair critics and moneyed collectors fortunately escape, his throat and chest grew gradually worse. His last effort was to try and write to Mr Benjamin Harrison, giving a new argument in favour of Plateau Man."

KARL LUDWIG FRIDOLIN VON SANDBERGER

BORN 22ND NOV. 1826 AT DILLENBURG, NASSAU. DIED AT WÜRZBURG, APRIL 11TH, 1898.

IT is over half a century since this veteran geologist commenced author. He is perhaps best known for the work written in collaboration with his brother Guido, "Die Versteinerungen des rheinischen Schichten-systems in Nassau," which was published during the years 1850-56, and has ever remained the classical account of the geology and palaeontology of that region. In consideration of this work the authors received the Wollaston Fund from the Geological Society of London in 1855. The next important work on which Dr Sandberger engaged was an account of the shells of the Tertiary Basin of Mayence (1863). The studies made in connection with this gave rise to a general account of fossil land and fresh-water shells, which was issued in two volumes, 1870-75. Dr Sandberger studied many other fossil Mollusca, as well as the structure of the Brachiopoda. In later years, however, he turned his attention chiefly to mineralogy, and in 1882-85 published his "Researches on Mineral Veins." He also made many contributions to the study of the microscopic structure of eruptive rocks. In 1849 Fridolin Sandberger was made curator of the Natural History Museum of Nassau, and in 1854 went to the Karlsruhe Polytechnic as Professor of Mineralogy and Geology. Here he stayed till 1863, when he was called to Würzburg to fill the chair of Mineralogy, to which Geology was then added for the first time. This post, which carried with it the direction of the Mineralogisches Institut, he held till a short time before his death. The Geological Society of London elected him a foreign member in 1875, and in the following year he received the Cothenius gold medal from the Leopold-Caroline Academy.

JULES MARCOU

BORN AT SALINS, FRENCH JURA, 20TH APRIL 1824. DIED AT CAMBRIDGE, MASS., 17TH APRIL 1898.

WHAT interesting reminiscences Jules Marcou's would have been had he published them! A citizen of two hemispheres, writing with equal facility and spirit in French and English, the pupil of Thurmann, the friend of Agassiz, the explorer of the West, a historian of ancient maps and a maker of new ones, a lover of truth at all hazards, a hater of humbug, and a thorough-going fighter. Perhaps he has left a manuscript somewhere. At any rate, he did the next best thing in producing a life of Louis Agassiz, which both before and after publication stirred up a good deal of controversy. Above all things

Marcou was a geologist of the Jura, and an authority on the fossils found near his first home. In 1849 his "Recherches géologiques sur le Jura salinois" were published by the Geological Society of France, and in the same year he was appointed 'préparateur' in mineralogy at the Sorbonne, while in 1847 he was entrusted with the arrangement of the palaeontological collections in the Museum. In the following year he was enabled to go to North America as travelling geologist from the Paris Museum, and visited Agassiz, who had just begun his work at Boston. With him he went to the L. Superior region; after which he studied the geology of New Jersey, Pennsylvania, Virginia, and the Mammoth Cave. In June 1850 he returned to France, and prepared the first general geological map of the United States, published in 1853. In that year he went back to America, having been appointed geologist to the government expedition, which, under Lieut. A. Whipple, explored the 35th parallel from the Mississippi to the Pacific, for the purpose of a railroad. Thus he made the first discovery of Jurassic fossils in America. Illness forced him to return to Europe, and interfered with the preparation of his report. In 1855 he was appointed Professor of Geology at the Zurich Polytechnic, where he stayed till 1860, when he again returned to America and helped Agassiz in founding the Museum of Comparative Zoology. In 1862 was issued his geological map of the world, of which a new edition appeared in 1875.

To allude in detail to Marcou's numerous writings would be a lengthy task. It should not be forgotten that he proposed the name Dyas for the rocks usually called Permian, and, what is more, contributed to our knowledge of them in the Old and New Worlds. Among his controversial writings, one recalls his vigorous polemics on the Taconic and Jurassic rocks of N. America, and his attacks on the United States Geological Survey, which were not without effect. He always displayed deep interest in the history of the discovery of America, and had finished the manuscript of a fresh paper on the subject shortly before his death. In this work he was more than once assisted by his son, Mr John Belknap Marcou.

Among other losses to science we note the following: On March 24, aged 64, ALFRED U. ALLEN of Bath, who was the secretary of the Postal Microscopical Society, and editor since 1882 of the *Journal of Microscopy and Natural Science*, the cessation of which we noticed a short time ago; Lieut. BRASSEUR, the Congo State traveller, in a fight with the Arabs on the banks of the Luapula; Dr MAX DAHMEN, the bacteriologist, at Crefeld; on April 12, aged 67, Prof. AIMÉ GIRARD of the Conservatoire des Arts et Métiers, Paris, and member of the section of Rural Economy of the Paris Academy of Sciences, a leading authority on vegetable fibres, wheat, sugars, and woods; Dr SAMUEL GORDON, president of the Royal Academy of Medicine in Ireland, and successor to the late Dr Haughton as president of the Royal Zoological Society, Dublin; on March 26, aged 26, BRADNEY B. GRIFFIN of Columbia University, author of papers on the fertilisation of the egg in *Thalassema*, the nemerteans of Puget Sound, and other subjects; at Elmina, West Africa, on April 19, aged 32, Dr JOHN SHEARSON HYLAND, F.G.S., for some time on the staff of the Geological Survey of Ireland as a petrologist, but latterly engaged in reporting on mineral resources in the United States and Africa; EDWARD KOKOSINSKI, the bacteriologist, at Lisle, on February 26th; Dr GIUSEPPE PALMA, assistant in Zoology at Naples University on January 18th; HERMANN PUTZ, Honorary Professor of Veterinary Science at Halle on March 4th, aged 68.

NEWS

Mr Alexander Agassiz, Director of the Museum of Comparative Zoology, has been appointed Professor Emeritus of Harvard College by the President and Fellows in concurrence with the Board of Overseers.

Among other appointments we note: Dr Franz Steindachner to succeed Dr Franz v. Hauer as head of the Naturhistorische Hofmuseum in Vienna, himself being succeeded in the professorship of zoology by Friedrich Mor. Brauer; Dr Giovanni Battista Condorelli, as professor of natural history at the Gaeta Technical School; Dr Karl Chun of Breslau to succeed the late R. Leuckart, as professor of zoology at Leipzig; Dr Bela Haller, of Heidelberg, to be professor of zoology there; Pierre Fauvel of Caen, as professor of zoology at the Angers University; E. S. Goodrich to be Tomlinson-Aldrichian Demonstrator of Anatomy at Oxford University; Dr Alessandro Coggi of Bologne, as professor of zoology, anatomy, and physiology at the Perugia University; Harold Heath of Pennsylvania University, to be assistant professor of zoology; Dr Karl Hischeler, assistant in the Zoological Laboratory of Zurich, to be privat-docent; Dr Henry T. Fernald, to be economic zoologist of the State of Pennsylvania; Dr G. J. Born of Breslau, to be full professor of anatomy in that university; Dr A. L. Bolk, to be professor of anatomy at Amsterdam University; Mr W. J. Gies, as instructor in physiology at Yale; and Messrs A. H. Redland and H. E. M'Dermott, as assistants in the same office there; Dr Glaister, to be professor of forensic medicine in Glasgow University, in succession to Dr P. A. Simpson; Dr W. J. Simpson, late Health Officer of Calcutta, to be professor of hygiene in King's College, London.

Dr Otto Warburg, to be titular professor of botany at Berlin University; Dr E. Zacharias, to be director of the Botanical Garden at Hamburg; Alfred J. M'Clatchie, of the Throop Polytechnic Institute, Pasadena, Cal., to be professor of agriculture and horticulture in the University of Arizona at Phœnix; Prof. F. W. Card, of Nebraska University, to be professor of agriculture in Rhode Island University; Cornelius L. Shear, to be assistant agrostologist in the U. S. Department of Agriculture; Dr Jenvresse, as professor of technical and agricultural chemistry at Besançon University.

Dr E. Kalkowsky, professor of mineralogy and geology at the Technical College, Dresden, to be director of the Geological Museum and Prehistoric Collection at the Zwinger Palace, in place of Dr H. B. Geinitz, who retires after fifty-one years of service; Dr G. Adolf. Sauer, as associate professor of mineralogy at Heidelberg; Dr Emil Bose, of Karlsruhe, as state-geologist to the Republic of Mexico; Dr Domenico Sangiorgi, as assistant in the Geological and Mineralogical Cabinet of Parma University. In the Geological Survey of the United Kingdom, Dr Wm. Pollard to be assistant geologist in the Petrographic Dept. at Jermyn St., and H. J. Seymour to the corresponding post in Dublin; C. B. Wedd to be assistant geologist, filling the vacancy due to the resignation of C. E. De Rance.

Mr J. H. Cooke of the Education Department, who has been doing such valuable work in Lincolnshire in the promotion of natural science, especially in connection with the Lincolnshire Science Society, has been transferred to the Worcester district, where no doubt he will find an equally fertile field for his energies.

Dr C. Wardell Stiles has been appointed *attaché* to the United States Embassy in Berlin, in order that he may keep the Agricultural Department informed concerning advances in agricultural science, protect the agricultural imports from America, and inquire into the food-products exported from Germany to the United States. He passed through England a few weeks ago, and will return for the Zoological Congress.

DR FRANZ V. HAUER receives a pension from the Vienna Museum, and Sir George King, late custodian of the Sibpur Botanical Gardens, also has been pensioned on his retirement.

MR J. H. TEALL, of the Geological Survey, has been elected a member of the Athenaeum Club, under the rule which empowers the annual election of nine persons "of distinguished eminence in science, literature, the arts, or for public services."

THE 25th of March last was the fiftieth birthday of Dr W. K. Brooks, Professor of Zoology at Johns Hopkins University. His former students and other zoologists took the opportunity of presenting him with a portrait of himself by Mr T. C. Corner.

Science can only suppose that it is a consequence of Tammany Government that Dr Tarleton H. Bean, the well-known Director of the New York Aquarium, has been asked by the President of the Park Board to resign the office which he has held with universal approval.

OXFORD UNIVERSITY is to expend a sum not exceeding £7500 in removing and reconstructing the iron laboratory at the University Museum, at present occupied by the Linacre Professor of Comparative Anatomy, and in erecting on or near to the site of that laboratory a new laboratory and lecture-room for the joint use of the Professor of Botany and the Professor of Comparative Anatomy.

THE University of Paris contemplates borrowing money in order to build laboratories for elementary instruction in Physical and Natural Science, also to complete its laboratory of Vegetable Biology situated at Fontainebleau.

THE botanical department of the University of Pennsylvania has, says *Science*, received a gift of a collection of dried plants and seeds from the Biltmore estate, and specimens of fungi from Dr J. T. Rothrock.

THE University of Illinois has decided to organise this season a summer school of field and laboratory biology in connection with the third summer opening of the Biological Station on the Illinois River at Havana. Four regular courses will be offered to students, two in zoology and two in botany; in addition to these, opportunity will be given to students of experience to take independent work on special subjects, and to visiting investigators to pursue their personal researches at the station with the use of its equipment.

A COLLEGE of Forestry, says *Science*, has been established at Cornell University with an initial endowment of $10,000. The Trustees of the University are authorised to purchase not more than 30,000 acres in the State Park in the Adirondacks for the proposed College. There will be a professor, two instructors, a forest manager, and several subordinates. The Director is to be Prof. B. E. Fernow of the U.S. Forestry Division.

THE Botanical Survey of Nebraska, which is worked by the University of that State, has now been in progress for six years, although much work in that direction had been done before. The results were summarised by Prof. C. E. Bessey at the last meeting of the American Association. There is a herbarium of about 10,000 specimens, specially intended to illustrate plant distribution. Five reports have been issued, and a comprehensive work, the "Flora of Nebraska," of which Parts I. and II. appeared in August 1894, is in course of publication. The various botanical regions and districts of the State have already been mapped with some accuracy, and another map giving their physical features is almost ready for publication.

MRS PHOEBE HEARST has offered to construct and equip at her own expense a building for the College of Mines at the University of California.

THE Select Committee appointed by the House of Commons to inquire into the administration and cost of the Museums of the Science and Art Department, have issued an interim report regarding the South Kensington Museum, and the Geological Museum in Jermyn Street. "They are unanimously of opinion that, with a view to present efficient management, to economy of administration, to future development of the collections, and to their full use for the purpose of exhibition and of instruction, it is necessary—(1) That the whole area on the east side of Exhibition Road (except that occupied by the Royal College of Science, which cannot be sacrificed except at great cost) be exclusively devoted to the Art Museum and the Art Library, with provision for the conduct of the business connected with loans of art objects, and the art schools. They are satisfied that the whole of this space is required for the art schools, the due exhibition of the art collections, and the administration connected with such a museum. (2) That provision for the whole of the science collection, the science library, for loans of scientific objects, and for the science schools, be made on the west side of the Exhibition Road. They are convinced that this concentration of art on one side of the road, and of science on the other, is essential to good administration, to satisfactory results from the money expended, and efficiency both in the museum and in the schools. This arrangement would allow space for the future development both of the art and of the science branches. They also unanimously recommend that the Geological Museum in Jermyn Street be no longer occupied for the same purposes as now; and that the collections there exhibited be removed to the west side of Exhibition Road, and made part of the science collections."

THE Historical Museum constructed by the Swiss Confederacy at Zurich, after many delays, will be finally inaugurated in June. The event will be celebrated by festivities for which liberal appropriations have been made. Particularly imposing are the plans for the historical procession organised by the Zurich guilds. The new museum contains among other treasures the collections from the Lake Dwellings, formerly scattered in private museums.

WE have received the Year-Book of the Bergen Museum for 1897. It contains, as usual, a number of interesting articles, to many of which we refer in our Notes and Comments. Whatever may be the case in English museums, it is the case that visitors to the Bergen Museum are most numerous on Sundays. The numbers for 1897 are as follows: — Sundays, 35,566; free week-days, 7438; pay-days, 1248; schools, and by ticket, 2582—which leaves a majority of over 24,000 in favour of Sundays. The additions to the collection were larger during 1897 than any previous year. The whole collection made by the Norwegian Society for National Ethnography, begun in 1892, was handed over to the Bergen Museum in December 1897. A large collection of German minerals was presented by Mr C. Sundt. Consul Bors presented over 500 specimens of some 358 species of *Lepidoptera* from North, Central, and South America. These collections cannot be displayed until the new buildings are finished, which should have been accomplished by the end of 1897. Mr E. Ingebrektsen, a missionary, has presented 82 specimens of South American Vertebrata. During August a course of lectures was given in the Museum, especially intended for teachers in the Board Schools.

THE Biological Station at Bergen has continued its useful investigations into the fisheries and the general biology of the sea. Dr Appellof has made researches into the development of the lobster and salmon, from both of which practical results of much value are expected, and pecuniary help has been given by the Storthing. Mr Nordgaard has taken part in expeditions to Lofoten, and results of much importance have been obtained. The work in marine biology that has long been carried out at Bergen is so valuable from both a scientific and a practical point of view that we sympathise strongly with the fear expressed in this

Report lest the establishment of a Biological Station in Christiania, as decreed by the Storthing in 1897, should draw away from Bergen both workers and helpers. By its position and its Museum, Bergen seems to be the natural headquarters of fishery investigations, and in these matters centralisation and co-operation are better than the splitting up of forces.

THE Trustees of the Philadelphia Museums are about, says *Science*, to consider the question of establishing branch museums in the principal cities of the Union.

THE following are the candidates recommended by the Council of the Royal Society for election this year:—H. F. Baker, M.A. Cantab., mathematician; E. W. Brown, M.A. Cantab., mathematical astronomer; Dr A. Buchan, meteorologist; S. F. Harmer, M.A. Cantab., zoologist, specially known for his writings on Polyzoa; A. Lister, distinguished for his researches on Mycetozoa; Lieut.-General C. A. McMahon, a leading authority on Himalayan geology and in petrology; W. Osler, Professor of Medicine at John Hopkin's University, and a foremost representative of clinical medicine and pathology; Hon. C. A. Parsons, M.A. Cantab., engineer; T. Preston, M.A., Dublin, physicist; E. W. Reid, M.B. Cantab., Professor of Physiology at Dundee; A. Scott, M.A. Cantab., chemist; A. C. Seward, M.A. Cantab., a leading palaeobotanist; W. A. Shenstone, chemist; H. M. Taylor, M.A. Cantab., mathematician; J. Wimshurst, electrician

THE Reception Committee for the Fourth International Congress of Zoology to be held in Cambridge during August announces that the Reception Room will be at the Masonic Hall, Corn Exchange Street, Cambridge, and that it will be open from 9 A.M. on Monday, August 22, for the issue of Cards of Membership, for which the subscription is £1. If paid before that date it should be sent to the Treasurers, Zoological Society of London, 3 Hanover Square, London, W. The congress will open formally at 10 A.M. on Tuesday, August 23, and there will be an informal reception at the Guildhall on the evening of Monday, August 22. On the termination of the official business of the Congress about noon on Saturday, August 27, members are invited to adjourn to London in order to take advantage of arrangements which are being made for their entertainment by the Executive Committee in London. All correspondence relating to the business of the Reception Committee should be addressed to the Secretaries (S. F. Harmer, and A. E. Shipley), The Museums, Cambridge.

THE Museums Association is to hold its ninth annual meeting in Sheffield during the first week in July, beginning on the 4th. The president-elect is Alderman W. H. Brittain, of Sheffield, who hitherto has been the popular treasurer of the Association, and will doubtless give a racy address.

THE American Association for the Advancement of Science will hold its fiftieth anniversary meeting from August 22nd to the 27th at Boston, in which city its first meeting was held. The attendance of foreign scientific men is specially hoped for. An attempt is being made to induce all who have ever been members of the Association to resume their membership, at all events for this meeting.

THE tenth Congress of Russian Naturalists and Physicians will be held at Kieff from the 21st to 30th August, under the presidency of Prof. Rachmaninov.

THE ninth International Congress of Hygiene and Demography, which met at Madrid, April 10-17, under the presidency of Dr Julian Calleja, was attended by 2000 members.

MR F. E. BEDDARD, Prosector of the Zoological Society, is delivering a course of lectures on amphibians and reptiles, in the lecture room at the Gardens in Regent's Park. The lectures are on Fridays at 5 P.M., and began on April 21st.

THE gold medal of the Linnean Society has been awarded to Mr G. C. Wallich, whose investigations into deep-sea life are well known. It is nearly forty years since he accompanied Sir Leopold McClintock on the survey of the bed of the North Atlantic for the laying of the proposed Atlantic cable. His observations on H.M.S. 'Bulldog' led him to maintain the presence of life at depths where many disbelieved in its existence, and he was largely responsible for the view that the bottom of the Atlantic was still in the Chalk period. His book "The North Atlantic Sea-bed" (1862) was the first to discuss fully and systematically, and in the main correctly, the various questions bearing on the biological relations of the ocean floor.

THE Royal Geographical Society has awarded its Royal Medals to Dr Sven Hedin and Lieut. A. E. Peary; the Murchison Grant to Mr H. Warington Smyth, whose book on Siam is finding such favour; the Back Grant to Mr G. P. Tate for survey work in Afghanistan, Baluchistan, at Aden, and on the Indus; the Gill Memorial to Mr E. J. Garwood for work in connection with the Conway expeditions to Spitzbergen; and the Cuthbert Peek Grant to Mr Poulett Weatherley for exploring the region between Lakes Mweru and Bangweolo. The Society has elected as honorary corresponding members: Don Marcos Jimenes de la Espada, Dr F. Moreno of Buenos Ayres, the Marquis of Rio Branco, Brazil, Dr Thoroddsen the Icelandic geologist, and Prof. Ratzel of Leipzig.

THE Dublin Microscopical Club has hitherto met at the private houses of its members. It has obtained permission to meet in future at the rooms of the Royal Dublin Society.

DURING 1897, 6000 more people visited the Gardens of the Dublin Zoological Society than in 1896. This, however, does not prevent the Society being in debt to over £400. Under these circumstances it is sad that a deputation to the Treasury in February 1897, to solicit a government grant towards the erection of necessary new buildings, proved unsuccessful. The new Aquarium House, however, was opened in March, and the reptiles and diving birds contained in it have proved a great attraction. The new Camel and Llama House has also been finished, and the health of the animals improved in consequence. Of two litters of lions all the cubs died shortly after birth. Two female cubs have been imported from Somaliland, as well as a fine young lioness.

THE Hull Geological Society has issued a list of forthcoming excursions. That on Whit-Monday is to the Bunter Sandstone and Glacial Deposits of Balby and Sandal, and is in connection with the Yorkshire Naturalists' Union. On June 11th evidences of post-glacial denudation and the present drainage system are to be studied in Central Holderness. Lincoln is to be visited on June 25th. The president of this Society is F. F. Walton, and the secretary, J. W. Stather, 16 Louis Street, Hull.

FROM the Haslemere Microscope and Natural History Society we have received the Report for the year ending 31st May 1897, as well as a record of lectures and addresses delivered before the Society during that year. To the latter Mr Grant Allen, who is president, contributes an introductory note. Mr Allen finds it necessary to regret that the Society consists largely of hearers rather than of workers, and in a letter to the secretary he says with some justice, "A Natural History and Microscopical Society ought to be composed of naturalists and microscopists. We ought to urge upon the individual members the desirability of taking up some one branch of Natural History." Since the Society numbers no less than 452 members and has an annual income of over £70 it should certainly be possible for it to turn out a little actual work. It should also be remembered that this Society shares the benefit of Mr Jonathan Hutchinson's admirable Educational Museum.

THERE has been founded a New England Botanical Club, which meets on the first Friday of each month, and has its herbarium in the Museum of Harvard University. The president is G. L. Goodale, and the corresponding secretary, E. L. Rand, 740 Exchange Building, Boston.

THE Philadelphia Academy of Natural Sciences has, says *Science*, recently received a collection of lichens from Dr J. W. Eckfeldt; of Jamaican fossil molluscs from Mr S. Schumo; and of Honduras Lepidoptera from Dr H. Griffith.

THIS summer a joint expedition of the West Siberian branch of the Geographical Society, and the Moscow Society of Amateurs of Natural Sciences will, says *Nature*, explore the hydrography and the fauna of the lakes in the south of Omsk. The collections will be divided between the two societies.

THE Geological Survey of Maryland has made such a good start that the Legislature of the State has not only appropriated the regular $20,000, but also an additional $10,000 for topography and $20,000 for the study of road-construction. The whole is under the direction of Prof. W. B. Clark of Johns Hopkins University.

THE Report of the Director of the Marine Biological Laboratory at Plymouth is a record of varied and useful work. To the Drift-Bottle experiment we allude in our Notes and Comments. Mr Garstang has also been occupied in investigations relating to the migratory pelagic fishes, especially mackerel. Mr Holt has made many observations on the eggs and larvae of fishes, and has studied the distribution of fish at different ages in the neighbourhood of Plymouth. Mr E. T. Browne, who has been working on medusae in the laboratory, has, in conjunction with Mr Allen, devised a useful apparatus for keeping those and other pelagic organisms alive in confinement. The apparatus is fully described in the April number of the *Journal*. We may remind our readers that any contributions from them to the library of this institution will always be welcome. No doubt they have already sent their pecuniary donations and subscriptions.

WE are glad to learn that, under its new curator, Mr Alexander Gray, the Robertson Museum at the Millport Marine Biological Station continues to prove of service to naturalists and of interest to the public. Dr Gemmill, lecturer on embryology, and Dr Rankin, demonstrator in zoology in Glasgow University, took several of their students to Millport during the Easter vacation; and it is expected that many students from Glasgow University, as well as those attending other science classes in the neighbourhood, will avail themselves during the coming season of the advantages offered by this institution for gaining a practical knowledge of the subject of their studies not otherwise attainable.

THE accommodation for students afforded by the Port Erin Biological Station has recently been extended by the erection of a floor beneath the open roof of the laboratory, on which there are five well-lighted work benches. The aquarium is now in a flourishing condition, the tanks having been thoroughly cleansed and replenished with local organisms early in the year. A small party of zoologists, including Prof. Herdman, Mr I. C. Thompson, Mr F. J. Cole, and Mr A. O. Walker, spent the Easter vacation at the Station in pursuit of their special lines of research. Unfavourable weather somewhat marred the success of two dredging excursions taken in the Lancashire Sea Fisheries Committee's steamer 'John Fell,' but shore-collecting and tow-netting in the bay provided all with abundant material for profitable work. Segmenting ova and larvae of *Alcyonium digitatum* in various stages of development were taken in the tow-net. Developing eggs of various food-fishes were numerous.

THE city of Geneva has received as a legacy from Philippe Plantamour the sum of 300,000 francs and his estate of Sécheron. The latter will be used as a botanic garden. A botanic garden is being founded at Nantes. In it special attention will be paid to plants useful in the colonies of France.

DR NICOLE has opened the Bacteriological Institution in Constantinople.

THE German Deep-Sea Expedition, already announced by us, has received a grant of £15,000 from the Reichstag. It is to sail in August, and begin work between Scotland and the Shetlands. It will then pass southwards by the

Canaries and Cape Verde Islands to the coast of West Africa, where it will specially study the cold Benguella current. The Antarctic currents and their union with the warm currents from the Indian Ocean will be investigated, after which the Expedition will return home by way of the Indian Ocean and the Red Sea. The whole voyage is expected to take nine months.

PRINCE ALBERT of Monaco, who is known to scientific readers from the valuable investigations that he has made in the Atlantic, gave an interesting lecture before the Royal Geographical Society on April 25th. His first vessel, in which he made many cruises between 1885 and 1889, was a schooner of 200 tons called the 'Hirondelle.' She was succeeded by a steam yacht, the 'Princess Alice,' of 560 tons. She, however, has been outgrown with the extension of the work, and now a new 'Princess Alice' has been built by Messrs Laird of Liverpool. Much of the work accomplished by Prince Albert has been in connection with ocean currents, and is alluded to in Mr Garstang's Report on the Drift of the English Channel. Two meteorological stations have been established at the Azores—one on the Island San Miguel, connected with the mainland by cable, and one, 100 miles further west, on the Island of Flores, from which a cable to America is planned. This, says *Science*, has been done by Captain Chares, a Portuguese, at the instance of the Prince of Monaco, and it is expected that the observations will be of value, especially with regard to cyclones.

THE 'Windward,' Captain John Bartlett, will sail from New York with the Peary Arctic expedition about the first week in July.

DR O. F. COOK, Mr Charles Louis Pollard, and Mr Guy W. Collins, of the U.S. National Museum, with Prof. E. L. Morris, of the Western High School, D.C., left on March 5th for a six weeks' botanical collecting trip among the Florida Keys. They were going first to Key West, where they would secure a small schooner and then visit the various Keys, and expected to reach Miami about April 5th. Dr Cook was commissioned to make a collection of Algae, which was to be sent to the Omaha Exposition. The other members of the party were to make a general collection, including herbarium and various economic material, and were also commissioned to obtain museum material for the New York Botanical Garden. We glean this information from the *Plant World*, which adds somewhat curiously that "this is a favourable season in which to visit Southern Florida." Most peace-loving citizens would avoid it at this juncture.

MR B. E. FERNOW, of the U.S. Forestry Division, has been sent by the Government to Hawaii to make preliminary explorations with a view to future Forestry Legislation.

MR J. B. HATCHER, whose expedition to Patagonia we have already noted, has sent to Princeton University various collections, including one of fossil shells from the Straits of Magellan. He has now, says *Science*, gone for an eight months' trip into the interior.

DR C. MARCHESETTI has gone on a botanical expedition to Palestine and Upper Egypt. Dr M. Pedersen (of Copenhagen) is investigating the flora of Disco Island, Greenland.

ON April 14th a Belgian expedition left to make a scientific exploration of the African territories south of the Congo Free State, including the Shiré country, Lake Nyassa, the Zambesi, Tanganyika, and the country of Lofoi. Among those taking part are Lieut. Lemaire, Dr J. de Windt, Captain Maffei, Mr Michel as photographer, and Mr Leon Dardenne as artist.

DR HANS BENDORFF of Vienna has gone to Siberia on behalf of the Vienna Academy to collect information on atmospheric electricity.

WE are glad to learn from *Nature Notes* that considerable progress has been made on the Continent in the protection of birds. An Austrian League was established in 1896, with its headquarters at Gratz, under the presidency of

Anna, Countess Buttler. This League has published a much-needed "Appeal to Ladies." A similar League has spread widely in Germany. In 1895 there were in Finland 11,000 ladies pledged to wear neither the feathers nor the bodies of birds in their hats and bonnets. An International Congress on the Protection of Wild Animals and Birds is to be held at Gratz from August 5th to 9th. The May number of *Nature Notes* contains a sonnet by Canon H. D. Rawnsley on Watts' picture, "The Altar of Fashion." The Field Naturalists' Club of Victoria is attempting to induce the Tasmanian Government to have the albatrosses on Albatross Island protected by law.

THE International Ornithological Exhibition, which was to have been held this year at St Petersburg, has assumed dimensions so far beyond the scope originally intended that the Russian authorities have decided to postpone it until April 1899.

WE learn from *Nature* that the establishment of the National Zoological Park, Washington, has led to the formation of many other zoological preserves in the United States. In the western part of New Hampshire is an area of 26,000 acres, established by the late Austin Corbin, and containing 74 bison, 200 moose, 1500 elk, 1700 deer of different species, and 150 wild boar, all of which are rapidly multiplying. In the Adirondacks, a preserve of 9000 acres has been stocked with elk, Virginia deer, mule deer, rabbits and pheasants. The same animals are preserved by W. C. Whitney on an estate of 1000 acres in the Berkshire Hills, near Lenox, Mass., where also he keeps bison and antelope. Other preserves are Nehasane Park, in the Adirondacks, 8000 acres; Tranquillity Park, near Allamuchy, N.J., 1000 acres; the Alling preserve, near Tacoma, Washington, 5000 acres; North Lodge, near St Paul, Minn., 400 acres; and Furlough Lodge, in the Catskills, N.Y., 600 acres.

THE executors of the late Baron Sir Ferdinand von Mueller, Government Botanist of Victoria, are collecting donations for the erection upon his grave in the St Kilda cemetery, Melbourne, of a monument of grey granite, 23 feet in height. It will stand in the centre of a grave-plot 12 feet square, planted with choice specimens of the Australian flora. The supplemental volume of the 'Flora Australiensis,' upon which Baron von Mueller had worked for years, was nearly ready for press at the time of his death, and is to be published together with two volumes giving a biography, an account of his administration to the Botanical Gardens, and a complete bibliography of his writings. The executors will feel favoured by the loan of any of his letters, or the communication of incidents in the Baron's life which friends may deem worthy of notice in the biography. Donations and letters should be addressed, "Rev. W. Potter, 'Vonmueller,' Arnold Street, South Yarra, Melbourne, Australia."

THE London County Council has decided to lay out plots of ground at Battersea Park, Ravenscourt Park and Finsbury Park, for the cultivation of certain typical plants suitable for the instruction in practical botany of scholars at elementary and secondary schools. Each specimen is to be labelled with its common name and its systematic name, and is to be visible from the foot-path. Spare specimens will be supplied for botanical study in the schools.

RUSSIAN schools, we learn from *Nature*, are beginning to send out their pupils in summer for small natural history and ethnographic excursions, during which the systematic exploration of some region is attempted. The Caucasus school administration is especially active in that direction. One such excursion will be made to the foot of Elbrus this summer by fifty pupils of the Ekaterinodar Gymnasium. The party intends to visit the Great Karachai region, to ascend Elbrus up to the snow-line, and to cross the main ridge. The excursion will last fifty days, during which the pupils will collect natural history specimens and ethnographical data, take photographs, sketch landscapes, and live amidst the beautiful pine-forests of the Caucasus.

CORRESPONDENCE

LLHUYD

YOUR interesting notice of Llhuyd does him no more than justice; it is indeed strange that Prof. Sollas should have forgotten to mention him in his inaugural lecture. Such was not the treatment Llhuyd received at the hands of Edward Forbes, who dedicated to him the starfish genus *Luidia*, and thus wrote: "He was a man of great knowledge and great talent. His studies were extended over large tracts of science and literature, and he enlightened both with his researches and his writings. He united a comprehensive and philosophical mind with an observing eye and the energy to execute. Amid the multiplicity of his studies there was no confusion. He wrote on insects, plants, fossils, antiquities, and languages; on all much and well. Ray praised him. Strange to say his name is omitted in many of our cyclopedias, which devote whole pages to men of less repute." W. B. Carpenter, also, in his "Researches on Antedon," spoke of Llhuyd as a "naturalist who deserves more honour than he has gained." His "Lithophylacii Britannici Ichnographia" is "a work which, the more it is examined, leaves a stronger and yet stronger impression of the industry and sagacity of its author. To elucidate the nature of Fossils by the comparison of their forms with those of existing Animals and Plants—familiar as the principle now seems to us—had not been systematically attempted by any previous Naturalist; and no one who may bestow a little attention on the contents of the 'Lithophylacium' can fail to perceive that it is something much more valuable than a mere collector's catalogue, and deals with questions far more important than those of nomenclature." The addition of my own opinion to those of these eminent naturalists would be an impertinence; but, since my studies have recently led me to peruse the writings of Llhuyd, as well as of his predecessors and contemporaries, I may be permitted to confirm the statements of W. B. Carpenter, with reference to Llhuyd's work on Crinoidea. Not only did he place all fossils showing what we now recognise as echinodermal structure in his class *Crustacea punctulata*, but he distinguished *Antedon* as the particular sea-star to which the stalked Crinoidea were most nearly related. Rosinus, to whom the merit of recognising the animal nature of fossil crinoids has often been ascribed, published his views sixteen years later, and compared the fossils not with *Antedon* but with *Euryale*, which is an ophiurid.

F. A. BATHER.

MR TAYLER ON EVOLUTION

THE article in the April number, p. 231, on 'The Study of Variations' must find a responsive echo in the minds of all who, in these later days, have struggled strenuously to contemplate the facts of nature in a true scientific spirit. Mr Tayler opens his remarks by asserting that the tendency in all branches of science is just now to neglect all purely theoretical conceptions, yet at the end of his paper he very properly fails to see the use of continuing the discussion anent the various theories of evolution as it now stands. He does not supply any hint as to the 'what next' theory that ought to be discussed in its place. After quoting a passage from Prof. Poulton, which reads like one from a light and early essay of Macaulay or De Quincey, and in which, as might be expected, the poetical imagination is hopelessly confounded with the scientific intellect, Mr Tayler states that "this passage appears to me to be singularly applicable to evolutionists of the present day," meaning, I suppose, that "the strictest self-criticism and the soundest judgment' have not been exercised anent the problems for and against use-inheritance, etc. Now, it would be exceedingly interesting to be informed as to the ways and means whereby any sort of scientific judgment can be pronounced, other than those directly deduced from or dependent on experiment. I might, for instance, theorise that the elements are really one, that they have been evolved from a primordial substance, a single principle, etc.; but if, like Stas, I tried to prove the hypothesis experimentally, I should certainly fail. My theory would be very nice, and if I were a poet, I should cling fondly to it, or, like a certain F.C.S. with plenty of cash, I might, in furtherance thereof and having denounced Stas's experiments, offer a prize of £100 to anybody who could show a spectrum of thallium having only one green line, etc. If such be the mode whereby questions of physics and chemistry admitting of ocular demonstration can only be settled, where does 'the strictest self-criticism,' etc., come in in matters of biology, which admit of no such precision and exactitude, or anything like them? Given the correct and proper scientific idea, and your self-criticism, etc., will be found to take care of itself.

All this, however, is somewhat beside the present question. The fact that equally plausible contrary positions may be taken up from the facts adduced by either side of the counter-theories of evolution is a tolerably certain proof or presumption that the real ultimate question at issue is not a scientific question at all, and cannot be settled by science. It may possibly be proved, for instance, and I believe it has been scientifically proved, that variations by direct adaptive modification are hereditary; but whether one species or definite type of organic life is transmutable into another by the ordinary forces of nature is a question which biological science, as such, has got nothing to do with. The unity of life, which underlies the main doctrine of evolution, may be a magnificent transcendental conception specially palatable to the poet and mystic; but to hold that many problems in modern medicine, etc., must remain more or less incomprehensible until the evolutionists cease from troubling and the weary wranglers are at rest, is rather too ridiculous.

Would it not be more decorous if British scientists declared once for all that theories of evolution are outside of their sphere? It has been held that the question of evolution can be settled only by biological observation and experiment. Just so; but why trouble about the matter at all? Why be enticed away from the true path of scientific investigation by phantom hypotheses regarding the unity and continuity of nature. The proper and fitting instruments and business of science are analysis, classification, and the detection of causes. The synthetic strain after unity and the absolute involved in evolution is distinctly hostile to the first, which carries with it also the second, viz., taxonomy; and of the four kinds of causes enumerated by Aristotle, the last, viz., the final cause, is also the last with which true science has to deal. It has been remarked that "final causes are the life which disturbs the prose of science"; and this, no doubt, is the reason why the poet and literate dabbling in scientific subjects turn first to teleology so very complacently, *e.g.*, the relations of flowers and insects. The poetic pantheism of evolution in general and the poetic life involved in final causes are the true and only mainstays of the terribly wasted and misguided energy of our pseudo-scientists. P. Q. KEEGAN.

PATTERDALE, WESTMORELAND.

MR HOYLE EXPLAINS

MAY I beg a few lines to explain two matters commented on in your last issue.

1. (p. 348) As regards the discrepancy between the introduction to and text of the paper on Lifu Mollusca, by Messrs Melville and Standen (*not* Standon), the former refers to the whole collection, the latter to the portions dealt with in parts 2 and 3 of the catalogue.

2. (p. 352) The "Supplement to the Catalogue of recent Cephalopoda" was *published* by the Royal Physical Society of Edinburgh in part 3 of vol. xiii. of its *Proceedings*, in December 1897. The Society was good enough to allow me to have a number of separate copies, which are not published, but a copy has been sent to every worker on the Cephalopoda, with whose name and address I was acquainted. The number of the volume should certainly have been placed on the cover, it was on the signature of the sheets, but the printer has removed it from the separate copies. WM. E. HOYLE.

May 11, 1898.

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