THURSDAY, OCTOBER 22, 1896.

THE BRITISH MUSEUM CATALOGUE OF CORALS.

Catalogue of the Madreporarian Corals in the British Museum (*Natural History*). Vol. II. *The Genus Turbinaria, the Genus Astræopora.* By Henry M. Bernard. 4to. Pp. 106; 33 plates. (London: 1896.)

THE first volume of the "Catalogue of the Madreporarian Corals in the British Museum" appeared in 1893; it consisted of a monograph of the very intricate genus *Madrepora*, and was the last work of the enthusiastic and talented George Brook. His lamented death threatened to seriously retard the production of the remaining volumes of the catalogue, for the Madreporaria are a peculiarly difficult group to classify, and as Dr. Brook had by that time obtained a considerable experience in the classification of corals, it was expected that he would have been able to bring out the succeeding volumes with a reasonable degree of celerity. Dr. Günther was fortunate enough at this juncture to secure the services of Mr. H. M. Bernard, who entered upon his laborious duties with characteristic energy and ability.

In the volume before us Mr. Bernard describes two genera which are allied to the one already catalogued. In the genus *Madrepora*, the free-swimming larva, on settling down, develops into an axial polyp, which gives off numerous tiers of daughter polyps, any one of which may become in its turn an axial polyp, giving off again numerous tiers of daughters, and so on.

In *Turbinaria* there is but one true axial polyp, which gives off only one ring of daughter polyps; these themselves bud, and their buds again bud, and so on. The daughter polyps may perhaps be considered as axial polyps which give off imperfect rings of buds, and these again parts of other rings of buds. Owing to this concentric budding in a single plane, a plate is formed, on the upper side of which the calicles alone occur. According to the angle at which the buds arise, so will the general expanse be cup-shaped or flattened; typically the corallum is salver-shaped, but owing to irregular growth the edges may become frilled, and eventually a complicated foliaceous mass, with more or less erect, irregularly fusing fronds may result. Sometimes the coralla form horizontal, dish-like growths, each fresh growth covers the previous one with a larger and thicker layer, there often being a space or fissure left between the two growths; or the corallum thickens enormously in the centre by the lengthening of the polyps, while the margin hangs down, each new growth creeping over the one which preceded it. In all cases the original axial polyp is submerged by subsequent growth. The downward streaming of the cœnenchyme not only thickens the stalk of the corallum, which may even be obliterated, but it tends to fill up the base of the hollow of the cup; this downward streaming leaves characteristic striations. Owing to the mode of growth, the Turbinarians are purely cœnenchymatous corals, the epitheca has dropped out of the skeleton except where it develops as a secondary epitheca on the under sides of fronds, especially where they tend to touch the surface upon which the cord is growing. The whole corallum is built up of the porous cœnenchymatous walls of the individual polyps, without any trace of epithecal envelopes, or of regular thecæ. Hence the term Athecalia, which has been proposed for such corals by Dr. Ortmann. Fifty-eight species of the genus *Turbinaria* are described by Mr. Bernard, of which some forty are new to science.

The only account we have of the structure of the soft parts of a Turbinarian is by Dr. G. H. Fowler (*Quart. Journ. Micro. Sci.*, xxviii. p. 1). The polyps have the usual two pairs of directive mesenteries; the lateral pairs of a particular polyp are not always equal in number. The septa arise only between pairs of mesenteries, as probably also do the tentacles. Nematocysts are evenly distributed over the tentacles. Zooxanthellæ are abundant in the superficial portions of the polyps and corallum.

In *Astræopora*, as in *Madrepora* and *Turbinaria*, the epitheca is extremely reduced. The genus *Astræopora* may be described as consisting typically of glomerate cœnenchymatous corals, in contrast, on the one hand, with *Madrepora*, which consists typically of branched, and on the other with *Turbinaria*, which consists typically of foliate cœnenchymatous corals. There is no definite system of budding, as in the other two genera, the colonies range in form from flat expanses to globular masses. The septa are very feebly developed. Fourteen species are known, of which nine are now described for the first time.

Mr. F. Jeffrey Bell had previously alluded (*Journ. Roy. Micro. Soc.*, 1895, p. 148) to the variations observed in large masses of *Turbinaria*, and his remarks are illustrated by a couple of excellent photographs. Those who have attempted to identify corals will also have experienced a feeling of dismay when confronted with variations in the form of coralla composed of apparently similar polyps, or of the vagaries of calicles on the same corallum. Often has the museum naturalist anticipated Mr. Bernard's question, "Is any classification of the various forms composing a genus into separate clearly-defined species possible?"

The following remarks by Mr. Bernard are worthy of the attention of systematists in other branches of zoology, and of those who interest themselves with the problems of variation.

"The only specimens which can be claimed with absolute certainty as specifically identical, are a few which have in each case been gathered at the same place and time, and resemble one another as closely as if they were two fragments of one and the same stock. Beyond these no certainty exists, and strict regard to the variations of form and structure would compel us to label all the remaining specimens as different species or varieties. Further, I do not remember ever having seen a specimen in other private or public collections which exactly recalled any single specimen in the British Museum. Are all these to be classed as new species? Such a course is only possible when the collection dealt with is very small; but when the number of specimens is measured by hundreds, one's courage fails. Hence recourse is had to a recognised but hardly satisfactory system of grouping: certain striking and conspicuous specimens (or single specimens which have already been described by previous workers) are selected as types, and the remainder are divided according as, in the opinion of the individual worker, they approach one or the other of these favoured specimens. The types are thus in the highest degree arbitrary and accidental, as is also, it must be confessed (though in a less degree), the selection of other specimens to be associated with them.

"It seems to me certain that we are rapidly nearing the time when our ever-increasing collections, revealing as they do the infinite grades of variation presented by living organisms—especially by stock or colony-forming animals, such as corals, in which the varying factors are doubled—will compel us to break loose from the restraint of the Linnean 'species.'"

Finally, the book is well printed, and the thirty collotype plates admirably illustrate the *facies* of the coralla. Mr. Bernard has wisely added three lithographic plates in which are represented carefully drawn details of a typical calicle of most of the species.

OUR BOOK SHELF.

A Compendium of General Botany. By Dr. Max Westermaier. Translated by Dr. Albert Schneider. Pp. x + 299. (New York: John Wiley and Sons. London: Chapman and Hall, Ltd., 1896.)

IN this book Dr. Westermaier has attempted to present an account of plants based on the lines indicated some years ago by Schwendener. But so far as English students are concerned, we cannot help thinking that he has rather fallen between two stools. The beginner, on the one hand, will find the book somewhat too advanced for his use; whilst on the other, a student who has already acquired some knowledge of the science, will discover that in the methods of dealing with some parts of his subject, Dr. Westermaier is rather one-sided. Thus, in discussing the factors operative in effecting the ascent of sap, a sketch is given of the views advocated by the author and by Schwendener, almost to the exclusion of those of other investigators; and we certainly cannot accept the conclusions as affording an "authoritative final explanation" of the process.

Notwithstanding, we are ready to admit that the book possesses some good points, and that it is interesting and even suggestive in places. But it scarcely deserves the somewhat ambitious title of "Compendium of General Botany."

The Testimony of Science to the Deluge. By W. B. Galloway, M.A. Pp. viii + 166. (London: Sampson Low, Marston, and Co., Ltd., 1896.)

IT is impossible to treat this book seriously. Such as it were common enough forty or fifty years ago, but we had hoped they had gone the way of the dodo. They are compounded after the following recipe: To the narrowest views in theology, add a general ignorance of the principles of inductive reasoning, collect a number of scraps from scientific books, mainly those written when geology was in its infancy, or if not, carefully separated from their context; stir all together into a hopeless confusion, and serve up with a sauce of pious intention flavoured by some inappropriate quotations from Scripture. Mr. Galloway is one of the stalwarts; he will be content with no local deluges; he will not let us off a square yard of the flood's extent, or a foot of its depth, except perhaps in equatorial regions. This cataclysm produced the rounded and scored rocks, the perched blocks and the boulder-clays with the scratched stones. But he does not explain to us why these products of a universal deluge are restricted to certain parts of the earth, and what were its leavings in districts where our so-called glacial deposits are wanting. A deluge, however, must have a cause. So Mr. Galloway finds this in a sudden shift of the earth's axis of rotation, amounting to about $18\frac{1}{2}°$; and he unearths some speculations by Dr. Halley, fully two centuries old, in support of his hypothesis. He tells us also much about terrestrial magnetism which does not seem particularly applicable, but we find no explanation of what caused the shift, no proof that the resulting disturbances of water would be powerful enough to transport heavy rock masses in an open country—particularly when it is admitted that the axis may not have "jumped" from one position to the other, but that "several rotations of the earth would probably take place in the progress of the change." Mr. Galloway cannot even cite his authorities accurately. J. Evans (now Sir John) becomes T. Evans, G. F. Browne's Ice-caves becomes Brown's Icy Caves, and so on. But it is waste of time to criticise this book. We present its author at parting with a motto which might have been printed appropriately on his title-page—"Deferar in vicum vendentem thus et odores, Et piper, et quicquid chartis amicitur ineptis." T. G. B.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Utility of Specific Characters.

ABSENCE from England has prevented my taking part in the discussion on this subject. At this stage I only desire to say that I quite agree with Prof. Ray Lankester, as I stated at the meeting of the Linnean Society, with respect to the contention that the specific characters of the systematist are not necessarily those upon which natural selection has directly acted in bringing about the specific differentiation. These external visible or measurable characters may be, and I believe often are, the outward expression of internal differences of constitution with which the external characters are correlated. In entering the lists at this late period, I am, however, mainly prompted by an omission on the part of Prof. Weldon to strengthen his own case by an argument which appears to me to be quite legitimate. The point at issue is whether the results of his laborious and, in my opinion, most valuable measurements of crabs, are to be interpreted as demonstrating the action of selection, or simply as revealing a law of growth. It might be imagined that if the latter alternative proved to be the correct interpretation, the case for selection falls to the ground. I do not take this view of the work, and, as a member of the Royal Society Committee concerned with the investigation, I am glad of the opportunity, afforded by the discussion in these columns, of giving expression to the idea which I have always entertained on this point, for whatever that idea may be worth. If future observation should show that there is no selection at work upon the young stages, weeding out the individuals whose breadth of carapace falls below a certain standard, but (according to the alternative) that the individuals get broader as they grow older, then it appears to me that the measurements may still be interpreted as indicating the action of selection; only the selection would have done its work in the past history of the species, instead of acting now, as on the original assumption. In other words, breadth of carapace (or some character correlated with it) had a selection value in the phylogeny; now this character appears at a late stage in the ontogeny. It is for Prof. Weldon to decide, by further observation, which of these interpretations is to be accepted. R. MELDOLA.

A Note on the Tesla Sparkand X-Ray Photography.

A CROOKES' radiometer was supported by its stem about four inches above the hand, which was placed upon a photographic plate enclosed in two light-tight cases. The terminals of a Tesla coil were placed about half an inch from the bulb on either side of it, inclined to one another at an angle of 120°, the vertex of the angle being in the axis of the radiometer. The Tesla discharge was allowed to bombard the bulb for four minutes. On development a clear picture of the bones in the hand appeared. The experiment shows that the X-ray photo can be produced when an exhausted bulb is used having no terminals.

I failed entirely to get any X-ray results when only the usual induction sparks bombarded the bulb, these sparks often destroying the vacuum. My Tesla apparatus gives a spark three inches long in air at normal pressure. The mica radiometer vanes after many experiments lost nearly all the black substance with which they were coated; it appeared in the form of a deposit on the inside of the bulb, and it was deposited in the form of concentric circles, the centres of which were situated exactly opposite to the ends of the pointed conductors attached to the Tesla coil.

Thinking that the mica vanes in the Crookes' radiometer might have played a considerable part in producing the X-ray photograph, I replaced the radiometer by a well-exhausted bulb 4 c.m. diameter, made of soda glass; the bulb was placed with respect to the conductors from the Tesla coil in the same position as the radiometer in the former experiment. I found that with the same exposure and distance, a good X-ray photograph of the bones in the hand was produced. During the experiment the bulb was lit up with a bright and yellowish green glow. Since the Tesla discharge rapidly produces ozonised air which is irritating to the nose, throat and lungs, it is best to place the terminals in a draught of air moving away from the operator.

Oxford, October 17. FREDERICK J. SMITH.

Siemens's Domestic Gas Fire.

HAVING reference to the request, in your number of September 17, for information on this subject, it is worthy of remark that the chief feature in Sir William Siemens's invention, namely the general idea of using gas to aid the ordinary fire (instead of applying it merely to heat inert material), is capable of much simpler, cheaper, and more extensive application than it has yet met with. Some such gas-aided fires, which have answered well, will be found described in the *Builder* of October 26, 1889. Their only difference from the ordinary household fire consists in the introduction of a few common gas jets among the fuel, which may be either coal or coke, or, preferably, a combination of the two.

The same idea may be applied in other ways. I lately saw, in an artist's studio, a "gas torch," which was attached to a flexible tube, and thrust between the bars when wanted. And Prof. Ramsay, in a recent lecture, has proposed another ingenious contrivance with the same object. In any case, the easy command which the gas gives over the general management of the fire is a great domestic convenience. Its only drawback is a temptation to indulge in a somewhat lavish gas consumption. W. POLE.

Athenæum Club.

The Variable Star *Z* Herculis.

IN the issue of NATURE for October 1, a note appears on the above variable, containing some important remarks on the general practice of smoothing curves, and rejecting outstanding observations. I fully agree with Mr. Yendell, that by carrying out this practice unduly, much valuable information may be lost. During eleven years' continuous observations of long-period variables, I have not rejected a single observation, and my light curves are produced by simply plotting down the observations (each of which is the mean of five comparisons with stars of known magnitude) on a squared form, and joining the dots by straight lines. The result has been to abundantly prove the existence of very many secondary and minor variations, both in the rise and fall of nearly all the stars under observation.

Specially marked instances of complex variation of light curve occur in the cases of R Aurigæ, T Ursæ Majoris, R Draconis, and S Cephei. In many cases the magnitudes and dates of maximum and minimum are very wide of the predictions; and I am forced to the conclusion, arrived at in the case of Z Herculis, "that the period of these stars must evidently be variable, though the character and value of the variation cannot at present be determined." CUTHBERT PEEK.

Rousdon Observatory, Lyme Regis, October 12.

"Eozoon Canadensa."

ONCE more the long controverted point as to the organic origin of this remarkable body was brought before Section C at the meeting of the British Association at Liverpool, by that indefatigable naturalist, Principal Sir William Dawson, Montreal, and which, as on all former occasions when brought before a scientific audience, provoked considerable discussion both for and against. Amongst others, Prof. Bonney took part. There is one remark that he made, which I beg of you to allow me to emphatically contradict, *i.e.* that the late Dr. Carpenter had been deceived by the geologist who sent him sections of the specimens from the West Highlands of Scotland. It was the writer of the present note who sent the sections referred to, and he thought of contradicting the assertion of Prof. Bonney at the time, but conceived it would be a gross abuse of politeness, not only to Sir William Dawson, but also to the members of the Section, to take notice of a matter so foreign to the subject under discussion, and also from the conviction that Prof. Bonney must have been misinformed. This is like the old story of the three black crows which, from being black as a crow, got metamorphosed into three black crows, feathers and all. The correspondence with Prof. Carpenter and others is lying before me, but at present I shall simply give a copy of my own letter that accompanied the specimens, and which I hope will satisfy Prof. Bonney and others that there is no truth in the assertion that I deceived Prof. Carpenter, or any of the other naturalists who believed that the structure was of organic origin. JAMES THOMSON.

6 Stuart Street, Shawlands, Glasgow, October 2.

(*Copy of letter referred to.*)

April 22, 1876.

DEAR SIR,—You did me the honour, nearly two years ago, to send me a type specimen of *Eozoon Canadensa*, in order that I might know the characters of that fossil organism if I should discover anything like it in the Highlands of Scotland. Since then I have been through part of Argyllshire, Inverness-shire, Ross-shire, Sutherlandshire, and Caithness-shire, and have at last discovered in the neighbourhood of Tarbert Harris what seems to me to be organic structure; and the fact that the rocks of that district have been described by Sir R. Murchison and others as being of Laurentian age, suggests that the enclosed specimens have some little interest, and more especially after the article that appeared in the *Annals* of last month. [I then gave a list of the names of the geologists and naturalists who had examined the specimens, all of whom, with one exception, pronounced the structure to be of organic origin. These names I forbear to introduce at present, but will give the latter part of the letter.] The parent rock is found interstratified with a dark grey shale. About ten feet to the south of this section there is some very beautiful graphic granite *in situ*; a suite of the specimens of which I procured. None of the graphic granite shows the beautiful structure that is seen in the intercellular spaces of the enclosed. The outer margin of the mass from which the enclosed is obtained approximates in external aspect to some of the varieties of graphic granite, suggesting the problem: What is graphic granite? May it not be a highly metamorphosed organic body? the enclosed being less metamorphosed, hence the preservation of the organic-like structure. Such seems to me probable, but not having seen the graphic granites from other localities, I cannot give an opinion, and will leave the solution of the problem in your hands, and shall be pleased to hear your opinion at your earliest convenience.

I am, faithfully yours,

(Signed) JAMES THOMSON.

To Prof. W. B. Carpenter, M.D., F.R.S., &c., London.

The Departure of the Swallows.

LORD HOBHOUSE's observations with regard to the "swallows," would lead one to suppose that all the birds would have gone away south before now; but yesterday I saw two swallows and three martins hard at work flying about. This may not be very late for the martin, but surely it is quite an unusual date for the swallow, though White of Selborne records having seen them as late as November 3, but does not say anything about the martin. He adds to his record, "None [swallows] have been observed at Selborne since October 11." E. P.

Newnham.

Wasps and Flies.

MANY years ago I was in a country butcher's shop, and saw several wasps occupied in cutting out and carrying off small chunks of meat. (Kidney was most in demand, as being "short" in texture.)

I pointed out the marauders to the butcher, and was told that he was always glad to see wasps in his shop, as they kept the bluebottles away. E. H.

October 17.

BARON SIR FERDINAND VON MUELLER.

NEWS of the death of this distinguished botanist and geographer reached London on the 10th inst., causing some surprise, as it was not known here that his health was failing. Born at Rostock in 1825, and educated at Kiel, he emigrated to Australia in 1847, in consequence of hereditary symptoms of phthisis; having previously lost his parents. Mueller belonged to the school of botanists, now fast diminishing in numbers, who began their studies in the field instead of in the laboratory. Before leaving Europe, he devoted much time between 1840 and 1847 to the investigation of the flora of Schleswig-Holstein. On his arrival in Australia, he took service as a druggist's assistant in Adelaide—a post he seems to have held for a brief period, as he was soon engaged in exploring South Australia. From 1848 to 1852 he travelled at his own expense. At this date he was appointed, by Governor La Trobe, to the newly-created post of Government botanist, and soon visited the previously unexplored Australian Alps. About this period he entered into correspondence with the late Sir William Hooker, which led to the publication of the results of his earlier journeys in Hooker's *Kew Journal of Botany*, beginning with the fifth volume. In 1854 the Victorian Institute was founded[1]—the first institution of its kind, I believe, in Australia proper, though Tasmania had its Royal Society some three years earlier; and Mueller was one of the first and most prolific contributors to its *Transactions*. It was here that he published the new plants collected in the Australian Alps.

In 1855–56 Mueller was attached as botanist to Gregory's expedition across North Australia, from the Victoria River to the Albert River. In 1857 he was appointed Director of the Melbourne Botanic Garden; but in 1873 he was superseded, owing to his too rigidly scientific management, though he still retained charge of the herbarium and library. Great as were his exertions and his enthusiasm on the introduction and cultivation of useful and ornamental plants, he failed from a practical standpoint. His work on "Select Extra-tropical Plants eligible for Industrial Culture," &c., was an extraordinary success; yet not on account of its practical value, for it has none, but as a work of general reference it is very useful. Nine editions have appeared, including an American, a French, and a German edition.

During the forty-nine years of his Australian life, Mueller was such an unceasing and copious writer, that it is impossible to do more than glance at some of his more important publications. It was from the first his ambition to write a "Flora" of the entire country, and his almost innumerable papers were written with that view; but when it came to the point, the task, for various reasons, was confided to the late George Bentham, and Mueller most cordially co-operated with him by sending his collections and notes to Kew. Of that I can speak with some authority, having acted a very humble, though congenial, part in connection with the earlier volumes of the classical "Flora Australiensis." Mueller, however, found enough to do in publishing the thousands of novelties collected by himself, and by others under his direction. His "Fragmenta Phytographiæ Australiæ" is the chief, but by no means the sole repertorium of his descriptions. Prominent among his more utilitarian works are the illustrated monographs of the genera *Eucalyptus* and *Acacia*. His "Census of Australian Plants," so carefully compiled with regard to dates, references and authorities, is exceedingly useful for purposes of comparison with the floras of other countries, and has been extensively used by the writer and others. But Mueller was much more than a botanist and geographer; he was always a promoter, and often the originator, of movements for the scientific, social, and material welfare of the country he had made his home. He was in turn President of the Philosophical Institute, of the Geographical Society (Victorian branch), of the Australian Association for the Advancement of Science, and various other bodies and societies. He has also the reputation of having been a most devout and philanthropical person. And, in spite of his not being a practical horticulturist, he did more probably than any other person to promote the commercial—that is to say, the useful—development of cultural industries in Australia, and more than any other person in the diffusion of useful Australian plants in other parts of the world. He had probably a wider correspondence than any living botanist, and few are the establishments that have not been in some way benefited by him. The value of his work consists largely in the fact that he did exactly the kind of work that was required in a young country for its material as well as its moral development. It is true that his work exhibits more industry than genius; but, after all, what he undertook gave little scope for the latter quality. There was, however, a weak side in his character, which it would be affectation to pass over entirely, though one would say as little about it as possible. He had an inordinate craving for titles, distinctions, and admiration. This led him to publish, in all sorts of places and languages, what it would have been much better to have kept together, and to indulge in vagaries in botanical nomenclature which are simply deplorable and damaging to his character as a sincere servant of science. Nevertheless, the country to which he devoted nearly half a century of active life was proud of him, and justly so, and willingly honoured him during his lifetime, and will doubtless long cherish his memory

W. BOTTING HEMSLEY.

[1] Subsequently the Philosophical Institute, and then the Royal Society.

NOTES.

OUR American contemporary, *Science*, suggests the formation of an International Association for the Advancement of Science, recent events having shown that members of the various national Associations regard co-operation in a cordial manner. The British Association meets in Toronto next year, and the American Association, after meeting at Detroit, on the Canadian frontier, will adjourn to Toronto to welcome our Association to the American continent. Another instance of community of feeling is afforded by the decision of the British Association to meet at Dover in 1899, in order to promote an interchange of visits between its members and those of the French Association, which will meet at Boulogne in the same year. These signs of fellowship indicate that the time has come when an international congress for the advancement of science may be profitably considered. Among the many subjects which would benefit by international co-operation are bibliography, nomenclature, definition of units, exploration, and science teaching. The amalgamation would also impress the collective weight of science upon the outside world, and would thus be able to claim a more adequate support and recognition of scientific progress. The proposal of our contemporary is that the first meeting of an international congress of this character should take place in Paris in the first year of the twentieth century. In considering the question of the amalgamation of Associations for the Advancement of Science, it must be remembered that great international congresses are often too unwieldy to be satisfactorily managed, and that the confusion of tongues at such gatherings is a constant factor working against success.

SCIENCE has just lost two of its foremost workers. We refer to Dr. Henry Trimen, F.R.S., late Director of the Royal Botanic Gardens, Ceylon, who died at Peradeniya on Sunday last, in his fifty-third year; and M. Tisserand, the distinguished Director of the Paris Observatory, who died on Tuesday.

THE Academy of Natural Sciences of Philadelphia has conferred the Hayden Memorial Geological Award for 1896 on Prof. Giovanni Capellini, of the University of Bologna.

A SLIGHT earthquake occurred on the 16th inst. in Northwest Italy. A small shock was felt at several places in Southern Piedmont and in Liguria, but no damages have been reported. The disturbance was observed by Prof. Guido Cora in Costigliole d'Asti, and he informs us that the shock took place at 7.18 a.m.; it was undulatory, in the direction from north to south, and lasted only a few seconds.

MR. WILLIAM WHITAKER, F.R.S., who joined the Geological Survey of Great Britain in 1857, has just resigned his post on the staff. For many years he has acted as District Surveyor in superintending the survey of the southern counties of England. The loss of his experienced services will be much felt by his colleagues, to say nothing of the loss of an ever-cheery companion. We trust he may long live to labour in the cause of geology.

THE Paris correspondent of the *Times* reports the occurrence of a serious explosion, on Saturday last, in a building where for two months M. Raoul Pictet, the distinguished chemist, has been manufacturing acetylene. One of the steel tubes, 3 feet long, used for storing the new gas, exploded. Such was the violence of the explosion that the building was blown up, and the windows of all the neighbouring houses were shattered. It has been ascertained from the fragments that the tube which burst, and which was practically full, was returned from Brussels on the 13th inst. along with seventy-four empty ones. The exact cause of the explosion is at present unknown.

WE regret to see announcements of the deaths of Dr. David Garber, Professor of Mathematics and Astronomy in Muhlenburg College, Allentown, Pa.; Dr. Theodor Margo, Professor of Comparative Anatomy and Zoology in the University of Budapest; Dr. Rochard, formerly President of the Paris Academy of Medicine; Dr. W. H. Ross, formerly Professor of Anatomy in the Mobile Medical College, Alabama; Dr Callender, Professor of Neurology in the Vanderbilt University, Nashville.

THE following gentlemen have been nominated by the Council of the London Mathematical Society for election as the Council and officers for the ensuing session:—President, Prof. Elliott, F.R.S.; Vice-Presidents, Major Macmahon, R.A., F.R.S., M. Jenkins, and Dr. Hobson, F.R.S.; Treasurer, Dr. J. Larmor, F.R.S.; Secretaries, R. Tucker and A. E. H. Love, F.R.S.; other members, Lieut.-Colonel Cunningham, R.E., H. T. Gerrans, Dr. Glaisher, F.R.S., Prof. Greenhill, F.R.S., Prof. Hill, F.R.S., Prof. Hudson, A. B. Kempe, F.R.S., F. S. Macaulay, and D. B. Mair. At the annual general meeting of the Society, which will be held on November 12, Major Macmahon will take as the subject of his valedictory address, "The Combinatory Analysis." On the same evening the De Morgan medal will be presented to S. Roberts, F.R.S., who will be the fifth recipient of the medal.

CONJOINTLY with the Leigh Browne Trust, the Humanitarian League has arranged a series of five "Humane Science Lectures," to be given at St. Martin's Hall, Trafalgar Square, W.C. The programme is as follows: October 27, "The Need of a Rational and Humane Science," by E. Carpenter; November 17, "Natural Selection and Mutual Aid," by P. Kropotkin; December 8, "The Humane Study of Natural History," by J. Arthur Thomson; January 19, "The Treatment of Criminals," by Rev. Douglas Morrison; February 9, "Suggestion: its place in Medicine and Research," by Dr. Milne Bramwell. The general title, under which the lectures are grouped, is explained in the following extract from the prospectus. "The various departments of science are ever growing rapidly in extent, so rapidly that their correlation tends to fall behind, and in some directions to be overlooked; yet this correlation is not only an end, but a means of scientific progress. The objects, methods, and results of each department should tend to the advance of science as a whole, physical and mental, and only when thus directed will they conduce to permanent human welfare. An uncorrelated department of science tends to lose either life or balance. To illustrate this, and to show methods of research which do not violate the essential unity of nature, and the excellent result to be obtained by such methods, is one of the aims of the proposed course of lectures."

FIFTY years ago last Friday, on October 16, 1846, the first surgical operation under the influence of ether was performed in the Massachusetts General Hospital, Boston, by Dr. John C. Warren, the anæsthetic being administered by Dr. W. T. G. Morton, who had already proved its anæsthetic properties in tooth extraction. In commemoration of the introduction of this blessed relief to suffering humanity, the current number of the *British Medical Journal* contains a very interesting account of the circumstances attending the discovery and use of ether as an anæsthetic, and of the subsequent introduction of chloroform into general use. The credit of having practically proved for the first time the possibility of abolishing sensation so entirely that a painful operation could be done without being felt, belong to Horace Wells, a young dentist of Hartford, Connecticut, who had a tooth extracted while under the influence of nitrous oxide gas on December 11, 1844. Morton followed with the use of ether in 1846, and in the next year Sir James Simpson communicated his discovery of chloroform to the Medico-Chirurgical Society of Edinburgh, in a paper entitled "Notice of a New Anæsthetic Agent as a Substitute for Sulphuric Ether," the first operation under its influence being performed on November 15, 1847. To Simpson also belongs the credit of having made anæsthesia triumph over the violent opposition with which it was assailed. In addition to a general history of anæsthetics, the *British Medical Journal* contains an account, by Dr. W. Squire, of the first operation under ether in Great Britain, and a retrospective article by Dr. Dudley W. Buxton.

Two interesting instances of birds apparently profiting by experience are related by Dr. R. Williams in the *Zoologist*. The proprietor of a certain wood, having found that the wood was a nesting stronghold of blackbirds and thrushes, made systematic raids on their nests in consequence of the damage done by the birds to his fruit. The result was that both the blackbirds and thrushes departed from their usual habit in the choice of nesting sites, and, instead of building in the thickets and small fir-trees with which the wood abounded, they built their nests upon the ground. The second case refers to the common sandpiper, which usually nests on patches of gravel thrown up by a river, and more or less covered with docks and other weeds. On one occasion when the sandpipers had built their nests and commenced to sit, the river near Dr. Williams' house overflowed its banks, and the nests were destroyed. On the subsidence of the water, the birds built again on their old sites, only to have their nests again swept away by another flood. In the next season the sandpipers neglected the eligible riparian building sites, and nested away from the river. The observations indicate that the birds remembered former calamities, and made use of their dearly-bought experience by choosing positions inaccessible to the highest flood. The birds continued to nest at some distance from the river for three seasons, after which they resumed their former nesting-places close to the water.

FROM a short article in the *Chemical News*, we learn that in the course of researches on monazite sand, M. P. Barrière appears to have come upon a new elementary body, to which he has given the name *Lucium*, and which he purposes using for the production of an incandescent gas light similar to that of Auer von Welsbach. Careful investigation has been made of the new and independent character of lucium, in order to prove that its use was not anticipated by the Welsbach patents. The examination showed that while the salts of cerium, lanthanum, and didymium form with sodium sulphate insoluble double salts, lucium does not. Thorium and zirconium form insoluble double salts with potassium sulphate; this is not the case with lucium. Yttrium, ytterbium, and erbium are not precipitable by sodium thiosulphate, whilst lucium chloride is precipitable. From glucinium lucium differs, as its salts are precipitable by oxalic acid. The lines in the spectrum of lucium are special, and only approximate slightly to those of erbium. Erbium oxide, on ignition, appears of a very pure rose-colour, and its nitrate is red. On the contrary, lucium oxide is white, slightly greyish, and its nitrate is white. The aqueous solutions of the erbium salts are red or rose-colour; those of lucium, even if containing 15 or 20 per cent. of the salt, are almost colourless. These and other reasons seem to show that lucium is a new distinct elementary body. Its atomic weight has been calculated as = 104.

THE last number of *Modern Medicine and Bacteriological Review* contains a notice of some elaborate investigations which have been carried out by Drs. Chittenden and Mendel, of the Physiological Department of Yale University, on the influence of alcoholic drinks upon the chemical processes of digestion. The report in question was prepared by request, and presented to the Committee for the investigation of the liquor problem in New York. The investigations were made by means of artificial digestive experiments, in which the digestive fluids were allowed to act upon various food substances under definite and constant conditions. Absolute alcohol in four cases appeared to actually stimulate digestive action by a fraction of 1 per cent., but the amount of alcohol present did not exceed 1 or 2 per cent. Whenever alcohol was added in quantities over 2 per cent., digestive activity was markedly checked; in one instance, 3 per cent. of alcohol reduced the digestive activity by 17·6 per cent. Pure rye whisky containing 50 to 51 per cent. of alcohol yielded practically the same results; even an addition of 1 per cent. of this spirit was found, taking the average of the experiments, to reduce digestive activity by over 6 per cent. In three cases, however, an increase in digestive activity of from 3 to 5 per cent. was recorded when additions of whisky in the proportion of from 1 to 3 per cent. were made. Brandy, rum, and gin gave practically the same results. Messrs. Chittenden and Mendel consider that their experiments, as far as they go, justify them in concluding that "whisky can be considered to impede the solvent action of the gastric juice only when taken immoderately and in intoxicating quantities."

SEVERAL successful experiments of scientific kite-flying, for the purpose of exploring the upper air, have been made during the past summer at Mr. Rotch's observatory at Blue Hill, near Boston, and the results of some of these are noticed in *Science* of October 2. The kites used are of the tailless and the box patterns, provided with registering instruments specially made by M. Richard, of Paris. The altitudes reached are determined in three ways—by theodolites, by the angle and length of the kite-line, and by the barometric pressure recorded. The height of one mile was exceeded on six occasions; on July 20, at a short distance above the earth, the kite entered a cloud in which the humidity reached saturation, while after a further ascent of about 2500 feet, the air was found to be much dryer. On August 1, the recording instrument reached an altitude of 7333 feet above sea-level. The temperature at the maximum altitude was 20° less than at the observatory (640 feet above sea-level), while the relative humidity showed variations of 30 to 80 per cent. The results obtained from these investigations at Blue Hill are attracting much attention.

IN the current number of the *Annales de Chimie et de Physique*, M. Moissan continues the account of his researches with the electric furnace. He gives the preparation and properties of titanium, molybdenum, uranium, and the borides of iron and of carbon, the preparation of manganese, and an historical account of the researches already made on the crystallised carbides of the alkaline earths. In the latter paper he lays claim to the discovery of crystallised carbide of calcium, while assigning to Mr. Wilson the credit of having introduced its manufacture in the United States. With regard to titanium, M. Moissan has found that with a current of 50 ampères and 50 volts, titanic acid is converted into crystallised oxide of titanium. With 350 ampères and 70 volts, the bronze-yellow nitride, Ti_2N_2, is obtained. When 1200 ampères and 70 volts are used, the temperature rises above the point of decomposition of this substance, and the carbide TiC, is formed, free from nitrogen; and if this is heated with an excess of titanic acid, titanium containing only 2 per cent. of carbon is obtained. These successive actions, says M. Moissan, give a decisive proof of the increase of temperature of the electric arc dependent on an increase of the current, and form the starting-point of another long series of experiments. The preparation of the crystallised compound of iron and boron containing over 15 per cent. of boron, and nearly corresponding to the formula FeB, effectually disposes of the assertion of some workers on iron that it is impossible to alloy these two elements.

THE Australian Museum, Sydney, like many other colonial museums, suffers from lack of funds to acquire specimens by purchase. In the report of the Trustees of the Museum, lost opportunities due to this deficiency are lamented. To an enthusiastic curator nothing is more heartrending than to see objects urgently needed in the collection under his charge, and to be unable to acquire them; a woman coveting a pretty bonnet which she cannot buy, may be able to understand his feelings, but no one else could adequately sympathise with him. Owing to this want of funds, it has only been possible for a few isolated purchases to be made during the year 1895. The same difficulty applies to collecting, and consequently the Trustees have been unable to continue systematic exchanges with other institutions from which they have been accustomed to receive specimens. Notwithstanding these limitations, 11,499 specimens were acquired during the year. The more important acquisitions were:—A fine collection of mounted sheep, goats, and dogs from the museum at Florence, a large native drum from the Bismarck Archipelago, and one of Captain Cook's original MS. Journals, or Log of H.M.S. *Endeavour*, which was kept by him in triplicate. It is satisfactory to know that a sum of £6000 has been voted by Parliament for the further extension of the Museum buildings. The assistance came none too soon, for an accident to the plaster revealed the astounding fact that the woodwork of the entire roof over the central part of the main building had been destroyed by white ants. The destruction was so complete that it is surprising that the portion affected did not collapse. The building had to be temporarily supported in order to make it safe until funds became available for the erection of a new roof. In spite of these little tribulations, Mr. R. Etheridge, jun., and his assistants accomplished a large amount of work during the year covered by the report. Many of the collections have been thoroughly overhauled and rearranged, while the condition of all of them appears to have been improved.

Mr. T. Whitelegge, who has charge of the marine invertebrata in the museum, has conducted a number of experiments to test the value of formol as a preservative. The results have proved highly satisfactory, more especially in regard to delicate marine organisms; they show that a 2½ per cent. solution is sufficient to preserve many delicate organisms, and that for most others a 5 per cent. solution is ample.

A LIST of books in which botanical book-hunters will be especially interested, is the "Bibliographie Botanique," just issued by Messrs. J. B. Baillière et Fils, Paris. The books and brochures in this catalogue are classified geographically.

A BRIEF account of the excursion to the Isle of Man, after the recent meeting of the British Association, was given in NATURE of the 8th inst., by Prof. W. A. Herdman. It may interest some of our readers to know that a complete descriptive report of this supplementary meeting of archæologists, geologists, zoologists, and botanists, occupying no less than fourteen columns, appears in the *Isle of Man Times* of October 3.

WE have received the Report of the Botanical Survey of India for the year 1895–96, by the Director, Dr. G. King. The Botanical Surveys of Northern India and of the Bombay Presidency have been steadily progressing; while that of Southern India has been temporarily interrupted by the death of its Director, Mr. M. A. Lawson. Work has also been done in Assam and in Burma.

IN connection with this Survey, Dr. D. D. Cunningham and Mr. D. Prain have published a very interesting "Note on Indian Wheat-rusts," containing a great deal of valuable information respecting the diseases known as "rust," which attack the wheat and barley crops in different parts of India, and which appear to belong to four different species of the genus of parasitic fungi *Puccinia*, and their connection with a fungus which attacks *Launea asplenifolia*, a very common weed among cultivated crops, belonging to the Compositæ.

MESSRS. WILLIAM WESLEY AND SON have prepared and issued a new "Natural History and Scientific Book Circular" (No. 126), containing titles and prices of nearly two thousand works on the Invertebrates. The catalogue comprises descriptions of handbooks and other general works, a classified list of works on the Invertebrates from Protozoa to Mollusca, arranged according to Claus' "Text-book of Zoology;" and a section on economic entomology. This intelligent arrangement of the titles makes the catalogue a useful index to zoological literature.

THE renowned *Zeitschrift für physikalische Chemie* has now a friendly rival in the *Journal of Physical Chemistry*, edited by Profs. Wilder D. Bancroft and Joseph E. Trevor, and published at Cornell University. The first number of the new journal contains articles on "Irreversible Cells," "Chemistry and its Laws," and "Ternary Mixtures," reviews of books, and critical digests of papers bearing upon different phases of physical chemistry. The journal thus follows much the same lines as its admirable German prototype, and we anticipate that it will play a similar important part in the development of the rich domain where the realms of physics and chemistry overlap. The publication will be issued every month except July, August, and September. The London agents are Messrs. Gay and Bird.

A FRESH light has been thrown on the constitution of the nitro-paraffins by the researches of Prof. Hantzsch, of Würzburg, which are recounted in a recent number of the *Berichte*. At one time it was thought that the presence of the nitro-group in the methane molecule imparted an acid function to one of the hydrogen atoms, and that in the formation of a salt this atom was replaced by the metal, which thus became directly combined with the carbon atom, the formula of sodium nitro-methane being written $CH_2Na.NO_2$. The researches of Nef and others have, however, shown that most probably the free nitro-paraffins have a different constitution from their salts, and that in the latter the metal is not directly combined with carbon, but with oxygen. Prof. Hantzsch's discovery shows that this view is almost certainly correct. He has found that certain aromatic derivatives of the nitro-paraffins actually exist in two distinct forms, one of which, the normal compound, is an indifferent substance incapable of forming salts, and has the formula $R.CH_2\,NO_2$; whilst the other, the iso-compound, has the formula $R.HC\overset{O}{\diagup\!\!\!-\!\!\!\diagdown}N.OH$, and acts in all respects as an acid. When, for example, a solution of the sodium salt of bromophenylnitromethane is acidified with hydrochloric acid, the iso-compound is precipitated as a crystalline mass, which melts at 90°. When this is preserved, however, either alone or in solution, it rapidly undergoes a molecular change, and after twelve hours melts at 60°, and has all the properties of the normal compound, which can itself be directly obtained from the solution of the sodium salt by decomposition with a weak acid, such as carbonic acid. The normal compound does not react with ferric chloride, is much less soluble than its isomeride, and in aqueous solution is a non-electrolyte; whereas the iso-compound is a stronger acid than acetic acid, and gives a characteristic colouration with ferric chloride, a further proof that it contains the hydroxyl-group. The normal compound is at once converted by alkalis into the iso-derivative, which then immediately dissolves, forming the corresponding salt.

THE additions to the Zoological Society's Gardens during the past week include a Bonnet Monkey (*Macacus sinicus*, ♂) from India, presented by Dr. Allen M. Cleghorn; two Tigers (*Felis tigris*, ♀ ♀, juv.) from India, presented by H.H. the Gaekwar of Baroda; a Wild Cat (*Felis catus*), a Common Genet (*Genetta vulgaris*), two Avocets (*Recurvirostra avocetta*), two Eyed Lizards (*Lacerta ocellatus*), seven Green Lizards (*Lacerta viridis*), European, three Prairie Marmots (*Cynomys ludovicianus*), a Cat Bird (*Galeoscoptes carolinensis*) from North America, a Sulphury Tyrant (*Pitangus sulphuratus*) from South America, a Grey Coly Shrike (*Hypocolius ampelinus*) from Scinde, two Greater Black-backed Gulls (*Larus marinus*), a Herring Gull (*Larus argentatus*), a Black-headed Gull (*Larus ridibundus*), British, presented by the Lord Lilford; two Grey Francolins (*Francolinus ponticerianus*) from India, presented by Lieut.-Colonel D. K. Robertson; a Loggerhead Turtle (*Thalassochelys caouana*) from Spain, presented by Miss A. Steer; five Spotted Salamanders (*Salamandra maculosa*), European, presented by Miss Minks; a Yellow-cheeked Lemur (*Lemur xanthomystax*) from Madagascar, a Moorish Tortoise (*Testudo mauritanica*) from North Africa, deposited; two Nylghaies (*Boselaphus tragocamelus*, ♂ ♀) from India, received in exchange.

OUR ASTRONOMICAL COLUMN.

TELEGRAMS ABOUT COMETS.—At the meeting of the Telegramm-Commission at Bamberg on September 18 last, it was decided to make an alteration in the scheme of cypher that has been in use up to the present time. It has now been settled that the *date of observation* and the *brightness* of the object shall be included in a group of five figures, and allowed for in the "control" figures, which are always added as a check.

To prevent mistakes the following example is added—

"Comet Witt D.A. 09120 October 13000 Berlin, Urania. 02554, 07630, 35946, 35957, 04207."

This reads when deciphered—

"New Comet Witt 1896 D.A. 9 October, 13h. mean time Berlin, Urania. Apparent R.A. = 25° 54′. Apparent N.P.D. 76° 30′. Daily movement −14′, −3′. Magnitude 12m."

In cases where it is impossible to give the magnitudes, the three last figures will be written as three zeros.

This alteration will come into use on the first of next month (November) in all telegrams from the "Centralstelle" in Kiel.

COMETS PERRINE (1895 IV.) AND PERRINE-LAMP (1896).—A most interesting description of these two comets, obtained from eye observations and photographs, is given by Joseph and Jean Fric, in a communication presented on April 24 of this year to the Cisaře Františka-Josefa (5th year, No. 26). Up to the time of the perihelion passage of Comet Perrine, the observations made during this period were published in the *Bulletin* of the same Academy (No. 8), the last observation dating from December 9. The path of the comet at and since the time of its perihelion passage, is here indicated in the chart accompanying this communication. In the cliché taken on February 15 of this year, the tail of this comet appears in the form of a thin line, with a position angle of 120°, being turned towards the sun. This latter exceptional fact has been verified on two negatives taken on February 20 and 21, both of which were made under the best atmospheric conditions. The description of the original clichés that were taken on February 15, shows us that this comet presented a very dim line directed towards the sun, and of 1° in length. The nucleus was nearly of fifth magnitude, and resembled a star. This cliché is further interesting from the fact that it shows the first trace of the new comet Perrine-Lamp. On February 20 the tail presented a fan-like form, being somewhat more dense at the position angle 120°. Its breadth was 15' at its centre, and its length 1½°. The position of the tail was abnormal, being turned towards the sun. By April 21 its length had increased to 2°. On March 15 only the nucleus was visible, and by the 20th a photograph showed only a feeble trace of it. The clichés which show the appearance of the Perrine-Lamp comet are also full of interest. A striking feature of these photographs is the bifurcation of the tail, exhibited on the cliché made on February 22 and March 3, and its spontaneous development on February 21 and 22. The direction of the tail, in its relation to the sun, was normal. The communication contains, besides the chart referred to above, reproductions of the several clichés mentioned in the pamphlet.

THE CANALS OF MARS.—First Schiaparelli and then Lowell have both shown us that the Martian surface is a network of canals. The number of canals, as the latter observer informs us, is really far more numerous than has yet been recorded, but these are less in size, and only flash out clearly under the very best and exceptional conditions of seeing. As one would expect, the greater the number of canals, the greater becomes the difficulty in identifying them. In fact, unless one has first-class conditions for observation, and also considerable experience, it is rather rash to suggest the discovery of new canals. Mr. Brenner seems, however, to be certain of his powers of identification, and describes some of his observations in the *Bulletin de la Société Astronomique de France* (October). Without diagrams it is unsatisfactory to try to describe the positions of these suspected new canals, but a reference to Mr. Lowell's chart seems to indicate that these may be cases of not exact identification. Mr. Brenner makes it very difficult for readers of his notes, as he inserts woodcuts of the surface markings, numbered most carefully, these numbers having no reference at all to the text. For instance, referring to one of the drawings he says: "One sees the following canals: (1) Steropes, (2) Glaucus, (3) Phlegethon, (4) Cérannius, &c."

As these are the only numbers used in the text, it is natural to suppose them to refer to the illustrations; this, however, is far from the case, hence the delusion.

An interesting point is touched upon by Prof. V. Cerulli, concerning the conspicuousness of the canals Ulysses and Sitacus. These canals are not charted by Schiaparelli, but were discovered by Lowell two years ago Prof. Cerulli asks the question, How is it that they have been previously not seen, considering that the former is now as prominent as Sirenius and Araxes, both in the chart of Schiaparelli, and that the latter surpasses in distinctness the Euphrates and Phison? They are not simply canals that were observed in 1894 for the first time, but they are canals which till then had no existence. Mr. Lowell also remarked a peculiarity in this respect. Referring to the canals not on Schiaparelli's chart (Lowell, "Mars," p. 148) he says: "The most peculiar case, however, is the relative conspicuousness of the Ulysses."

THE HUXLEY LECTURE.—RECENT ADVANCES IN SCIENCE, AND THEIR BEARING ON MEDICINE AND SURGERY.[1]

II.

NOW let me turn to another theme suggested by what has happened in science and in the profession since the days of Huxley's studentship, and that is the complexity of the bearings of any one discovery, of any one advance, as well on science itself as on the applications of science.

In the garment of science, with which man is wrapping himself round, or rather is being wrapped round, the several threads are woven into an intricate web. As the loom which is weaving that ever-spreading garment takes in new warp and new woof, such threads only of each are taken in as can be fitly joined to those which have come in before, each thread as it is twisted in becomes a hold for other threads to be caught up later on. No single observation, no single experiment stands alone by itself, nor can its worth be rightly judged by itself alone. The mistaken philanthropists who have put restrictions, and would put more on physiological investigations, betray that ignorance of the ways of science, which seems to be a necessary condition of their attitude, when they ask us to state in a sentence the direct application to the good of man of each experiment on a living animal. In the doors of science, each the opening as often of a path as of a chamber, it is not, as such folk seem to think, that each bobbin pulls only one latch. Every experiment, every observation has, besides its immediate result, effects which, in proportion to its value, spread away on all sides into even distant parts of knowledge. The good of the experiment by itself is soon merged in the general good of scientific inquiry. The science of physiology, and by implication the art of medicine, is built up in part on experiments on living animals; in part only, but that part is so woven into all the rest that any attempt to draw it out would lead to a collapse of the whole.

It is because each experiment or observation is thus a thread caught up in a close-set web, that its value depends not alone on the mere result of the experiment or observation itself, but also, and even more so, on the time at which, and on the circumstances and relations under which it is made. This truth the real worker in science has borne in upon him again and again; it is this which leads him to that humility which has ever been the outward token of the fruitful labourer. He feels that it is not so much himself working for science as science working through him.

Let me attempt to illustrate this by dwelling on some two or three single observations in physiology, made almost at the time, or very soon after the time at which Huxley was a student. It will, I think, be seen that each of them has reached a long way in its bearing on the science of physiology and on the art of medicine, that the full effect of each has been dependent both on what went before and on what has happened since, and though they were all made, so to speak, long ago, some of their fruits were brought in as it were yesterday, and their full fruition is perhaps not yet accomplished.

I will first invite your attention to a single experiment, for, though repeated on various animals, we may call it a single experiment, which in the fall of the year 1845 Ernest Heinrich Weber, then Professor of Anatomy at Leipzig, and his brother Eduard Friedrich, reported to an assembly of Italian scientific men in Naples, and of which they subsequently published an account in Müller's *Archiv* in 1846. Making use of the recently introduced rotating electro-magnetic apparatus (the physical discovery begetting the physiological one), they found that powerful stimulation of the vagus nerves had the unexpected result of stopping the heart from beating.

This single experiment, which I may quote by the way as a typical experiment on a living animal—for it is impossible to imagine how the discovery of this action of the vagus on the heart could have been made otherwise than by an experiment on a living animal—this single experiment has made itself felt far and wide throughout almost the whole of physiology.

In the first place, it has made us understand in a way impossible before the experiment, how through the intervention of the nervous system, the work of the heart is tempered to meet the strain of varying circumstances. As I said a little while back, only a few years before even eminent observers were

[1] Delivered at Charing Cross Medical School, on October 5, by Prof. Michael Foster, Sec.R.S. (Continued from p. 583.)

groping about in a dim light, hotly discussing whether the brain and spinal cord could affect the beat of the heart. To all these discussions Weber's experiment came as a great light in a dark place.

There is no need for me to insist how this knowledge that impulses descending the vagus slow or restrain the heart beat, and the knowledge genetically dependent on this, that impulses reaching the heart along the cardiac sympathetic nerves from the thoracic spinal cord, stir up the heart to more vigorous or frequent beats, have since served as a guide for the physician in the intricate problems of cardiac disease, and that with increasing security as our knowledge of the details of the actions has increased. The knowledge may not always have been wisely used. On this point perhaps I may be allowed to repeat the caution which I may have given elsewhere, concerning the dangers of taking a new physiological fact direct and straight, raw and bleeding as it were, from the laboratory to the bedside. The wise physiologist takes care, even in physiology itself, not to use a new fact as an explanation of old problems without a due testing and a direct verification of its applicability. How much more is it needful that the doctor who sails not on the calm seas of the phenomena of health, but amid the troubled tempests which we call disease, should not hastily and heedlessly rush to make practical use of a new fact, tempting as the use may be, until he also has tested its applicability by that clinical study which is his only sure guide. But this is by the way.

In the second place, as a mere method, Weber's discovery has in physiological experimentation borne most important fruit. Before Weber's experiment many an investigation, not only on the vascular system itself, but in many other branches of physiology, came to a standstill or went astray because the experimenter had not the means on the one hand to stop or slacken, or on the other to quicken and stir up the heart, without interfering largely with the object of his research. Thanks to Weber's experiment and what has come out of it, that can now be done with ease, and thus solutions have been obtained of problems which otherwise seemed insoluble.

In the third place, the experiment has had a profound and widespread influence by serving to introduce a new idea, that idea which we now denote by the word inhibition. Before the experiment, though men's minds were gradually getting clearer concerning the nature of a nervous impulse, all known instances of the action of a nervous impulse had for the result an expenditure of energy; and it was a still open, though hotly debated question whether in such actions as when a muscle was thrown into contraction by a nervous impulse this feature of expenditure was not impressed on the muscle by the very nature of the impulse itself. That question the experiment answered in the negative once and for all. Whatever the exact nature of a nervous impulse, it was evidently of such a kind that it might on occasion check expenditure and bank up energy in an increased potential store. Observation soon showed that the heart and vagus was no solitary example. It was recognised that the due regulation of many of, if not all, the so-called nervous centres was secured not merely by the intrinsic forces of passive rest making themselves felt in the absence of stimulation, but also, and even more so, by the alternating play of antagonistic influences. Throughout all the sciences the resolving a stability seemingly due to intrinsic causes into an equilibrium arising out of the balance of opposing forces, has again and again marked a step forward; and it is perhaps not too much to say that a like analysis, prompted by the story of the vagus and the heart, has profoundly modified all our conceptions of the way in which nervous impulses, sweeping along the intricate yet ordered network of paths in the brain and spinal cord, determine the conduct of life. The idea has of course been abused as well as used, as what idea has not? Such a word as inhibition could not but fail to have a blessed sound in the ears of the ignorant; the idea has been ignorantly and wrongly applied; but this is of little moment in view of the help which it has given to wise and well-directed inquiry.

And the idea has spread with fruitful results beyond the limits of nervous impulses; it has been carried deep down into the very innermost molecular processes of life. The closer we penetrate into the physical-chemical events through which living matter grows, lives and dies, the clearer does it seem that life itself is a shifting outcome of two opposing sets of changes—one synthetic constructive, the other destructive, analytic, and that the key to this and that riddle of vital action lies within the grasp of him who can clearly lay hold of the mutual relations of these conflicting changes. The story of the vagus and the heart is a tale, not of the heart alone, not of the nervous system alone, but of all living matter. The light which first shone in the experiment of the brothers Weber may in a sense be said to have gone out into all the lands of physiology.

Let me now turn your attention to an experiment made a few years later. This is also an experiment made on a living animal, and whatever good may have come out of that to which it has given rise must be reckoned as the fruit of an experiment.

In 1851, Claude Bernard made known that division of the cervical sympathetic led to a widening of the blood-vessels and a warming of the ear and other parts of the head and neck. This was the beginning of what may rightly be called the great vaso-motor knowledge. It may be true that more than a hundred years before, in 1727, Du Petit had observed much the same thing, but nothing came out of it; the germinal time had not yet arrived. It may be true that other observers since Du Petit had divided the cervical sympathetic, and noted the effects; but these had their attention directed chiefly to changes in the pupil. It may be true that Brown-Séquard and Waller, a few months before Bernard himself was able to do so, supplied the complement to the original experiment by showing that stimulation of the peripheral part of the divided sympathetic constricted the blood-vessels and reduced the temperature. All this may be true, but there remains the fact that with Bernard's experiment the new light began; that experiment marks the beginning of our vaso-motor knowledge.

I have already spoken of the prolonged discussions, which just before the date of Huxley's studentship were taking place, touching the question whether or no the blood-vessels were muscular and contractile. That question had, meanwhile, been definitely settled by Henle's demonstration, in 1840, that the tissue in the middle coats of arteries really consisted in part of muscular tissue, of the kind known henceforwards as plain muscular tissue. But for some years no use was made of this discovery in the direction of explaining the intervention of the nervous system in the government of the circulation. That began with Bernard's experiment.

It would, I venture to think, be sheer waste of your time and mine, if I were to attempt to labour the theme of the large share in our total physiological knowledge which is now taken up by the vaso-motor system and all that belongs to it, and of the extent to which the physiology of that system has woven itself into pathological doctrines, and helped medical practice. I would simply ask the lecturer on physiology in what stress he would find himself if he were forbidden in his teaching to say a word which would imply that the calibre of the blood-vessels was influenced by the contraction of their walls through nervous influence; or ask the student how often, in an examination of to-day, he would have to sit seeking inspiration by biting his pen, or staring at the roof, if he too, in his answers, could never refer to vaso-motor actions. Whatever part of physiology we touch, be it the work done by a muscle, be it the various kinds of secretive labour, be it that maintenance of bodily temperature which is a condition of bodily activity, be it the keeping of the brain's well-being in the midst of the hydrostatic vicissitudes to which daily life subjects it—in all these, as in many others, we find vaso-motor factors intervening; and, to say nothing of the share taken by these in the great general pathological conditions of inflammation and fever, they also have to be taken account of by the doctor in studying the disordered physiological processes which constitute disease, whatever be the tissue affected by the morbid conditions. Take away from the physiological and pathological doctrines of to-day all that is meant by the word vaso-motor, and those doctrines would be left for the most part a muddled unintelligible mass. To so great an extent as that which Bernard's experiment began entered into our modern views.

It was Bernard's good fortune, but deserved good fortune, to announce, almost at the same time, two fundamental discoveries. For I venture to claim for his discovery of the formation of glycogen in the liver, briefly indicated in 1850, more fully expounded in 1851, an importance only second, if second, to that of the experiment with which we have just been dealing.

To judge of its importance we must look at it from more than one point of view.

At the time when Huxley was sitting at the feet of Wharton Jones, the teaching of the Schools was largely governed by the view that the animal organism, in contradistinction to the vegetable organism, was essentially destructive in its chemical actions, possessing no power in itself of synthetic construction. It is true that the possible synthesis of organic compounds special to the animal body had long before, in 1828, been shown by Wöhler's artificial formation of urea. It is true also that Huber, in the case of bees, and Liebig, in the case of cows, had already shown that wax and fat must be in part manufactured out of something that was not fat. The conclusions, however, of these observers were at best somewhat distant inferences from statistical data; and, in any case, had not as yet made much way in the direction of general acceptance. But Bernard's experiment was in the form of an ocular demonstration. The glycogen which had been formed in the liver could be extracted, could be seen, handled, and, if need be, tasted, a result adequate to convince even a physiological Thomas. We may claim for Bernard's glycogen discovery that as the first realistic proof of the synthetic powers of the animal organism it did much to establish a truth, which succeeding observations have only served to confirm and extend, namely, that the animal, no less than the vegetable organism, possesses synthetic powers, and that the want of prominence of these in the ordinary work of the animal body is to be attributed to economic reasons, and not to absence, or even scantiness of power.

But there is another aspect from which the discovery must be viewed.

At the time of which we are speaking, physiologists were still, as they had been of old, largely under the influence of a somewhat mechanical conception of the body as a collection of organs, each of which had its special use or function, the unity of the body being maintained by the mutual adaptation of the constituent organs. This was further developed into the view that when a use of an organ had been satisfactorily made out, when a function had been made clear, all that remained to be done, in the way of research, was simply to inquire how far and in what ways the performance of that function was influenced by changes in the rest of the body, or by external circumstances. It was acknowledged, for instance, on all hands that the function of the liver was to secrete bile, and physiologists in general were content to look forward for future discoveries which should throw light on the exact nature of the mechanism of the secretion, and on why the liver secreted now more, now less bile, and to these alone without expecting anything else.

Bernard's discovery that the liver not only secreted bile, but manufactured glycogen, fell on physiologists like a bolt from the blue. The knowledge that the same hepatic cell was engaged both in secreting bile and manufacturing glycogen, and that the sugar or other products of digestion were carried from the intestine, not straight to the tissues which they were destined, in any case, ultimately to nourish, but to the liver, there to undergo transformation and await some future fate, marked the beginning of a new way of looking at the problems of nutrition. It was recognised that these became less simple, more complex than they had formerly seemed; but the very complexity gave hope of possible solutions. It was seen that as the blood swept in the blood stream through the several tissues, it might undergo profound changes without any visible outward token, such as that of the appearance of secretion in the duct of a gland, or of the contraction of a muscle, might undergo changes which could only be demonstrated by differences in the composition or properties of the blood as it came to or left this or that tissue. The technical difficulties of the analysis of blood prevented any immediate marked steps in the way of advance, and attempts to establish, in respect to any particular tissue, the changes which the blood underwent in it, by inference from the results of experimental interference, met with difficulties of another but no less serious kind. Hence the world had to wait some little time before the new idea which Bernard's discovery had started bore important or striking fruit. Yet it was not very long before it was seen that the hepatic cell had heavy duties touching the metabolic changes of proteid, as well as a carbohydrate material; that it, and not the kidney alone, had to do with urea as well as sugar, and the difficulties, which physiologists in the early half of this century must have keenly felt, how to reconcile the bald task of secreting bile, which alone technical physiology allotted to the liver, with the overweening importance which not only popular experience, but more exact clinical study, could not but attach to that organ, began to steal away. A little later on, exact experimental inquiry converted into certainty the suspicions which clinical study had raised, that the blood in streaming through the thyroid gland underwent changes of supreme importance to the nutrition of the tissues of the body at large. Still, a little later, the Bernardian idea, if I may so venture to call it, doubling, so to speak, on itself, led to the discovery that the mysteries of the fate of sugar in the body were not lodged in the liver alone, but might be traced to the pancreas. It was seen that as the blood streaming through the liver worked on sugar, besides secreting bile, so the pancreas, besides secreting its marvellous omnipotent juice, also influenced, though in a different way, the career of sugar in the body, that the disease we call diabetes was or might be in some way connected with the pancreas no less than with the liver. I need not go on to speak of recent researches on the supra-renal capsules or of other organs. It is enough to note that one of the most promising lines of inquiry at the present day is that relating to the changes of which I am speaking, sometimes known under the name of "internal secretion." Every year, nay, almost every month, brings up some new light as to the details of the great chemical fight which the blood is carrying on in all the tissues of the body—it may be perhaps to-morrow that we shall learn of some work of a kind wholly unexpected which is carried out by that great Malpighian layer of the skin which wraps round our whole frame. In any case, the line of inquiry is one of the most fruitful of those of the present day. I may add too, I think, that it is one which has been of the greatest direct use to mankind, and promises still more. It is true that Bernard's discovery of glycogen, and perhaps especially the diabetic puncture, raised hopes which have not been fulfilled. Not to-day, any more than forty years ago, it is in our power wholly to remove the disease which we call diabetes. But short of complete mastery, how great is our power now compared with then. And when we remember that the pancreatic relations of sugar are far from being worked out, and that such knowledge as physiologists already possess has not yet made much way in clinical study, we may look forward to marked progress possibly in no very distant time.

Further, if there be any truth in what I have insisted upon—that the value of a discovery is to be measured not only by its immediate application, theoretical and practical, but also by the worth of the idea which it embodies and to which it gives life; and if it be true, as I have suggested, that by the genesis of ideas the discovery of glycogen is mother of all our knowledge of internal secretion, in its widest sense, of the work of the thyroid and other like bodies, then the good to suffering mankind which may be laid to the door of Bernard's initial experiment is great indeed.

The next result to which I will call your attention is again an experiment, and once more an experiment on a living animal. In 1850, Augustus Waller described, in the *Philosophical Transactions*, the histological changes which division of the hypoglossal and glosso-pharyngeal nerves in the frog produced in the fibres of the distal portions of the nerves, and shortly afterwards developed this initial result into the more general view of the dependence of the nutrition of a nerve-fibre on its continuity with a cell in the central nervous system, or in the case of afferent fibres, in the ganglion of the posterior root.

This discovery was at the time, and has since continued to be of value as a contribution to physiological ideas; it had its share in promoting the progress which, though slight, is still a progress, of our understanding the obscure influences which the part of a cell enclosing the mysterious nucleus exercises over all the rest of the cell. And perhaps even to-day the theoretical value of that degeneration of nerve-fibres, the knowledge of which we owe to Waller, is not adequately appreciated, and the lead which it gives not followed out as it might be. In spite of all we know, we are too much apt to fall back on the conception that, when no nervous impulse is travelling along a nerve-fibre, the nerve-fibre is in a state of motionless quiescence, and that a nervous impulse, when it does come, sweeps over the fibre as a wave sweeps over a placid lake. But the Wallerian degeneration gives such a view the lie direct. When we reflect that the finely-balanced molecular condition, which itself is nothing more than the falsely seeming quiescence of an equilibrium of opposing motions, in the ultimate fibrils of the nerve-twigs, in the ultimate phalanx of the finger, by which we touch and get to know the world without us, is dependent on what is going on around the nucleus of a cell, or the nuclei of some cells in the ganglion or ganglia of certain upper spinal nerves, so that if the continuity of the axis cylinder process be anywhere broken, the figure of the molecular dance changes at once, and riot takes the place of order. When we reflect on this, it is clear, I say, that between the molecules of the ultimate fibrils branching in the Malpighian layer of the ball of the finger, and the molecules within the immediate grasp of the nucleus of the cell from which those fibrils start, there must be ever-passing thrills—thrills, it is true, of so gentle a kind, that no physical instrument we as yet possess can give us warning of them, so gentle, that compared with them, the wave, which carries what we call a nervous impulse, must appear a roaring avalanche, but still thrills the token of continued movement. And of such gentle impalpable unnoticed thrills, we must in the future take full account, if we are ever to sound the real depths of nervous actions.

It is not, however, as a contribution to theoretical conceptions, but rather as a method, that the results of Waller have so far had their chief effect on the progress of physiology and medicine. And I have chosen it as a thing to dwell on, because it seems to me a striking instance of the value of a method merely judged as a method, and, further, because the value of its use illustrates my theme that the success of any one scientific effort is contingent on the converging aid of other efforts. For some time, it is true —for years, in fact—the Wallerian method was employed solely or chiefly in what, without reproach, may be called the smaller problems of physiology; it settled many topographical questions, it cleared our views as to the distribution of afferent and efferent fibres; it seemed to add or replace a few stones here and there in the growing building, but it did not greatly change the whole edifice. After a while, however, it met with two helpmates—the one sooner, the other later—and, by means of the three together, we have gained, and are still gaining such additions to our knowledge of the ways in which the central nervous system works out the acts which make up our real life, as to constitute perhaps the most striking progress in the physiology of our time. A wholly new chapter of nervous physiology has, through them, been opened up.

The one colleague is to be found in the experiments of Fritz and Hitzig; and of Ferrier, again, experiments on living animals—experiments which, by demonstrating the existence of definite paths for the play of nervous impulses within the central nervous system, opened up paths for the play of new ideas concerning the working of that system. I say "demonstrating the existence of definite paths," for this—and not the topographical recognition of so many centres of hypothetical nature—is the solid outcome of experiments on local stimulation of the cerebral cortex. Views come and go as to what is happening when the current is flitting to and fro between two electrodes placed on a particular spot of the Rolandic area; the solid ground on which each view strives to establish itself is, that the particular spot is joined by definite nervous paths to particular peripheral parts. I say "demonstrating the existence of particular paths," but what would have been the demonstrative value of the experiments of stimulation, or of removal, by themselves, without the anatomical support furnished by the Wallerian method? And I may justly include within the Wallerian method, not the mere tracking out the degenerated fibre by the simple means at Waller's own disposal, but such finer, surer search as is afforded by the later help given by the newer development of the staining technique.

They who have the widest experience of experiments on living animals are the first to own that in a region of delicate complexity like that of the central nervous system, the interpretation of the results of any experimental interference may be, and generally is, in the absence of aid from other sources, a matter of extremest difficulty, one in which the observer, trusting to the experiment alone, may easily be led astray. I need not labour the question what would have been the value of the mere effects of stimulating or even of removal of parts of the cerebral cortex, and whither would they have led us, had the experimental results not been supported and their interpretation guided by the teachings of the Wallerian method. It is not too much to say that the experiments of Ferrier and his peers, brilliant as they were, might have remained barren, useful only as isolated bits of knowledge, or might even have led us astray, had they not been complemented by anatomical facts. They have not remained barren, and they have not led us astray. The Wallerian method picked out from the tangle of nerve fibres making up the white matter of the brain and spinal cord, the pyramidal tract running from the Rolandic area, to the origins of all the motor roots, even of the lowest, and so joining hands with the experiment, made it clear that whatever might be the exact nature of the events taking place in a particular spot of the cortex of that area, that spot was, by the definite paths of particular nerve fibres, put in connection with definite skeletal muscles. The pyramidal tract was further shown to be merely one—an important one, it is true—but still merely one of a large class. So it is that the experimental results and the Wallerian results, not merely in that Rolandic area where the results of experiment take on the grosser form of readily appreciated interference with movements, but in other regions where other finer, more occult manifestations of nervous and psychical actions have to be dealt with are, it may be slowly, but yet surely, resolving that which seemed to be a hopeless tangle of interweaving and interlacing nerve fibres and cells, into an orderly arrangement of which the key is seen to be that each nerve filament is a path of impulses coming from some spot—it may be from near, it may be from afar—where events are taking place, and carrying the issue of those events to some other spot, there to give rise to events having some other issue.

But a third factor was wanting to forward our insight into this orderly arrangement, and especially by again affording an anatomical basis to open the way towards explaining what was the order of events in the spots or centres, as we call them, in which the filaments began or ended, and what was the mechanism of the change of events. This, I venture to think, we may find in the special histological method which, however much its usefulness, has been enhanced by its subsequent development in the hands of Cayal, Kölliker, and others, as well as by the coincident methyl-blue method we owe to Golgi. The final word has not yet been said as to the exact meaning and value of the black silver pictures which that method places before us; but this, at least, may be asserted that by means of them the progress of our knowledge of the histological constitution of the central nervous system has within the last few years made strides of a most remarkable kind. It may be that those pictures are in some of their features misleading, it may be that the terminal arborisation, and their lack of continuity with the material of the structures which they grasp, does not afford an adequate explanation of the change in the nature of the nervous impulses which takes place at the relays of which the arborisations seem the token; it may be, indeed it is probable, that we have yet much to learn on these points. But notwithstanding this, it must still be said that, by the help of this method, our knowledge of how the fibres run, where they begin and where they end within the brain and spinal cord, has advanced, and is advancing in a manner which, to one who looks back to the days when Huxley was studying within these walls, seems little short of marvellous.

Let me once more repeat, the value of this silver method is not an intrinsic one, it has its worth because it fits in with other methods, it is available on account of what is known apart from it. I imagine that if in 1842 Huxley, at Wharton Jones' suggestion, had invented the silver method, it would have remained unknown and unused. The time for it had not then come. The full fruition which it has borne, and is bearing in our day has come to it, because it works hand in hand with the two other methods, of which I have spoken—the Wallerian and the experimental method.

It is these three working together which have brought forth what I may venture to call the wonders which we have seen in our days, and I cannot but think that what we have seen is but an earnest of that which is to come. In no branch of physiology is the outlook more promising, even in the immediate future, than in that of the central nervous system. But surely I do it wrong to call it merely a branch of physiology. It is true that if we judge it by even the advanced knowledge of to-day, it takes up but a small part of the whole teaching of the science; but when we come to know about it that which we are to know, all the rest of physiology will shrink into a mere appendage of it, and the teacher of the future will hurry over all that to which to-day we devote so much of the year's course, in order that he may enter into the real and dominant part.

There is no need for me to expound in detail how the knowledge gained by the three methods, of which I have been speaking, in laying bare the secrets of nervous diseases, and opening up the way for successful treatment accurate and trustworthy prognosis, has helped onward the art of medicine. Even the younger among us must be impressed when he compares what we know to-day of the diseases of the nervous system with what we know, I will not say fifty, but even twenty, nay even ten, years ago. Do not for a moment suppose that I am attempting to maintain that the great clinical progress which has taken place, has resulted from the direct, immediate application to the bedside of laboratory work, or that I wish to use this to exalt the physiological horn. I would desire to take a higher and broader standpoint, namely this, that the close relations and mutual interdependence of laboratory physiology and that bedside physiology, which we sometimes call pathology, and the necessity of both for the medical art, are nowhere more clearly shown than by the history of our recent advance in a knowledge of the nervous system as a whole. In this, when we strive to follow out the genesis of the new truths, it is almost impossible to trace out that which has come from the laboratory and that from the hospital ward, so closely have the two worked together; an idea started at the bedside has again and again been extended, shaped or corrected by experimental results, and been brought back in increased fruitfulness to the bedside. On the other hand, a new observation, which, had it been confined to the laboratory, would have remained barren and without result, has no less often proved in the hands of the physician the key to clinical problems, the unlocking of which has in turn opened up new physiological ideas.

And, though the scope of these Huxley lectures is to deal with the relations of the sciences to the medical art, I shall, I trust, be pardoned if I turn aside to point out that this swelling knowledge of how nerve-cell and nerve-fibre play their parts in bringing about the complex work done by man's nervous system, is not narrowed to the relief of those sufferings which come to humanity in the sick room. Mankind suffers, much more deeply, much more widely, through misdirected activities of the nervous system, the meddling with which lies outside the immediate calling of the doctor. Yet every doctor, I may say every thoughtful man, cannot but recognise that the distinction between a so-called physical and a so-called moral cause is often a shadowy and indistinct one, and that certainly so-called moral results are often the outcome, more or less direct, of so-called physical events. I venture to say that he who realises how strong a grip the physiologist and the physician, working hand in hand, are laying on the secret workings of the nervous system, who realises how step by step the two are seeing their way to understand the chain of events issuing in that sheaf of nervous impulses which is the instrument of what we call a voluntary act, must have hopes that that knowledge will ere long give man power over the issue of those impulses, to an extent of which we have at present no idea. Not the mere mending of a broken brain, but the education, development and guidance of cerebral powers, by the light of a knowledge of cerebral processes, is the office in the—we hope—not far future of the physiology of the times to come.

I might bring before you other illustrations of the theme which I have in hand. I could, I think, show you that the very greatest of all recent advances in our art, that based on our knowledge of the ways and works of minute organisms, has come about because several independent gains of science met, in the fulness of time, and linked themselves together. But my time is spent.

I should be very loth, however, and you, I am sure, would not wish that I should end this first Huxley lecture, without some word as to what the great man, whose name the lectures bear, had to do with the progress, on some points of which I have touched. He had an influence, I think a very great one, upon that progress, though his influence, as is natural, bore most on the progress in this country.

The condition and prospects of physiology in great Britain at the present moment are, I venture to think, save and except the needless bonds which the legislature has placed upon it, better and brighter than they ever have been before. At one time, perhaps, it might have been said that physiology was for the most part being made in Germany; for, in spite of the fact that some of the greatest and most pregnant ideas in physiology have sprung from the English brain, it must be confessed that in the more ordinary researches the output in England has at times not been commensurate with her activities of other kinds. But that cannot be said now. The English physiological work of to-day is, both in quantity and quality, at least equal to that of other nations, having respect to English resources and opportunities. Part of this is probably due to that activity which is the natural response to the stimulus of obstacles; the whip of the antivivisectionists has defeated its own end. But it is also in part due to the influence of Huxley.

That influence was two-fold, direct and indirect. I need not remind you that not only when he sat on the benches of Charing Cross Hospital, but all his life-long afterwards, Huxley was at heart a physiologist. Physiology, the beauty of which Wharton Jones made known to him, was his first love. That Morphology, which circumstances led him to espouse, was but a second love; and though his affection for it grew with long-continued daily communion, and he proved a faithful husband, devoting himself with steadfast energy to her to whom he had been joined, his heart went back again, and especially in the early days, to the love which was not to be his. What he did for morphology may perhaps give us a measure of what he might have done for physiology, had his early hopes been realised. As it was, he could show his leanings chiefly by helping those who were following the career denied to himself. Unable to put his own hand to the plough, he was ever ready to help others, whom fate had brought to that plough, especially us younger ones, to keep the furrow straight. And if I venture to say that the little which he who is now speaking to you has been able to do, is chiefly the result of Huxley's influence and help, it is because that only illustrates what he was doing at many times and in many ways.

His indirect influence was perhaps greater even than his direct.

The man of science, conscious of his own strength, or rather of the strength of that of which he is the instrument, is too often apt to underrate the weight and importance of public opinion, of that which the world at large thinks of his work and ways. Huxley, who had in him the making of a sagacious statesman, never fell into this mistake. Though he felt as keenly as any one the worthlessness of popular judgment upon the value of any one scientific achievement, or as to the right or wrong of any one scientific utterance, he recognised the importance of securing towards science and scientific efforts in general a right attitude of that popular opinion which is, after all, the ultimate appeal in all mundane affairs.

And much of his activity was directed to this end. The time which seemed to some wasted, he looked upon as well spent, when it was used for the purpose of making the people at large understand the worth and reach of science. No part of science did he more constantly and fervently preach to the common folk, than that part which we call physiology. His little work on physiology was written with this view, among others, that by helping to spread a sound knowledge of what physiology was, among the young of all classes, he was preparing the way for a just appreciation among the public of what were the aims of physiology, and how necessary was the due encouragement of it.

And if, as I believe to be the case, physiology stands far higher in public opinion, and if its just ambitions are more clearly appreciated than they were fifty years ago, that is in large measure due to Huxley's words and acts. I have not forgotten that he was one of a Commission whose labours issued in the forging of those chains to which I have referred; but knowing something of Commissions, and bearing in mind what were the views of men of high influence and position at that time, I tremble to think of what might have been the fate of physiology if a wise hand had not made the best of adverse things.

One aspect of Huxley's relations to science deserves, perhaps, special comment. On nothing did he insist, perhaps, more strongly than on the conception that great as are the material benefits which accrue from science, greater still is the intellectual and moral good which it brings to man; and part of his zeal for physiology was based on the conviction that great as is the help which, as the basis of the knowledge of disease, and its applications to the healing art, it offers to suffering humanity in its pains and ills, still greater is the promise which it gives of clearing up the dark problems of human nature, and laying down rules for human conduct. No token, in these present days, is more striking or more mournful than that note of pessimism which is sounded by so many men of letters, in our own land, no less than in others, who, knowing nothing of, take no heed of the ways and aims of science. Cast adrift from old moorings, such men toss about in darkness on the waves of despair. There was no such note from Huxley. He had marked the limits of human knowledge, and had been led to doubt things about which other men are sure, but he never doubted in the worth and growing power of science, and, with a justified optimism, looked forward with confident hope to its being man's help and guide in the days to come.

ZOOLOGY AT THE BRITISH ASSOCIATION.

SECTION D met on the afternoon of Thursday, September 17, in the Zoology theatre, University College, the President's address having been given in the Arts theatre in the morning. The principal feature of the Section was the large number of discussions, these occupying the mornings throughout the meeting, and two of them being in conjunction with the sections of Physiology and Botany (I and K).

Thursday, September 17.—The first paper was by Mr. R T. Günther, on Roman oyster culture. The author's facts were drawn both from classical writings and, also, from pictorial representations on ancient vases. Some of the latter gave fairly intelligible pictures of the processes used, and showed what means were adopted for attaching and preserving the young oysters. He believed that there was good evidence to show that ropes and other similar substances were used for the former purpose.

Mr. Walter Garstang read a preliminary communication on the utility of specific characters in the Brachyurous Decapods, referring, however, particularly to the crabs. The object of the inquiry was to ascertain the essential meaning of the denticulation of the edges of the frontal area of the carapace. The author first drew attention to the fact that in some crabs the respiratory current was from before backwards—the reverse of that believed to have applied in all cases by Milne Edwards. This led to the inference that the function of the serration was to filter off solid matter from the water entering the branchial chamber from before. This conclusion was supported by the fact that the denticulations were characteristic essentially of the burrowing crabs, in which, as being buried in sand, it was important that some filtration of the respiratory current should be provided for, in order to prevent the otherwise inevitable blocking up of the respiratory chamber by foreign matter. When, owing to the habits of a crab, the serrations should *à priori* be absent, they were, in fact, not found. The conclusion was that the denticulations on the frontal area of the carapace were functionally correlated with the flow of the respiratory current from before backwards, thus confirming the theory of natural selection by proving the utility of specific characters which would otherwise have been concluded as useless. In the discussion which followed, Prof. W. F. R. Weldon remarked how necessary it was to exercise caution before concluding that any specific characters were useless. Dr. C. H. Hurst drew attention to the anterior position of the renal aperture in Crustacea, which he thought implied a forward, and not a backward, flow of the respiratory current. He suggested that this might also be a diagnostic character in determining the direction of the flow, as it would doubtless be essential that the products of the nephridia should be carried away from the gills. The Rev. T. R. R. Stebbing stated that in many Crustacea the position of the renal aperture could not possibly be correlated with the flow of the respiratory current, and that, therefore, its forward position had no significance in this connection.

The following reports of Committees were then presented: (1) "The Zoology of the Sandwich Islands," by Prof. A. Newton. Three papers had been published as the result of the work of the Committee's collector, on the Orthoptera, Slugs, and Earthworms. Prof. Newton emphasised the importance of proceeding with the work as rapidly as possible, as the fauna in some parts was being partially destroyed by animals introduced into the islands. (2) "The Occupation of a Table at the Marine Biological Laboratory, Plymouth." The report dealt with the work of Mr. George Brebner on the Algæ of the Plymouth district. (3) "Zoology and Botany of the West India Islands." Five papers had been published as the result of the work of the Committee, and other papers were in hand on the Isopod Crustacea and Diptera. During the year much work had been done on the flora of the islands, and the Committee required a grant of £50 to aid in working out the collections already made. (4) "The Biological Investigation of Oceanic Islands." The Rev. T. S. Lea and the Rev. Canon Tristram spoke of the important work being done by the Committee, and of the necessity of its being done at once.

Friday, September 18.—The morning was devoted to a discussion on Neo-Lamarckism, Prof. C. Lloyd Morgan having undertaken to open it. Prof. Morgan, after referring at some length to the precise positions taken up by Neo-Lamarckians and Neo-Darwinians, and the difficulty of disproving either belief, said what was wanted was a really crucial case. If they could in some way exclude natural selection in some cases, and allow it to act in others, they would obtain such crucial cases; and if the habit was equally transmitted, whether natural selection was present or not, that would present an exceedingly strong point for the transmissionist. The nearest approach to such a crucial case, from his own observations, was the reaction of young birds to water. There did not seem to be any instinctive reaction to the sight of water, even on the part of ducklings. But as soon as the bill incidentally touched the water, the appropriate drinking response was at once called forth. Why did not a chick or duckling respond instinctively to the sight of something so essential to its existence as water? He had very little doubt that, under natural conditions, the mother bird taught them to drink, and this implied that the presence of the mother, as a source of instruction, shielded the young from the incidence of natural selection. Now, though the mother could lead her young chicks to peck at the water, she could not suggest the appropriate drinking response. In this matter she did not shield them from the incidence of natural selection, and those which failed to respond to the stimulus would die of thirst, and be eliminated. Thus, when natural selection was excluded, the habit had not become congenitally linked with a visual stimulus, and where natural selection was in operation the habit had become congenitally linked with a touch or taste stimulus. Prof. Morgan concluded by saying that it was the consideration of such cases as these that had induced him to take up the Weismannian position. In the discussion, Prof. C. S. Minot said he could not defend the Neo-Lamarckian position, as the facts of embryology directly negatived it. Prof. W. F. R. Weldon deplored a metaphysical treatment of the subject. These matters could be proved or disproved by observation, and what they wanted were facts and not polemics. Mr. F. A. Bather thought the Ammonites afforded at least some proof of the Neo-Lamarckian doctrine. Prof. Hartog, Dr. Hurst, Mr. McLachlan, Sir Henry Howorth, the Rev. T. R. R. Stebbing, and Mr. E. W. McBride also took part in the discussion.

In the afternoon Mr. F. Enock read a paper on "The Life-History of the Tiger Beetle (*Cicindela campestris*)." The burrows of the larva, and how they were made, were described, and the method which the larva adopted in order to catch its prey was illustrated by some beautiful coloured lantern slides. The various changes undergone by the larva to produce the perfect insect were then outlined, and the method of egg-laying also described. It was pointed out how perfectly the larva was adapted to the conditions under which it lived, both in its anatomy and habits. The author emphasised the importance of studies of this character, and remarked that only very few of the life-histories of these animals were known in any detail. After this paper the Section adjourned to the loan museum, where a series of slides of Eozoön was exhibited by Sir William Dawson. The latter explained that upon the question of the organic or inorganic character of these remains he had still an open mind.

Mr. J. W. Woodall gave an account of Dannevig's Flodevigen hatchery for salt-water fish. This hatchery was erected in 1883, with the object of ascertaining whether it was possible to produce large numbers of the fry of the better class of salt-water fish at a reasonable cost, the decrease in the fisheries, especially the cod-fishing, having at that time been greatly felt. Difficulties had all along been a considerable bar to the work, but between the years 1890–96, 1203 millions of fry had been hatched at a cost of 0·65*d.* per 1000, whilst last season the cost had been one-third of a penny per 1000, with, it was thought, a still further chance of lowering the expenses. The hatchery cost £800, and the annual expenses were about £500. It was claimed that as a result of the operations of this hatchery the cod was rapidly increasing on the south coast of Norway, and especially at those points where the fry had been liberated. In the discussion on this paper, Dr. J. Hjort thought that ocean currents caused either the destruction or the removal of a great number of the fry liberated by the hatchery. The fry could not be kept sufficiently long in the hatchery at a reasonable expense, and if liberated before, the destruction of them must be very great. Prof. W. F. R. Weldon concurred. He wanted more evidence as to the survival of an appreciable number of the young animals when cast into the sea. Mr. Walter Garstang thought it possible that the supply of food fishes had been increased by means of fish-hatcheries. The following two papers were then read:—"On the necessity for a Fresh-water Zoological Station," by Mr. Scourfield; and "On Improvements in Trawling Apparatus," by Mr. J. H. Maclure.

The report on the "Index generum et specierum animalium" was presented by Mr. F. A. Bather. This index is being compiled by Mr. C. D. Sherborn, who has already been occupied for five years on the work, and has registered over 135,000 species. A small grant was asked for in order that this important work might be more quickly proceeded with. Mr. Bather quoted cases that had occurred in his own individual experience showing the importance of the work being carried on by Mr. Sherborn, and the Rev. T. R. R. Stebbing agreed that for systematists a complete index would be of the greatest assistance, and was becoming year by year more indispensable. The following reports were also submitted:—"On the Coccidæ of Ceylon," "On the transmission of specimens by post," and "On zoological bibliography and publication."

Saturday, September 19.—The report and discussion on the migration of birds, presented by Mr. John Cordeaux, occupied the whole of the morning, and attracted the largest meeting of the Section. The report, prepared by Mr. W. Eagle Clarke, of the Museum of Science and Art, Edinburgh, is a digest of the results obtained concerning the migration of birds as observed at lighthouses and lightships of the British Islands during the years 1880–1887 inclusive. The contents of the report had reference only to the facts obtained by the Committee, and its object was not to solve problems connected with the causes of the phenomena, the evolution of the migratory instinct, or other purely theoretical aspects of the general subject. The digest having been made from at least one hundred thousand records, it was claimed that a sufficient basis had been obtained on which a sound and proper conception of many of the phenomena of the migration of British birds could be based. The migration was treated by Mr. Clarke under the three heads of Geographical, Seasonal, and Meteorological, and a very valuable collection of facts is detailed under each section. Prof. A. Newton opened the discussion by pointing out that Mr. Clarke's labours were not by any means at an end, it being his intention to work out the migration of each species of British bird in as much detail as his data allowed. He (Prof. Newton) could wish that their observations were even more numerous, as they were still very far indeed from having exhausted the facts. Rev. Canon Tristram gave the results of some personal observations tending to show that during the day the birds flew nearer the surface and were guided by sight, whilst flying at a higher altitude during night migrations, when the difficulties of direction were evidently greater. Mr. R. M. Barrington did not think that the wind had much influence on migration. Dr. Hewetson and the Rev. E. P. Knubley also took part in the discussion.

Monday, September 21.—The morning was occupied, in conjunction with Section I, in hearing Dr. Gaskell's presidential address on the ancestry of the vertebrates, the discussion on the latter, requested by Dr. Gaskell, but unusual, taking place in the afternoon. Prof. W. F. R. Weldon, after criticising several special points in Dr. Gaskell's address, said that the great difficulty was the substitution of one alimentary canal for another. If this had been done in the way that had been suggested, they would have expected that vertebrate ontogeny would show some evidence of it; but in no vertebrate was the pharynx formed by the coalescence in the mid-ventral line of a series of buds representing arachnid appendages. He was also not all impressed by the so-called thyroid of the Scorpion. It was easy to find such clusters of cells in many animals. Prof. C. S. Minot could not follow Dr. Gaskell with regard to the central nervous system. The formation of a tube was altogether secondary, and the central nervous system must be described as being originally solid. Further, the origin of both the epithelium lining the neural canal, and the surrounding nervous material, was the same, and this would not be the case if Dr. Gaskell's hypothesis were correct. He therefore differed from Prof. Weldon, who had seen no special difficulty in this part of Dr. Gaskell's address. Mr. E. W. McBride pointed out that if the vertebrate alimentary canal was phylogenetically more recent than its nervous system, ontogeny would of necessity bear out Dr. Gaskell's conclusions. This, however, it did not do. The alimentary canal was always formed first, and the nervous system afterwards. Mr. McBride further, in maintaining that the invertebrate and vertebrate alimentary canals were homologous, stated that in the Decapod *Lucifer*, in which segmentation was not affected by yolk, the formation of the alimentary canal was essentially the same as in vertebrates. He maintained that this objection was absolutely fatal to Dr. Gaskell's theory. Mr. Walter Garstang said that two alternative theories of vertebrate ancestry had been mentioned by Dr. Gaskell, but there was also another which required respectful consideration. That was that the vertebrate nervous system had been formed by the coalescence of lateral cords. He maintained that there was considerable evidence in favour of this. Mr. W. E. Hoyle thought that Dr. Gaskell had been misled by the superficial resemblances of adults, and had not attached enough importance to the early stages. Mr. F. A. Bather stated that palæontology afforded no evidence for Dr. Gaskell's theory. It was very extraordinary, if the vertebrates had been preceded by a series of *Limulus*-like animals having a skeleton of the most imperishable substance known, that absolutely no traces should have been left of these animals in the fossiliferous rocks. Dr. H. Gadow, Prof. A. M. Paterson, and Prof. S. J. Hickson, also took part in the discussion.

As having some bearing on the above discussion, Dr. R. H. Traquair gave an account, illustrated by the original specimens, diagrams, and a model, of the remarkable fossil *Palæospondylus Gunni*. He insisted on its Cyclostome affinities, and expressed his belief that Dr. Bashford Dean's pectoral fin did not belong to the fossil at all. He had examined hundreds of specimens, and had seen no traces of it.

Tuesday, September 22.—The Section was occupied in the morning, in conjunction with Section K, with a discussion on the cell theory, an account of which will appear in the report of the Botany Section. Prof. C. S. Minot read a paper "on the theory of panplasm." He agreed with Bütschli in regarding protoplasm as a mixture of two fluids, similar in nature to an emulsion of oil and water. There was no evidence to show that vital functions were localised in small particles, and that each particle in itself was a unit of living material, and with a number of other such particles went to constitute the protoplasm of a single cell. He supposed that all the materials of the cell by their interaction produced living protoplasm, and that therefore the particles were mutually dependent. Hence the name panplasm. Prof. E. Zacharias thought that the study of living protoplasm was one which would produce valuable results, and had been too much neglected. He did not think protoplasm had a fibrillar structure, and such statements usually rested on an insecure basis of fact. Prof. M. M. Hartog then read a communication on the "relation of multiple cell-division to bipartition at the limit of growth," in which Herbert Spencer's explanation of bipartition was criticised and a new view expounded.

In the afternoon Mr. E. W. McBride opened with a paper on "the value of the morphological method in zoology." He stated that for some time back a distrust of the morphological method of studying evolution had been growing up amongst zoologists, and several alternative methods had been proposed. All of these, however, had their drawbacks. The reason of the discontent with the morphological method was that it proved too much, and the most contradictory conclusions were to be drawn from the same premises. Several suggestions were offered as to better ways of dealing with morphological facts. It was a gratuitous assumption that similarity in broad outlines of structures which were adaptive indicated descent from the same species. Structural resemblance indicated not primarily identity of ancestry, but similarity of past environment; and there might be all degrees in this similarity, both in extent and duration. Such a conclusion was tacitly admitted by systematists who made the basis of their system minute and apparently unimportant peculiarities of external form, colour, or arrangement of similar organs. It was, however, the origin and history of adaptations which interested the morphologist, and his task must be not primarily to draw up genealogical trees, but to correlate adaptations as far as possible to the external conditions which had caused them. Mr. F. A. Bather largely followed the conclusions of Mr. McBride. A great deal of misconception had arisen on account of general conclusions having been drawn from the study of specialised types. As an instance of this he cited the case of the Crinoid *Antedon*, which was a most specialised form, and yet had done duty for a primitive type. Morphologists should be more careful in the selection of their types if they wanted to base general conclusions on their results. Prof. F. Y. Edgeworth then read a paper upon the habits of wasps, showing how statistical methods could be utilised with success in the study of the migrations and other movements of animals such as wasps and other insects.

The following business concluded the proceedings for the day: Prof. C. S. Minot read a paper on the morphology of the olfactory lobe; a report by Mr. J. E. S. Moore was presented on the fauna of the African lakes; Prof. M. M. Hartog read a paper on the Morphology of the Rotifera and the Trochophore larva; and a letter was read by Prof. A. Newton from Dr. Stirling, on *Genyornis Newtoni*, an extinct Ratite bird from Australia allied to the Emu, but with leg-bones like those of the Moa, supposed to belong to the order *Megistanes*.

Wednesday, September 23.—The first paper of the final meeting of the Section was by Mr. A. T. Masterman on "*Phoronis*, the earliest ancestor of the Vertebrates." Mr. Masterman described two diverticula of the gut in the Actinotrocha larva, which he concluded from their structure represented a double notochord. He hence proposed a new group, to be called the Diplochordata. Hence the supposed relationship of *Phoronis* to the primitive vertebrate was confirmed. Mr. E. W. McBride said that there was such a strong tendency to discover ancestors for the Vertebrata, that great caution should be exercised before needlessly adding to the list. He thought that a double notochord was too great a demand upon their credulity, although Mr. Masterman's diverticula might function as a notochord.

Prof. W. A. Herdman then read a report on the Zoology, Botany, and Geology of the Irish Sea (illustrated by the lantern). A very interesting account was given of the work done by the members of the Liverpool Marine Biology Committee and other naturalists, and slides were shown of the Laboratory at Port Erin and its surroundings. The Committee were doing a useful work, and a work which was very far from being complete. The Rev. T. R. R. Stebbing spoke of the admirable faunistic work being done by the members of the Committee, and thought that they were to be congratulated on their report. Mr. W. E. Hoyle thought the results obtained by Prof. Herdman and his colleagues had an important bearing upon questions of general oceanography, and it was to be hoped, therefore, that the work of the Committee would not cease. Prof. Johann Walther testified to the admirable work that had been done in British seas during the last fifty years. This work, which was so important to marine biologists and oceanographists, had been initiated by Edward Forbes, and continued by Prof. Herdman, whom he regarded as Forbes' natural successor. Dr. Hjort and Mr. A. O. Walker also took part in the discussion.

Mr. Masterman read a further paper on "Some Effects of Pelagic Spawning on the Life-Histories of Marine Fishes," in which he maintained that pelagic spawning was more primitive than littoral. This explained many well-known facts in the migration of fishes. Dr. W. B. Benham then read a short paper on the structure of the genital glands of *Apus*, which, he asserted, could not be described as an hermaphrodite. He had recently made some observations on the reproductive organ of a male *Apus*, and showed diagrams of the spermatogenesis. The specimen had not been well preserved, but, except in this respect, he believed he was the first to study the testis of *Apus* according to modern methods. After some remarks by Prof. Hartog, the meeting concluded with a paper on the life-history of the Haddock, by Prof. W. C. McIntosh, communicated by Mr. Masterman.

MECHANICS AT THE BRITISH ASSOCIATION.

THE meetings in Section G—that devoted to mechanical science—at the recent Liverpool meeting of the British Association were generally well attended, and, on the whole, the proceedings compared not unfavourably with those of recent years. But only qualified praise can be given, as for long "G" has fallen short of its vocation. We look back to past times, to the days of Rankine and Froude, when the Section was more constant to its true mission, and sigh over later records. Mechanical science, though only applied science, *is* science; and though the Section must be utilitarian, it need not be a penny-readings or a means of trade advertisement. We think that any one acquainted with the proceedings of later years will agree that both the latter elements have been too much in evidence. With regard to the penny-readings or popular-lecture side of the question, we had more than one example during the recent meeting. There were some most interesting lectures and discourses, illustrated by equally interesting lantern slides, but they could hardly be classed as scientific They were just admirable penny-readings—nothing more.

With regard to the second undesirable feature to which reference has been made, we feel we are on delicate ground. A man having made an invention of a useful nature, and translated it into a machine or a process, naturally wishes to bring it prominently before the world for financial reasons. A cheap and efficacious method of doing so, is by reading a paper before a technical society. That is a perfectly legitimate proceeding, and is thoroughly recognised by the various societies and institutions of this nature; for however much they may strive to pose as scientific, they know well enough they are no more than technical, and founded on commercial bases. Were it not for the hope of advertisement—it is best to call spades, spades—not one half the papers read before engineering societies would ever be written; but that is no reproach to the societies. They do most admirable and useful work, without which the country would not make the progress it does. The morality of technical societies is, as it should be—"If a man has anything new and instructive to tell us, he is entitled to his advertisement, short of introducing purely commercial details."

But the British Association for the Advancement of Science should take higher ground than this, even in Section G. It should not allow a paper to be read on a trade article at the same time that illustrated catalogues and price-lists of the article are distributed amongst the audience. Neither should it allow its officials to distribute among the audience touting circulars asking members present to subscribe to a public company which bears evidence of being a trade association.

There were, however, at the recent meeting one or two good examples of the work Section G ought to do. Mr. Beaumont's paper may be taken. It was an endeavour to account for a somewhat obscure, but well-known, engineering phenomenon by the aid of scientific or physical data. The author may have been wrong in his conclusions, even in his premises, as some speakers during the discussion suggested, but at any rate he had a proper conception of what a British Association paper should be, and some regard for the dignity of the Section. Mr. Wheeler's report on tidal influences was also a piece of good work, which will be useful to those making scientific investigation of the subject; and there were one or two other items in the programme of a character proper to the Section; but we will proceed to details.

This year Sir Douglas Fox was President of the Section, and on Thursday, September 17, the proceedings were opened by his inaugural address. This we have already printed in full. The first paper taken was by Mr. G. F. Lyster, and was on the "Physical and Engineering Features of the River Mersey, and the Port of Liverpool." This was not a contribution of the popular-lecture order, because it was not popular, and it was certainly not a "mechanical science" paper. It could hardly be called an engineering paper, excepting in respect of it being a catalogue of engineering works. It was very long, and its author read it to the bitter end. It is to be printed in the *Proceedings*.

Mr. Beaumont's paper, to which reference has already been made, came next. The following is an abstract of this contribution. The author was of opinion that the failure of any rail, however perfect, is chiefly a question of the number and weight of the trains passing over it. The result of the rolling of the heavily loaded wheels of engines and vehicles is that a gradual compression of the upper part of the rails takes place, and this produces internal stresses which are cumulative and reach great magnitude. That which takes place in the material of a rail head under the action of very heavy rolling loads at high speed, is precisely that which is purposely brought into use every day in ironworks. The effect is, however, obscured by the slowness of the growth and transmission of the forces which are ultimately destructive. It was pointed out, further, that when a piece of iron or steel is subjected to pressures exceeding the limit of elastic compression, by a rolling or hammering action, or by both these combined, the result is spreading of the material and general change of the dimensions. This is equally the case with a plate hammered or rolled on one side while resting on a flat surface. In these cases, the hammering or rolling work done upon the surfaces tends to compress the material beneath it, but being nearly incompressible and unchangeable in density, the material flows, and change of form results. Generally the material thus changed in form suffers permanently no greater stresses than those within its elastic limit of compression or extension. When, however, the material is not free to flow or to change its form in the directions in which the stresses set up would act, the effect of continued work done on the surface is the growth of compressive stress exceeding elastic resistance.

In the case of railway rails the freedom for the flow of the material is very limited. Hardening of the surface takes place, and destructive compression of the surface material is set up. If the material be cast iron, the destructive compression causes crumbling of the superficial parts, and the consequent relief of the material immediately below it from stress beyond that of elastic compression; but when the material is that of steel rails, the stress accumulates, the upper part near the surface being under intense compression, differentiating from a maximum at the surface. This compression gives rise to molecular stresses, analogous to those which, on the compression side or inner curve of a bar bent on itself, originate traverse flaws on that side. This condition of compression exists along the whole length of a rail, so that when its magnitude is sufficient to originate crumbling or minute flaws, any unusual impact stress, or a stress in the direction opposite to that brought about by the usual rolling load, the rail may break into two or into numerous pieces. Stresses originating in the same manner explain the fracture of railway tyres as described fully by the author in the "Proceedings of the Institution of Civil Engineers," 1876, vol. xlvii.

A good discussion followed the reading of this paper. It was opened by Prof. Unwin, who took a somewhat different view from that of the author. The latter, the speaker pointed out, attributed the ultimate failure of a rail to the number of trains which passed over it; but his, the speaker's, experience told him that there was most danger in new rails. Again, according to the paper, one would expect soft rails to give way more quickly than hard ones; but here again experience negatived the assumption. A defect in the paper, however, was that the author had neglected to consider the composition of the rail, and this was the governing factor. Other points to which the speaker made reference were fatigue, and the change from a homogeneous to a non-homogeneous material. It was well known that a rail might be used in one way for a considerable time, but that when turned over it would be liable to break, and the speaker further illustrated his point by the analogy of a punched hole; but in this case one part was put in tension, so that annealing removed the defect. These things, however, did not solve the problem, and in his opinion work put upon the rail in use strengthened rather than weakened it; but the initial condition had far more influence than the rolling of wheels.

Mr. Johnson, of the Midland Railway, had strips taken from various parts of broken rails, and did not find difference in composition. Fractures had undoubtedly occurred through rails being made from a "piped" ingot—that is to say, one in which the whole of the head and pipe, in which the impurities collect, had not been sufficiently removed.

Dr. Anderson, Director-General of Ordnance Factories, pointed out the similarity of the effects described by the author in the case of rails and those observed in big guns which had been much fired. In the bore of guns a large number of minute cracks were discovered, and the deterioration of the A tube of a gun was due to the powder gases breaking out the squares. Here there was ultimate compression and release of pressure, as in a rail.

The President had examined rails which had failed, by the microscope, and had noticed the minute cracks referred to. He would point out that rails often gave way at the ends, and this bore out the theory that defects were caused by insufficient cropping leaving the "pipe." He pointed out that a crack once started might easily be extended by lower strains than would be required to start it; just as a tear or rent commenced on a piece of paper would be easily continued. Prof. Hele-Shaw pointed out that if the rail were planed, the latter defect would be removed. It may be worth putting on record that the late Mr. Spooner, chief engineer to the Festiniog Mountain Railway, who used to turn his rails at times, once told us that an unplaned rail was more liable to break than one which had been planed. Of course the object of planing was not undertaken with a view to prevent breakage, but to take out the dents from the chairs; but the result stated had been observed.

In replying to the discussion Mr. Beaumont stated, in regard to Prof. Unwin's remarks, that he, the speaker, had submitted facts, and not speculations, to explain breakage of rails. The Board of Trade inquiry on the subject had proved that there was a good deal to learn, and he had mainly put forward his paper with a view to raising discussion. He could produce figures tending to show that sometimes the hardest rails lasted longest, though when they did give way they were apt to break into a greater number of pieces. Undoubtedly the rail must be of good steel—not impure—to do its work properly; that, he had concluded, was a foregone conclusion. The question of the rail forming a continuous girder affected the matter of end breakage, and in this respect the influence of the modern stiff fish-plate had to be considered.

On the following day, Friday, September 18, the proceedings opened with the report of the Sectional Committee appointed to consider the effect of wind and atmospheric pressure on the tides. The members of the Committee were Profs. L. F. Vernon-Harcourt and W. C. Unwin, Messrs. G. F. Deacon and W. H. Wheeler. The latter acted as secretary, and drew up the report. Information had been obtained from various ports in England. It was concluded, firstly, that the tides are influenced both by atmospheric pressure and by the wind to an extent which considerably affects their height; secondly, that the height of about one-fourth the tides is affected by wind; thirdly, that the atmospheric pressure affecting the tides operates over so wide an area, that the local indications given by the barometer at any particular spot do not afford any trustworthy guide as to the effect on the tide of that particular port; fourthly, that although, so far as the average results go, there can be traced a direct connection between the force and direction of the wind and the variation in the height of the tides, yet there is so much discrepancy in the average results when applied to individual tides that no satisfactory formula can be established for indicating the amount of variation in the height of the tide due to any given force of wind; fifthly, the results given in the tables attached to the report relating to atmospheric pressure indicate that the effect of this is greater than has generally been allowed, a variation of ½ inch from the average pressure causing a variation of 15 inches in the height of the tides. As the report will be printed in full in the published *Proceedings* of the Association, we have thought it unnecessary to give more than the conclusions reached, but the whole is well worthy of the attention of those interested in the subject. Mr. Wheeler is well known as a trustworthy and diligent student of this question, and his professional status enables him to obtain information from a wide source.

A brief report on the calibration of instruments in engineering laboratories was the next item in the programme. Copies of this report, so far as we could ascertain, were not distributed.

Mr. Barry's lecture on the Tower Bridge followed, and attracted a large audience. It was interesting, and the lantern slides were well managed. Mr. J. Parry followed with a long paper of the historical-record order, dealing with the Liverpool Waterworks. The last item on this day was a contribution by Mr. A. J. Maginnis, entitled "The present position of the British North Atlantic Mail Service." It was a good paper in its way, but its way was not quite that of mechanical science; indeed, the author dwelt rather on the economics of ocean service than on its engineering aspects. Some instructive figures in regard to coal consumption were given, it being stated, among other things, that the *Campania* burns 20 tons of coal per hour. To drive an improved *Campania*, 700 feet long and 74 feet wide, 23 to 24 knots would require 46,000 indicated horse-power, supposing existing practice were followed. The cost of the vessel would be £800,000.

The next sitting of Section G was held on the Monday following, September 21, and was, according to custom, devoted to electrical engineering. The first business was the reading of a report by the Committee on small screw gauges. This report has been looked forward to with interest for some time. It will be printed in full in the *Proceedings* of the Association. Mr. Preece (the chairman of the Committee) drew up the report. After giving details of the method of work followed by the Committee, and referring to the labours of others in the same field, the report proceeded to notice a method, suggested by Colonel Watkin, for making very accurate comparison. There would be thrown, side by side on a screen, photographic images of the screw to be examined, and of the standard with which would be compared, together with the image of a scale which might be divided to one ten-thousandth of an inch. The images of these three objects being so close to one another, a comparison to a very high degree of accuracy could be made. Mr. Price, a member of the Committee, submitted a microscopical method, in which the screw to be measured is attached to the stage of the microscope, the traversing slide of which is provided with a vernier scale, while a vernier cross-hair in the eye-piece forms the index of the instrument. When the microscope has been adjusted for clear focus, the screw is traversed across the field until the cross-hair intersects the thread of the screw at the desired point. The traversing screw of the slide is then turned until the corresponding point of the next thread is intersected by the cross-hair, and the reading of the vernier on the scale gives the measurement of the pitch with great accuracy.

The Committee decided that gauges for ordinary workshop use would be best tested, as regards pitch and form of thread, by a template or "comb," the accuracy of which would be verified by the photographic method. External dimensions could be obtained by micrometer gauge, and the internal diameter, or core, by a gauge suggested by Mr. A. Stroh, a member of the Committee, the details of which have yet to be worked out. The Committee failed to discover any very trustworthy method of testing a female standard gauge. Naturally a mathematically accurate male gauge cannot be screwed into a mathematically accurate female gauge of like dimensions, but the variation should not exceed a "good fit." A table prepared by Prof. Le Neve Foster, dealing with this subject, was added as an appendix. The details given refer to works managers' gauges. Those used by the workman or foreman need not possess the mathematical accuracy of the standard gauges. For full details of this useful report, we must refer our readers to the published *Proceedings* of the Association, where it will be found printed together with the illustrations necessary for its full comprehension.

A long paper, by Mr. W. H. Preece, on "The Tests of Glow-Lamps," followed. It comprised the results of a very large number of tests, the details being given in diagrams handed round at the meeting. It would be impossible here to give even a summary of the results of tests, for the lamps tried were supplied by a number of makers, and varied according to the numerous conditions of trial. Some of the cheaper lamps gave results not at all in accordance with what would be expected from them if the statements of the makers were to be taken as guides. The experiments tended to prove that in continuous lighting for 1000 hours the candle-power fell about 30 per cent., and the watts per candle-power rose about 28 per cent. Lamps for installation work of about $3\frac{3}{4}$ watts per candle-power, burning from seven to nine hours per day, behave, as regards life and efficiency, about the same as when giving continuous illumination; but high efficiency lamps deteriorate more quickly. Good 100 to 105 volt 16-candle-power lamps, taking $3\frac{3}{4}$ volts per candle, should stand a gradual increase of pressure of direct current up to 225 or 280 volts in $3\frac{1}{2}$ minutes before the filament breaks. When the pressure is regularly raised in $2\frac{1}{2}$ minutes to 170 volts, and afterwards re-tested at ordinary voltage, the candle-power should not be less than 14·4, nor higher than 17·6, while the watts per candle should not exceed 4. The author also suggested in his paper a quick and ready way of satisfactorily judging the quality of lamps. To obtain this end the voltage of several lamps was gradually run up for each lamp singly at a uniform rate until the filaments broke. At the moment of rupture the voltage, current, and time of running up were noted. Before increasing the normal voltage the current was measured and the resistance calculated. The average breaking voltage of the filaments was found to be 230, and the time of running up was $3\frac{1}{2}$ to $4\frac{1}{2}$ minutes. Mr. Preece also gave a standard specification for glow-lamps which he had drawn up for use in the Post Office.

Prof. Ayrton, in the discussion on the paper, said that it was to be expected, as noticed by the author, that lamps which gave at first less than their nominal candle-power would last longer, as they were worked at a lower pressure. He pointed out that certain figures given by the author as to the cost of illumination by glow-lamps showed electricity to be dearer than gas burnt in an Argand burner, and very largely in excess of gas burnt by the Welsbach system. It had been noted that the illuminating power had gone up in certain glow-lamps, though the voltage remained constant. That was an interesting point, and one difficult to account for. It had been thought that the improvement was due to improved vacuum, but this was hardly to be believed, and he suggested it might arise from improvement of the filament during use. Prof. Fleming referred to the unsatisfactory nature of the standard candle, and also to the importance of personal error in photometric investigation. Mr. Swan approved of the short test suggested by the author, and pointed out that the length of life of a lamp depended upon constancy of pressure, a thing often much to be desired in central stations. Mr. Preece, in replying, said that though gas might be cheaper per hour than electricity, yet the ease with which the latter was turned on and off led to less light being wasted, and therefore an equality of cost was produced. If, however, local authorities would use electricity for tram propulsion, the cost of electric light per hour would be brought greatly below that of gas, in consequence of equalisation of the load factor.

A paper by Mr. S. B. Cotterell, on the "Liverpool Overhead Railway," was next read, in which the author described the engineering and other details of this construction. Mr. E. W. Anderson also read a paper on "Electric Cranes," the author expressing opinions favourable to the application of electric power for lifting heavy weights. Papers on "Hysteresis," by Prof. Fleming, and on "Street Lighting," by Mr. Walker, were also read.

The Section had a long sitting on the Tuesday of the meeting, but some of the papers were not of great importance. The first taken was by Captain Jaques, of New York, and was on "Armour and Ordnance." It was devoted largely to showing the great superiority of the United States over the rest of the world in the field. A spherical balanced valve was described by its inventor, Mr. J. Casey. It is an engineer's fitting involving an application of known principle. Prof. Hele-Shaw next gave an interesting description of certain instructional apparatus used in the Walker Engineering Laboratory, including Froude's dynamometer break, the speaker giving an excellent popular description of this ingenious appliance. A good discussion on the subject of technical education followed, in which, among others, Profs. Perry, Beare, Schröter (of Munich), Ritter (of Zürich), Merrivale, and Hele-Shaw took part. The opinion was expressed that the course of instruction proposed for the establishments known as Polytechnics, which have been so plentifully started in this country of late, is too ambitious, and the apparatus so complicated that evening students have not either time or ability to take advantage of it. Papers on "Colour Printing," by Mr. T. Cond, and on "Expanded Metal," a species of network made by slitting metallic sheets, were also read. The last sitting of the meeting was held on Wednesday, September 23. A paper by Mr. J. Bell described a system of wreck-raising, which the author and others had worked out. Lifting pontoons are employed in the ordinary way, but in place of the rise of tide being used to raise the wreck from the bottom, winches are adopted. The details of construction were illustrated by models. Finally a lecture on "Motor Carriages," by Mr. Sennett, was given. It was of an entirely popular character.

This brought the proceedings in Section G to a close.

ANTHROPOLOGY AT THE BRITISH ASSOCIATION.

AFTER the President's Address (*cf.* NATURE, October 1, p. 527), the remainder of Monday was devoted to papers dealing with Prehistoric Archæology. Mr. Seton Karr exhibited specimens and photographs of the palæolithic implements which he had collected in Somaliland; these form an interesting link in the series of finds extending from India to Britain. It is well known that ordinary palæolithic implements of the river-gravel type are wanting in Ireland; but Mr. W. J. Knowles contends that the older flint implements he has found in the north-east of Ireland belong to this epoch, and that some bear striæ which "have been pronounced to be glacial." A discussion arose in the afternoon, in connection with some photographs of dolmens in Brittany exhibited by Prof. Herdman, as to the age of such structures. Prof. Boyd Dawkins maintained that they belonged to the Bronze Age, while Dr. Montelius, Dr. Garson, and others recognise that they are essentially Neolithic.

The proceedings on Friday commenced with speeches by the President, Sir William Turner, Prof. A. Macalister, and Mr. Brabrook, in commemoration of the centenary of the birth of Prof. A. Retzius, who was the originator of some of the modern methods of craniology, and who did a great deal to stimulate anthropological science in Scandinavia. Mr. A. W. Moore and Dr. J. Beddoe read a joint communication on the physical anthropology of the Isle of Man as analysed from the "Description Book of the Royal Manx Fencibles," in which are contained particulars of 1112 Manxmen enrolled between 1803 and 1810. Speaking roughly, there are three ethnic districts: in the north-west the stature is highest, but dark hair and eyes are least prevalent; dark hair, coupled with grey eyes, is most abundant in the somewhat infertile parishes of Maughold and Lonan; while dark eyes are comparatively frequent in the central parishes where the Scandio-Gaelic stock is probably less pure. The chief paper of the day was an elaborate study of the Trinil femur, by Dr. D. Hepburn. The femora of various savage and civilised races were compared with that of *Pithecanthropus erectus*. The author dealt especially with the popliteal space, and followed the methods adopted by Manouvrier; in fact, this paper was largely an extension of the French investigator's careful study. He noted the absence of symmetry between two femora of the same individual, and exhibited an Australian femur with the same popliteal measurements and index as those of the Trinil femur. The condyles of the Trinil femur are human, and not simian. The author stating that the distinguishing features of the Trinil femur are found singly and in conjunction on human femora, with sufficient frequency to enable them to rank as human characters; and thus its features do not entitle it to the distinction of a separate genus, but it is a true human femur, although of very ancient date. This paper led to a good discussion, in which several speakers took part Prof. Boyd Dawkins did not regard the Trinil find to be of Pliocene Age. Dr. Garson believed that these specimens belonged to a new genus and species of the Hominidæ. Sir John Evans reserved his judgment; this he summed up in his felicitous manner in the following rhyme, which was not, however, uttered in public:—

> About three things pray let us have the truth—
> The skull, the thigh bone, and the Trinil tooth.
> The thigh is human, does the skull belong?
> Is the tooth human, or is Dubois wrong?
> But, after all, where were the relics found—
> Was it in ancient or in modern ground?

Dr. Garson exhibited a lantern slide of an outline figure which embodied the mean proportions of the head, body, and limbs of the members of the British Association who have been measured at the various meetings. In the afternoon Mr. F. T. Elworthy gave a fully illustrated lantern demonstration of the survivals in modern South Italian charms from very ancient Pagan times.

On Saturday morning Mr. Brabrook presented the Report of the Ethnographical Survey of Great Britain and Ireland, which showed that the survey is steadily progressing. There were several appendices to this report, the two most important being Dr. W. Gregor's, on Galloway folk-lore, and one by Mr. Gomme, on the method of determining the value of folk-lore as ethnological data. This was a solid and novel contribution to the right apprehension of folk-lore, which deserves to be widely read; it consisted mainly of an analysis of fire rites and ceremonies in the British Islands. Among these numerous customs nine are reckoned as constituent elements, two are suggestive of the original culture stage (the use of stone implements), while eight are divergent elements. If lines are drawn on a map connecting the localities where more or fewer of these customs occur, an "ethnological test-figure" is arrived at for each country. These fire customs are held to be of Aryan origin. Mr. Gomme has previously stated reasons for considering water-worship customs to be non-Aryan in origin; to belong, therefore, to the pre-Celtic people of these islands, and the "ethnological test-figure" produced by mapping the occurrence of water customs, differs radically from that connected with fire customs.

Mr. C. H. Read urged the formation of an Ethnological Bureau for this country, analogous but not similar to the famous Bureau of Ethnology in the United States. He recognised that a certain amount of partial or isolated work was being done in India and elsewhere, but what was wanted was a uniform system of inquiry extending all over our possessions, and the collection and collation of the results in a central office. He recommended (1) that the reports should be systematised and on a uniform method, (2) that such work should be held to be part of the duties of the local Government officer, and consequently (3) the officer should obtain credit for such work when well done. In conclusion, he repeated, "a nation having under its Government or protection so many primitive or uncivilised races, as are now within the confines of the British Empire or upon its borders, is bound both by interest and policy to study and to put on record all facts connected with their history, beliefs, and manners and customs, the knowledge of such facts being, in the first place, essential to the maintenance of peaceful and friendly relations, and, in the second, of the highest interest to science." The proposition was warmly supported by Profs. Macalister, Boyd Dawkins, Haddon, and Sir John Evans. In his paper on "Anthropological opportunities in British New Guinea," Mr. S. H. Ray re-affirmed the danger of delay in investigating the anthropology of British New Guinea, and called attention to the opportunities which exist for successfully carrying it out. If anything is to be done, it should be done soon. Stress is laid upon languages as folk-lore; religious beliefs and practices and legal customs can only be thoroughly studied through the medium of the languages. We want to know the native's reason for his thought and practice, as the European often draws most erroneous conclusions from his own observations. The country is now quiet and safe, and facilities would doubtless be offered by the present enlightened administrator, Sir Wm. MacGregor. Prof. Haddon followed with an earnest appeal for the immediate investigation of the anthropology of all islands and other districts where the indigenous population is being exterminated or largely modified by the advent of the white man.

Monday morning was devoted to a discussion of the origin of the knowledge of copper and iron in Europe. This was led off by Mr. J. L. Myres, who indicated the part played by Cyprus and its relation to the trade routes of South-east Europe. Dr. J. H. Gladstone gave a series of analyses of prehistoric metal implements, which demonstrated a transition for the use of pure copper to the widely-spread bronze; various methods for hardening the copper were employed, such as the sub-oxides of copper and various natural alloys of copper with antimony and arsenic, but when the tin bronze was discovered it quickly superseded all the others. An interesting discussion followed, in which Dr. Munro maintained that there was no proof of a Copper Age in Europe, the copper implements being "starved" bronze, and only manufactured when the supply of tin ran short; but this view did not gain general support. Prof. Ridgeway read a paper on the starting-point of the Iron Age in Europe, in which he pointed out that Hallstatt, in Austria, is the only place in Europe where articles of iron are found gradually replacing those of the same kind made in bronze, and that within a very short distance of the Hallstatt cemetery lies one of the most famous iron mines of antiquity, Noreia. He suggested that the accidental finding of an outcrop of volcanic iron, such as that known in at least one place in Greenland, led to iron smelting; there is no need to suppose that meteoric stones first supplied man with that metal. This theory was adversely criticised by several speakers. Mr. Myres gave an abstract of Sergi's theory of a Mediterranean Race (this subject has already been referred to in our columns). It was a disappointment to many that Prof. Sergi was unable to fulfil his promise to be present and explain his views. Dr. Munro and Prof. Boyd Dawkins detailed at length the results of the recent excavations of the Lake Village of Glastonbury. A model of accurate archæological research was afforded by Dr. Stolpe, of Stockholm, in his account of the Vendel finds in Sweden. These boat graves ranged from a period of about 600 to 1000 A.D., and various modifications were noted during that period; numerous beautiful drawings and lantern slides of the bronze objects found were exhibited. Mr. R. A. S. Macalister gave an interesting account of a recent exploration he had made of a prehistoric settlement in Co. Kerry, which was illustrated by numerous lantern slides.

A great discussion on the early civilisation of the Mediterranean was opened on Tuesday morning, in a fine fighting speech by Prof. Ridgeway, entitled "Who produced the object called Mykenæan"? The genial Irishman made a brilliant onslaught on many generally recognised views. The credit of this civilisation belonged either to the Achæans or to the Pelasgians. The traditions of the Greeks themselves point to the latter. The age of Mykenæ is that of Bronze, that of Homer's Achæans is distinctly of Iron. Engraved gems are characteristic of Mykenæ, but these were unknown to Homer; but the converse is the case with fibulæ. The Mykenæans had a peculiar figure of 8 shield, no breastplate, no metal greaves, and they wore their hair in three locks behind; whilst the Achæans had round shields, bronze breastplates and greaves, and wore their hair flowing. There is no need to cut Homer to pieces to fit the Mykenæan Age; this culture is that of the Bronze Period and Pelasgian in origin, and was supplanted by the Iron Age, which was introduced by the Achæans into Greece. Prof. Petrie supported Prof. Ridgeway by adducing the argument of a continuity of artistic pre-eminence from Mykenæan times to the art of Pheidias in Attica, which was a Pelasgian settlement. Dr. Beddoe pointed out that the skull-form of Pericles and other noted Greeks was Pelasgian in character. Principal Rendall strongly supported the orthodox classical view of Reichel and Leaf, and scoffed at the "Pelasgian heresy." Dr. Munro, Sir John Evans, and Mr. Myres continued the discussion; the latter ingeniously showed how a round shield could be twisted into a figure of 8. The President also spoke, and Prof. Ridgeway replied to the criticisms; and so terminated one of the most lively and interesting debates that Section H has ever experienced. Dr. O. Montelius gave a characteristically careful and learned paper on pre-classical chronology in Greece and Italy, in which he distinguished four divisions of the Bronze Age in North and Central Italy, dating from 2100 to 1100 B.C., two Protetruscan Periods, from 1100 to 900 B.C., in Central Italy, and two Central Italian Etruscan Periods from 900 to 700 B.C. The forms of the implements, fibulæ, pottery, &c., that characterised these several periods were fully illustrated by lantern slides. He stated that the copper implements were made in the same shapes as those of the old stone implements. Prof. Petrie referred to a recent discovery of his own, in Egypt, of iron tools of such a character that they must have been made by a people long acquainted with iron; they were associated with an Assyrian helmet which can be dated about 670 B.C. This is the oldest known datable iron find. The beginning of the use of copper tools in the Mediterranean area was from 3500 to 3000 B.C. The President read a paper on pillar and tree worship in Mykenæan Greece, as illustrated by signets, on a gold ring from the early Mykenæan Period (about 1500 B.C.) a dual cult of a male and female divinity in their pillar shape is engraved. Other signets show deities as pillars and trees enclosed in small shrines; the cult of the fig-tree and the early sanctity of the dove were referred to; and attention was also drawn to the fact that pillar and tree worship of Mykenæan Greece is seen largely to survive in the rustic cult of classical Greece. Mr. G. Coffey gave a lucid account, illustrated by lantern slides, of the relation of the stone-carvings of the tumuli of New Grange, Dowth and Loughcrew to Scandinavian art. He has lately discovered in Dowth the representation of a boat identical with those inscribed on Swedish rocks; this is the first undoubted example found outside Scandinavia. Other new evidence was brought forward to substantiate his view of a direct borrowing of Norse motives, many of which in their turn had come into Scandinavia from the Mediterranean up the valley of the Danube, and round Hungary. Mr. Kermode concluded a long and very interesting day's work by describing a magnificent series of rubbings and drawings of Celtic and Scandinavian crosses from the Isle of Man. In a recent number (*cf.* NATURE, October 8, p. 547) we have referred to the appreciation by numerous members of the Association of Mr. Kermode's endeavours to preserve and record these beautiful and most interesting remains.

Prof. Flinders Petrie brought forward on Wednesday his scheme of an ethnological and archæological storehouse. Most of the speakers who followed admitted that more room was needed than most existing museums can possibly afford if large collections were to be preserved intact, and it was also recognised that long series of objects were necessary for scientific study. Certain details of his proposed museum were criticised, but Prof. Petrie thought that all these could be met.

An interesting paper on "The Duk-duk and other Customs as Forms of Expression of the Melanesian's Intellectual Life" was read by Graf von Pfeil. During his long stay in the Bismarck Archipelago he came to regard the natives' strong desire for physical and psychical seclusion as an explanation of some of their ceremonial customs. They still hate the white man, and distrust their fellow countrymen. The Duk-duk apparently serves to propitiate evil spirits and to levy blackmail on non-initiates. The Eineth and Marawot ceremonies were described for the first time; the former appears to be related to ancestor worship, and taboos are placed on various foods, actions and words. Little, however, is as yet known about this or the Marawot; the latter consists mainly of a dance on a high platform. The author urged the immediate importance of studying the habits of the Melanesians, owing to the change which is taking place. In the discussion which followed, Mr. Hartland and Prof. Haddon suggested that there was more behind these ceremonies than the author had yet discovered, but he was congratulated on approaching the subject from a psychological point of view. The Count maintained his view that the Melanesian was very largely actuated by mercenary motives.

Mr. F. T. Elworthy announced the very recent discovery of an Ancient British interment in Somersetshire, which led to a long discussion, the net result of which appeared to be that this was a burial, in a stone cist and with a decorated earthen vessel of the Neolithic type, of a man who, by his skull, undoubtedly belonged to the Bronze Race.

This session was one of the most successful of any meeting of Section H. Most of the papers maintained a high level, and the pre-arranged discussions proved an interesting and instructive feature. Numerous distinguished foreigners had expressed their intention of being present, but, unfortunately, only Drs. O. Montelius and H. Stolpe, and Prof. W. H. Goodyear actually arrived.

FORTHCOMING BOOKS OF SCIENCE.

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The autumn announcements of Messrs. W. and R. Chambers include:—"The Romance of Industry and Invention, comprising chapters on Iron and Steel, Big Guns, Evolution of the Cycle, Telegraph, &c.," edited by R. Cochrane; Dr. M'Kendrick's "Elementary Human Physiology"; Dr. Findlater's "Elementary Physiology," new edition, edited by D. Forsyth; three volumes of their "Elementary Science Readers."

Messrs. Baillière, Tindall, and Cox have almost ready:—"Applied Bacteriology," containing the latest methods of Research, by Messrs. Pearmain and Moor, illustrated; "Chemical Notes and Equations (Inorganic and Organic) for Students," by G. H. Gemmell; "Atlas of Practical Histology," by Prof. Arnold Brass, assisted by German Professors; authorised English translation by R. A. Young, illustrated.

Mr. J. C. Nimmo gives notice of:—"A History of British Birds," to which is added the author's Notes on their Classification and Geographical Distribution, by Henry Seebohm, in four volumes, illustrated (cheaper edition); "The Geographical Distribution of the Charadriidæ; or, the Plovers, Sandpipers, Snipes, and their Allies," by Henry Seebohm, in one volume, illustrated.

In Messrs. A. and C. Black's announcements we notice:—"A Dictionary of Birds," by Prof. Alfred Newton, F.R.S., part 4, or four parts in one volume; "Introduction to Structural Botany," by Dr. D. H. Scott, F.R.S., Part 2 (Flowerless Plants), illustrated; "Plea for a Simpler Life," by Dr. S. Keith, new edition.

Mr. Fisher Unwin calls attention to:—"Climbing Reminiscences of the Dolomites," by Leone Sinigaglia, translated by Miss Mary Alice Vialls, illustrated; "Rambles in Galloway," by Malcolm McL. Harper, illustrated; "On the Nile with a Camera," by Anthony Wilkin, illustrated.

Messrs. Seeley and Co., Ltd., will publish:—"The Life and Work of Charles Pritchard, D.D., F.R.S., late Savilian Professor of Astronomy in the University of Oxford," by his daughter, Ada Pritchard. A notice of his theological work by the Right Reverend the Lord Bishop of Worcester, and an account of his astronomical work by his successor, Prof. H. H. Turner. With a portrait.

Mr. Heinemann's announcements include:—"Genius and Degeneration: a Study in Psychology," by Dr. William Hirsch, translated from the second edition of the German work, "The New Africa: a Journey up the Chobé and down the Okovango Rivers," by Dr. Aurel Schulz and Augustus Hammar; "Timbuctoo the Mysterious," by Felix Dubois, illustrated.

Mr. W. F. Clay, Edinburgh, announces:—"Select Methods in Quantitative Analysis," by the late Prof. W. B. Cheever, third and enlarged edition, by F. C. Smith; "Researches on Molecular Dissymmetry of Natural Organic Products," by Louis Pasteur, 1860; "Early Papers on Chlorine," by Scheele, Berthollet, Gay-Lussac, Thénard, &c. (Alembic Club Reprints); "Handbook of the Diseases of the Eye," by Douglas Watson.

Messrs. Lawrence and Bullen, Ltd., give notice of:—"The Kafirs of the Hindu-Kush," by Sir George Robertson, K.C.S.I., illustrated; "Turkish Fairy Tales and Folk-Tales," collected by Dr. Ignacz Kunos, translated from the Hungarian version by R. Nisbet Bain, illustrated; "The Encyclopædia of Sport," edited by the Earl of Suffolk and Berkshire, Hedley Peek, and F. G. Aflalo, illustrated. in twenty parts.

The following works will be issued by Messrs. Smith, Elder, and Co.:—"Prehistoric Man and Beast," by Rev. H. N. Hutchinson, illustrated; "A Course of Practical Histology," by Prof. E. A. Schäfer, F.R.S., new edition, illustrated; "A Practical Treatise on Traumatic Separation of the Epiphyses," by J. Poland.

Messrs. Gurney and Jackson have in the press: "Parasitic Diseases of Poultry," by Mr. F. V. Theobald, which, besides giving descriptions and illustrations of the various external and internal parasites affecting fowls, &c., will contain suggestions as to the best means for their destruction and for the cure of the diseases caused by them.

Messrs. Grevel and Co. announce:—"The Elements of Electro Chemistry treated experimentally," by Dr. R. Lüpke, translated by M. M. Pattison Muir, illustrated; "The Religion of the Ancient Egyptians," by Dr. A. Wiedemann, illustrated; "The Care of Children in Sickness and Health," by Father Kneipp.

Mr. Charles Carrington, Paris, announces "Untrodden Fields of Anthropology," observations on the esoteric manners and customs of semi-civilised peoples, being a record by a French army surgeon of thirty years' experience in Asia, Africa, and America, in two vols.

In the list of Messrs. Kegan Paul and Co., Ltd., we notice:—"The Polar Aurora," by Alfred Angot, translated (International Scientific Series); "In the Land of the Bora; or, Camp Life and Sport in Dalmatia and the Herzegovina," by "Snaffle," illustrated.

Messrs. W. Blackwood and Sons announce;—"From Batum to Baghdad, *viâ* Tiflis, Tabriz, and Persian Kurdistan," by Walter B. Harris, with illustrations and two maps; "The Story of Mr. H——, the Herbalist," by Hugh Miller.

Messrs. Hodder and Stoughton promise:—"Beginnings of Life in the Dawn of Geological Time," by Sir J. W. Dawson, F.R.S.; "The Land of the Monuments: Notes of Egyptian Travel," by J. Pollard.

In the list of Messrs. Cassell and Co., Ltd., we find:—"Charles Darwin and the Theory of Natural Selection," by Prof. E. B. Poulton, F.R.S.; "Social England," edited by Dr. H. D. Traill (sixth and concluding volume).

Messrs. F. Warne and Co. will issue:—"The Royal Natural History," edited by R. Lydekker, F.R.S., vol. vi., illustrated; "Favourite Flowers of Garden and Greenhouse," edited by E. Step, vol. i., illustrated.

Messrs. Ward, Lock, and Co., Limited, will publish:—"Coil and Current, and the Triumphs of Electricity," by Henry Frith and Stepney Rawson.

Messrs. Gay and Bird's books include:—"Building Construction and Superintendence," by Dr. A. E. Kidder, part i., Masons' Work, illustrated.

Messrs. Archibald Constable and Co. give notice of "The Popular Religion and Folk-Lore of Northern India," by Wm. Crooke, 2 vols., illustrated.

Messrs. W. H. Allen and Co., Ltd., will add to their "Naturalists' Library":—"Game Birds," vol. ii.; "Butterflies," vol. iii.; "British Birds," vol. iv.

Mr. L. Upcott Gill announces a work on "Fruit Culture," a section of which will deal with the life-histories of the insect and other pests which affect fruit-growers.

The list of Mr. David Douglas, Edinburgh, includes:—"Among British Birds in their Nesting Haunts," by Oswin A. J. Lee, illustrated, part i.

Messrs. Methuen announce:—Vol. ii. of Prof. Petrie's "History of Egypt, from the Earliest Times to the Present Day."

Messrs. Blackie and Son, Ltd., will publish:—"Fuel and Refractory Materials," by Prof. A. Humboldt Sexton.

Messrs. G. Philip and Son promise:—"Geographical Manual of Africa," and "Certificate Atlas of Africa."

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

OXFORD.—Mr. Ernest Henry Stapleton, of the Grammar School, Bradford, has been elected to the Bristol Scholarship (thrown open *pro hac vice*) in chemistry and physics at St. John's College.

CAMBRIDGE.—Dr. W. H. Gaskell, F.R.S., has been appointed a member of the General Board of Studies. Mr. J. E. Gray, Scholar of King's, has been nominated to occupy the University's table at the Naples Zoological Station. Mr. James Henry Widdicombe, First Class Parts I. and II. Natural Sciences Tripos, 1891–92, has been elected to a Fellowship at Downing College.

Memorials signed by 2237 members of the Senate deprecating the admission of women to the membership or degrees of the University have been presented to the Vice-Chancellor. Of these 1369 would, however, approve the granting of some non-gremial title to women who pass a Tripos examination.

At the congregation on October 15, Mr. A. C. Dixon, of Trinity College, was admitted to the degree of Doctor in Science.

AT the Massachusetts Institute of Technology, four associate professors have been promoted to full professorships: Mr. Dwight Porter, in hydraulic engineering; Mr. Alfred E. Burton, in topographical engineering; Mr. C F. Allen, in railroad engineering; and Mr. Peter Schwamb, in mechanism. New assistant professors are: Mr. George H. Barton, in geology Mr. George G. Robbins, in civil engineering: and Mr. Joseph J. Skinner, in mathematics.

AT the recent opening of the collegiate year at Columbia University, 2100 students were enrolled, being the largest in the history of the College. Several gifts were announced, the most important being that of Mr. Charles C. Worthington, who, as a memorial to his father, the late Henry R. Worthington, will equip, with all necessary apparatus, a laboratory for the experimental study of the sciences of hydraulics and engineering as applied to hydraulics.

THE following appointments have recently been announced: Dr. E. Wernicke to be professor of hygiene at Berlin; Dr. H. Stuhr has been appointed assistant in the Anatomical Institute at Breslau, in succession to Dr. Endres; Dr. Andreas Obrzut, of Prague, to the chair of Anatomy at Lemberg; Dr. Chermak to be professor of comparative anatomy and embryology at Dorpat; Dr. Winkler, Professor of Chemistry, to be director of the School of Mines at Freiberg i. S; Dr. Godschmidt to be assistant professor of chemistry in the University of Heidelberg.

APPLICATIONS are invited for the Fellowship founded in 1894 by the Worshipful Company of Salters for the purpose of encouraging chemical research in the elucidation of pharmacological problems. The Fellowship is of the annual value of £100, and may be held for three consecutive years in the Pharmaceutical Society's laboratories. The regulations relating to the award may be obtained from the Clerk to the Company at Salters' Hall, but applications for the Fellowship must be sent to the Registrar of the Pharmaceutical Society before Saturday next, October 24.

AMONG the many evidences of the activity of the various Committees entrusted with the technical education of the country, one of the most pronounced is that afforded by the periodical reports which are issued by the different county authorities. In Essex the form assumed is that of *The Journal of the Essex Technical Laboratories*. In the twenty-second number, which lies before us, we have, in addition to a brief recital of the most interesting local educational events, accounts of certain manurial trials which have been made in that county, and of experiments conducted at the Brightlingsea Marine Biological Station. The larger portion of the booklet, taken up with reviews, and notes on lessons in elementary chemistry, might be curtailed with advantage.

IN opening a Technical and University Extension College, and a School of Science and Art, at Colchester on Tuesday, Lord Rosebery dwelt upon the urgent need of increased facilities for technical and commercial education in England. He remarked that Germany had long been twenty, thirty, or forty years ahead of us in technical education, and Switzerland was just as far advanced. Referring to the Germans, he said :—"They are an industrious nation ; they are, above all, a systematic nation ; they are a scientific nation, and whatever they take up, whether it be the arts of peace or the arts of war, they push them forward to the utmost possible perfection with that industry, that system, that science which is part of their character. Are we gaining upon the Germans? I believe, on the contrary, we are losing ground. The other day one of the greatest authorities on this subject went to Germany, being stirred up by what he had seen of alarm in the newspapers on the subject. He came back and told a friend of mine that he was absolutely appalled by the progress made in the last twenty years by the Germans in technical and commercial education as compared with what was going on in England. When I last spoke on this subject I made a modest proposal. It was, 'Cannot the Government order an inquiry to be made into the facts of this matter?' It would not cost as much as an ironclad. It would cost a very small sum indeed. I do not suppose it would cost a year's pay of the chief engineer of an ironclad. I believe it would be infinitely more useful. If necessary, three men like Lord Farrer, Sir Philip Magnus, and Sir Courtenay Boyle could without the slightest difficulty produce all the facts bearing on this subject without any expense whatever in the space of six months."

IN acknowledging the vote of thanks for his address, Lord Rosebery gave further instances of the extraordinary vigour with which Germany is pursuing the work of technical education. The *Times* reports him to have said :—"At this moment the German Government are about to present a Bill to Parliament for the federalising, if I may so describe it, of all the skilled workmen of the country. Each craft of skilled craftsmen is to be formed into a guild, and each group of guilds is to be formed into a central committee. These central committees are, again, to elect chambers of handicrafts, on the model of chambers of commerce, to reside at the principal centres of industry. Side by side with this organisation is to be an organisation of apprentices, who will have their direct representatives on the central chamber. These organisations are to be formed under the direct supervision of the Government. They are to carry out measures designed to promote the moral and material welfare of workmen, to arrest strikes, to establish and assist the development of trade by inspection and supervision of the methods of training skilled labour. Technical schools are to be established and supported, and the whole system of technical instruction, already so perfect in our opinion, thoroughly overhauled. The Government Bill insists on the constant interposition of officials, mainly with the object of preventing the guilds from narrowing the recruiting ground, which they are now rather inclined to do. The main principle underlying the Bill is to create responsible bodies who should advise the Government what measures should be adopted to promote the interest of the skilled producer, and should carry out under Government supervision such measures as the Government on their advice should recommend. Now, I do not think that we like so much Government supervision as that in England. But I only call attention to the fact as showing how Germany, in spite of her start of us, and in spite of the apparent perfection of her methods, is still straining every nerve and every muscle to organise her skilled labour in such a way as to defy the competition of the world." We need only remark now that long ago we urged the formation of a responsible council to advise on matters affecting the progress of science and industry. Had such a council been instituted, our industries would have developed along with the increase of scientific knowledge. The nation will soon, perhaps, begin to realise what it has lost by neglecting scientific experience and advice.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, October 12.—M. A. Chatin in the chair.—Elliptic elements of the Giacobini comet, by M. Perrotin.—On the extension of complete functions to an important problem in polynomials, by M. Émile Borel.—Cryoscopy of precision : reply to M. Raoult, by M. A. Ponsot. In the previous paper of M. Raoult, to which this is a reply, some remarks of M. Ponsot are severely criticised, and yet the substance of some of these remarks is adopted. In the present note the conclusion is drawn that there is now complete agreement as to the conditions theoretically necessary for obtaining the true freezing point of a solution ; but there are still some differences of opinion as to the best means of practically realising these conditions. The propositions put forward by M. Raoult in his last paper are criticised in detail.—Thermal studies on cyanamide, by M. Paul Lemoult. The cyanamide was prepared from thio-urea, and carefully purified from dicyandiamide. The molecular heat of combustion is 172 cal., and heat of formation 8·4 cal. ; the transformation into urea sets free 20·2 cal. The neutralisation with soda gave out 3·55 cal., but excess of soda gave rise to no further heat development.—Study of the sub-intestinal nervous system of the Orthoptera of the tribe *Mecopodinæ* (*Platyphyllum giganteum*), by M. L. Bordas. The great number of nervous centres, and the numerous branches of the sub-intestinal nervous system of *Platyphyllum giganteum* and allied species, show that this system must play an important part in the carrying on of the digestive processes. In this species there is a frontal ganglion, an œsophageal or hypocerebral ganglion, a pair of lateral œsophageal ganglia, and two intestinal ganglia, making six in all. The position of these, with their connecting nerves, is given in detail.

DIARY OF SOCIETIES.

THURSDAY, OCTOBER 22.

SOUTH LONDON ENTOMOLOGICAL AND NATURAL HISTORY SOCIETY, at 8.—Discussion on Tephrosia biundularia and T. crepuscularia : C. G. Barrett.—Paper on the same subject : J. W. Tutt.

TUESDAY, OCTOBER 27.

ROYAL PHOTOGRAPHIC SOCIETY, at 8.—Demonstration of Acetylene Apparatus for Portraiture and the Optical Lantern : C. Hoddle.

FRIDAY, OCTOBER 30.

PHYSICAL SOCIETY, at 5.—Special Meeting, after which, at an Ordinary Meeting—A Satisfactory Method of measuring Electrolytic Conductivity by means of Continuous Currents : Prof. W. Stroud and J. B. Henderson.—A Telemetrical Spherometer and Focometer : Prof. W. Stroud.—An Experimental Exhibition : R. Appleyard.

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