THURSDAY, AUGUST 4, 1887.

THE JUBILEE OF THE ELECTRIC TELEGRAPH.

IT is something to have lived to take part in an epoch-making event. Many monarchs have celebrated their jubilees: printing, steam, gas, have passed through this period in silence and disregard, but the first practical application of electricity has commemorated the fiftieth anniversary of its birth with an *éclat* and success that reflect the highest credit on the managers of the banquet who brought together such a distinguished gathering on July 27.

It is remarkable that of all who were present not one took part at the birth of the electric telegraph. The pioneers are gone, and their memory was silently toasted. Of those associated with Cooke, William Watkins, who carried out his early experiments, and who put up the first overhead wires between Paddington and Slough, alone remains, but the inexorable duties of the law compelled his attendance on a special jury at Exeter on the day when he ought to have been present at the Holborn Restaurant. No collaborateur of Wheatstone in his early work exists. John Greener, who had charge of the telegraph on Bidder's celebrated rope railway between Fenchurch Street and Blackwall in 1842, was there, and many like Henry Weaver and J. R. France can date their telegraphic career from the incorporation of the first telegraph company in 1846.

One of the most interesting features of the meeting was the gathering around Mr. Edwin Clark of his old lieutenants. Edwin Clark's reforms in the early days of telegraphy (1850–54) still bear fruit. The footprints on the sands of telegraphic time are nowhere so deeply impressed as on the ground traversed by Clark. His mode of insulation, his underground work, his instruments, his test-boxes, still remain a type of English telegraphy everywhere. His work was well carried out by his brother and successor, Latimer Clark, and it is continued even in the present day by his pupil, Preece.

The success of telegraphy in this country is due essentially to the superposition of scientific method on to the rude rules of practice. The rule-of-thumb principles of the early engineers were inoperative in telegraphy, for the exact laws of Ohm, Ampère, and Coulomb, the experimental skill of Faraday, Joule, and Grove, the mathematical genius of Helmholtz, Thomson, and Maxwell, have kept our electricians in the straight path, and prevented them from wandering in the wilds of guess-work and in the labyrinth of tentative troubles. It is impossible to say how much this influence has been reflective. The science of electricity has been indebted as much to practice as practice has been indebted to science. Submarine telegraphy chronicles no failure. The first Atlantic cable raised the curtain. The conditions were evident. Thomson stepped in, and all was light.

To telegraphy "all the world's a stage." The inventor has no nationality. Alongside of Wheatstone we find Morse and Siemens, Meyer, Hughes, and Edison, La Cour, Varley, Leclanché, and Minotto. This polyglottism is seen in the nomenclature of the units of measurement, ohm, farad, ampere, and coulomb, the only universal system of measurement, excepting that of time, extant.

Telegraphy, without which railway traffic would be impossible, has followed the growth of railways, and it has revolutionized the procedure of commerce. Hence the great commercial nations, England and the United States, show the greatest development of its progress.

One regretted to hear so little said about the great commercial spirits who set the ball a-rolling. John Pender, Cyrus Field, Tom Crampton, deserve all that was said of them, but where were Ricardo and Scudamore in England, Orton and Vanderbilt in America?

The story as told by the Postmaster-General reads like a romance of fairyland. The first five-needle instrument of Cooke and Wheatstone required five wires to transmit at most five words a minute: now five wires can transmit 2500 words in the same time.

We can pride ourselves in England on being in advance of all other nations not only in the development of the business of telegraphy, but also in the invention and perfection of apparatus. It is something to have in ten years increased the capacity of the wires for the transmission of messages *tenfold*, and to have done that without patent, or any reward but the consciousness of having done well. Government officials are unfortunately placed in this respect. It is improper to patent an invention developed in the discharge of duty, while they are singularly liable to be assailed by the daily Press for their supposed shortcomings. The work they do is only known by their own writings, when they are allowed to write; and even then they are subject to unfair and dubious criticism. The Press takes no trouble to find out what is done. The feeling is, "What good can come out of Nazareth?" Yet the introduction into the Post Office system of high-speed repeaters and of shunted condensers marks two epochs as successful, eventful, and meritorious as the introduction of duplex, of quadruplex, or of multiplex working. We were told that the rate of working between London and Dublin had gone up from 50 to 462 words a minute. *One* cable will do the work of *ten*. What has been the reward? We venture to say, *nothing*; and that the Lords of the Treasury are profoundly ignorant of the good work that is being done in the service over which they preside—work which they are just as likely to reward with a kick as with a half-penny.

The jubilee is now over, and we have every reason to feel proud that Mr. Raikes, the present Postmaster-General, Sir Lyon Playfair and Mr. Shaw-Lefevre, his predecessors, had such excellent tales to tell, and so gracefully assisted at so successful a gathering.

THE CLASSIFICATION OF ALGÆ.

Till Algernes Systematik. Nya bidrag af J. G. Agardh. (Femte afdelningen.) Transactions of the University of Lund, Tom. XXIII., 4to, pp. 180, 5 plates.

THE indefatigable Dr. Agardh has recently issued the fifth instalment of his work on the systematic classification of Algæ. Although it bears a Swedish title, the work is in Latin. The subject treated is the interesting group of the Siphoneæ.

Dr. Agardh mentions that but few observations have been made upon the fruit of the Siphoneæ; but the little that is known on this subject proves that great differences exist in the fructification of these Algæ. Thus the organs of reproduction in Vaucheria differ from those of Botrydium, while those of the latter vary from those of Bryopsis, Codium, Dasycladus, and Acetabularia. Of many other genera observations are deficient or in various respects uncertain and inconclusive. For interesting general remarks on the fructification of the Siphoneæ the reader is referred to page 10, and for special details to the observations on each genus and on the fruit of such of the species as are best known.

Setting aside the true characteristics of the fruit, it becomes a question by what characters of the structure the Siphoneæ are to be distinguished from the Conferveæ and Ulvaceæ, and their affinities determined. In Dr. Agardh's opinion these characters are to be found in the filaments or utricles of the frond, being tubular, and not, as in the Confervaceæ and Ulvaceæ, consisting of subdivided cells. Good examples of the former are afforded by Caulerpa and Valonia, whose fronds, roots, stems, branches, and ramuli, though distinct, consist of a single cell. These remarks are followed by observations on the comparative structure in different genera and their affinities with each other.

With the exception of Vaucheria and Botrydium, says Dr. Agardh, the Siphoneæ inhabit the sea. This is unquestionably the case as regards Botrydium, but it may be asked whether it be quite true as to Vaucheria, several of the British species of which are recorded by Dr. Nordstedt ("Remarks on British Submarine Vaucheriæ," Lund, 1886) as growing at the lowest tide-marks, and one species (*V. piloboloides*, "probably in quite salt water." The greater number of the Siphoneæ are natives of the warmer seas, and are especially abundant on the shores of rocky islands of which the principal constituent is lime. They spread their fibrous roots among the sandy *débris*, and are thus useful in holding together the particles of sand.

Some of the Siphoneæ have creeping stems, as have the Caulerpeæ. These plants, by extending the network of their creeping stems and roots over the sand, seem to exercise on the coast, within tide-marks, the same functions as the Maram (*Psamma arenaria*). This plant grows on the coast of Norfolk, and is found so useful in holding together the particles of sand, and thus aiding in the formation of land and preventing the inroads of the sea, that strict regulations are in force to prevent its destruction. In the same manner as the Maram spreads over the dry sands, the Caulerpeæ extend on the sea-shore within tide-marks, and are thus uncovered at low water. When the tide is out, they resemble green meadows. The utility of these plants in protecting the land was, a few years ago, unexpectedly proved in the neighbourhood of Adelaide, South Australia. A farmer suffered his sheep to stray upon the coast where the Caulerpeæ were exposed at low water. The sheep devoured the Algæ; the sea consequently broke in and established itself, and land was thus permanently lost. Many species, as Halimeda and Penicillus, have roots which are occasionally as large as small hens' eggs, formed of innumerable branched fibres which penetrate deeply into the sand. Some of the stipiform species emit flagelliform creeping "propagula," from which spring new plants; hence, observes Dr. Agardh, the Siphoneæ may be said to be social plants. Some Algæ, as Anadyomene, grow in shallow water exposed to the full influence of light, while others, like Bryopsis, prefer deep water to which light scarcely penetrates.

Many, but not all, of the Algæ belonging to the Siphoneæ have, like the Corallina family, the power of absorbing lime from the water. Young plants are generally green, but the incrustation of lime, in certain species, increases with age. In some genera it is entirely absent, as in Codium; in others it is extremely slight; while in some species of Halimeda the whole frond is frequently cased with a hard coating of lime, and looks like a gigantic frond of Corallina.

The disposition of the families of which the Siphoneæ are composed must be attended with some difficulty until the fruit is more perfectly known. In the interim Dr. Agardh proposes the arrangement adopted in the work. The group has been considerably enlarged, including as it now does the Dasycladeæ and Valoniaceæ. With regard to the former, the author observes that the Dasycladeæ are quite distinct from all the other genera, with their verticillate stems and external sporangia; hence he considers that they undoubtedly form a natural family. The fructification nevertheless varies in different genera.

There appears no doubt as to the limits of the Caulerpeæ. These are set forth in the first part of the present work ("Till Algernes Systematik"), and, although some observations are still wanting as to the fructification, the Caulerpeæ form a very distinct family.

The limits of the Valoniaceæ are very difficult to determine. In the form and size of their cells they for the most part are very near the Caulerpeæ. They are little more than ramulose prolifications; hence the ramification, although it is in some species more or less obscured, may be said to have a common character. But little is known of the fruit of these plants. In some genera the structure varies from that of the other Valoniæ, and approaches near to that of Ulva, as may be seen in Dictyosphæria and Anadyomene.

Among the remaining genera are some whose fronds consist almost entirely of compound tubes incrusted with lime. In these the normal ramification is di-trichotomous, but in Bryopsis and Codium—which, however, are not coated with lime—it may be said to be pinnate. Of the fructification of these plants few observations are recorded. In *Udotea Desfontainsii* and *Halimeda tuna*, true sporangia have been observed. Both belong to the Udoteaceæ. Whether the Spongodieæ, with their *quasi*-composite fronds, and the Bryopsideæ, with their free filaments, should be separated from each other, or united into one family, may be subject for consideration.

Vaucheria and Botrydium are not treated in the present work: neither does Dr. Agardh know to which family of the Siphoneæ they should be attached.

The whole group, in which the Dasycladeæ and Valoniaceæ are now included, is thus arranged by Dr. Agardh:—

I. BRYOPSIDEÆ.

(1) Bryopsis; (2)? Derbesia.

II. SPONGODIEÆ.

(3) Codium, ?? Cladothele.

III. UDOTEACEÆ.

(4) Chlorodesmis; (4*a*)? Avrainvillea; (5) Espera; (6) Penicillus; (7) Rhipocephalus; (8) Callipsygma; (9) Udotea; (9*a*)? Rhipidosiphon; (10) Halimeda.

IV. VALONIACEÆ.

(11) Valonia; (12) Siphonocladus, ? Ascothamnion, ? Trichosolen; (13) Apjohnia; (14) Struvea; (15) Chamædoris; (16) Dictyosphæria; (17) Anadyomene.

V. CAULERPEÆ.

(18) Caulerpa.

VI. DASYCLADEÆ.

(19) Dasycladus; (20) Chlorocladus; (21) Botryophora; (22) Cymopolia; (23) Neomeris; (24) Bornetella; (25) Halicoryne; (26) Polyphysa; (27) Acetabularia, ? Pleiophysa.

It will be observed that the position of Derbesia, Cladothele, Avrainvillea, Rhipidosiphon, Ascothamnion, and Trichosolen are not yet finally determined. Neither does the author yet see his way to include Chlorodictyon (*Ast. Holm.* 1870, Öfversigt No. 5, p. 427, tab. iv.) in the present arrangement. Codiolum is also excluded. The genus Balbisiana is mentioned (p. 10) once, but is not again referred to. Has *Bryopsis Balbisiana* been formed into a distinct genus under this name?

Under Avrainvillea Dr. Agardh includes the Fradelia of Chauvin, the Chloroplegma of Zanardini, and the Rhipilia of Kützing. This genus has the habit of Udotea, a cylindrical stem, a coriaceous, flabellate frond, of a very dark colour, with lacerated apex, forming irregular lobes, in which the zones of Udotea are absent. Of the fruit nothing appears to be known.

Of Rhipidosiphon there is no description, and very little seems to be actually known about this Alga. To Dr. Agardh it appears to be a young plant of Udotea.

Our knowledge of Ascothamnion (*Valonia intricata*, C. Ag.) is very limited, although the plant has been found in most of the warmer seas.

Trichosolen is a native of the Antilles, where it was found by Montagne. With Pleiophysa Dr. Agardh is acquainted only through Kützing's figure (*Tab. Phyc.*, vol. xvi. tab. 1). The habit and form of the sporidia agree with those of Halicoryne.

Penicillus Phœnix now appears as *Rhipocephalus Phœnix*. The new genus Callipsygma is founded on an Australian Alga which bears a certain resemblance to the last-mentioned plant. The former has an undivided terete stem incrusted with lime, while in the latter the stem is two-edged, without incrustation, and from the margins issue pinnate ramuli. The fruit of both genera is unknown.

Chlorocladus is between Dasycladus and Botryophora = *Dasycladus occidentalis*. These three genera are especially distinguished from each other by their fruit.

Of the whole group of the Siphoneæ three genera only have representatives on the British shores. These three genera are Bryopsis, Derbesia, and Codium. They have all a wide range. Of the nineteen species of Bryopsis, two are natives of these shores. Derbesia ranges from the Adriatic to the Faroe Isles and Norway. Dr. Agardh does not seem to be aware that *D. tenuissima* has been found on the British coast. Although Codium has so extensive a range, no species has yet been recorded from the east coast of the United States. That remarkable plant, *C. bursa*, which is found on the southern coast of Britain, the Mediterranean and Adriatic, has recently been obtained from Victoria, Australia. On the Sussex coast it may sometimes be picked up after storms. Its range in depth of water is about the same as that of *Rytiphlœa pinastroides*, with fragments of which, when hollow and torn, the frond is sometimes filled. *Sphacelaria plumula* grows on it occasionally. Dr. Agardh mentions that three or four fronds often grow together. The writer possesses a specimen from Brighton, which consists of a group of ten fronds, one of which is fixed to a piece of chalk; the others grow upon one another, a few filaments attaching the young plants to the older ones. In 1870 the Rev. E. S. Dewick was fortunate enough to pick up a specimen at Eastbourne, which, on examination, proved to be in fruit. He stated at a meeting of the Eastbourne Natural History Society (November 18, 1870) that "the Coniocystæ are produced on the outer surface of the clavate filaments, and differ from those of *C. tomentosum* only in being nearer the top of the filaments, and smaller in proportion to their size."

Codium tomentosum was reputed to be nearly cosmopolitan. Dr. Agardh, however, shows that several species have been included under this name, and that the so-called Australian forms belong to *C. Muelleri*, *C. Galeatum*, and *C. mucronatum*. In the last two species the utricles are mucronate, as represented in Plate 1, Figs. 1, 2, 3. *C. elongatum*, in which the frond, instead of being cylindrical, as in *C. tomentosum*, is compressed, is recorded by Dr. Agardh from Ireland. This fact is worthy the attention of British algologists. *C. latum*, found by M. Suringar on the coast of Japan, is not referred to in Dr. Agardh's work, neither is the plant, apparently allied to Codium, called by M. Suringar Acanthocodium (see "Alg. Jap.," p. 23). This also is a native of Japan, and probably but very little known.

Although so many points in the history of the Siphoneæ are still undetermined, this work of Dr. Agardh's will be found full of interest and instruction.

MARY P. MERRIFIELD.

AMERICAN MINING INDUSTRIES.

Report on the Mining Industries of the United States (exclusive of the precious metals.) By R. Pumpelly, 4to, pp. xxxviii.–1025. (Washington: Government Printing Office, 1886.)

THIS, the fifteenth and final volume of the Reports illustrating the results of the census of the United States taken in 1880, is in great part devoted to descriptions of the principal districts producing iron ores in the United States, the condition of the mines during the census year being studied in considerable detail, and in many cases illustrated by sketches of the workings. A very large number of samples of the ores of the different mines were collected by specially appointed agents, who visited every district and almost every mine of importance, and these were examined by a chemical staff at a special laboratory at Newport, Rhode Island. It was originally intended to make complete analyses of the greater number of the 1400 samples so collected, but the early exhaustion of the funds voted for the census necessitated an extensive curtailment of the plan, and only the more important minerals from the older rocks were completely analyzed; while for the bulk of the remainder, the properties of the more important constituents, iron, phosphorus, and sulphur, were alone determined, and the presence of titanium and manganese noted incidentally. The total number of samples investigated was 1250, 53 being completely and 1157 partially analyzed. The description of the methods of analysis adopted, and the tabulation of the results, occupy about a hundred pages, in addition to the 500 devoted to the geology and topography of the iron ore mines and their statistics.

The section devoted to coals, occupying eighty-seven pages, is mainly statistical, and has a very valuable introduction by Dr. Frederick Prime, Jun., which is perhaps the best condensed account of the nature and distribution of American coals that has yet appeared. A third section on the Cretaceous coals and lignites of the North-West is the result of an extensive exploration of the country traversed by the Northern Pacific Railway, made by the author subsequently to the completion of the census work proper, in 1882. This work, under the title of the Northern Transcontinental Survey, was suddenly stopped after about £20,000 had been expended upon it; and in order that the results might not be lost the observations have been reduced, analyses of the coals have been made, and a systematic memoir on the whole subject has been produced, which, although not exactly in the place where we should expect to find it, is too valuable an addition to American geology not to be welcomed in spite of its incongruous surroundings. The statistics of the base metals and minor minerals, occupying the remainder of the volume, are now of comparatively little interest, as these subjects have been treated from year to year in the returns published by the United States Geological Survey, and are available up to 1885. It must, however, be remembered that it is only in census years that returns from individual establishments can be obtained, and that therefore the figures for those years may be regarded as more authoritative than those of other dates. In any case, statistics five years old are tolerably ancient history.

In conclusion, we must call attention to the author's introductory paper on the geographical and geological distribution of the iron ores of the United States. This is a masterly abstract of the main subject of the book, and will be particularly useful to those who may wish to acquire some knowledge of the basis of the American iron industry without searching through the great mass of reports and surveys in which most of the detailed information is to be found. A plate of comparative sections of the strata in the principal iron-ore producing States is especially interesting as showing how the most important ore deposits are confined to the older rocks, such as the Archæan regions of New York and New Jersey, the Huronian of Michigan and Wisconsin, and the great stratified belt of hæmatite or "fossil ore" in the Clinton group of the Upper Silurian; while the most important iron-bearing strata of this country and Western Europe, the Lias and Lower Oolitic series, are entirely absent. Although the great activity of the iron trade in 1880-81 was the cause of very energetic explorations, very few discoveries were made in the older producing districts, and it became evident that to make these it was necessary to go into new fields, and in any case the author considers that the accessible rich ores may perhaps be practically exhausted within the life of the present generation. It will then be necessary to fall back upon the leaner kinds, containing from 30 to 45 per cent. of iron, which are known to exist in vast quantities, though generally far removed from coal suited for smelting purposes.

H. B.

OUR BOOK SHELF.

Theory of Magnetic Measurements. By F. E. Nipher, Professor of Physics in Washington University. (London: Trübner and Co., 1887.)

THIS little work is intended to furnish information as to the practical details of a magnetic survey. The description of the instruments used is poor. Full details as to the necessary calculations are given. The directions for the use of the instruments involve in a few cases unnecessary precautions, while in others the method suggested appears rather rough. Thus the statement that it is advisable not to make any observations with a dip needle till ten minutes after magnetization, is not, we think, borne out by experience. On the other hand, the suggestion that the vibrations of a declination needle may be checked by the finger would be likely to mislead beginners. It would have been better to describe the method of bringing the magnet to rest by means of a small auxiliary magnet. On the whole, English students will probably find all that they want, and with more direct reference to the Kew pattern instruments, in Stewart and Gee's "Practical Physics," and are thus not likely to make much use of Mr. Nipher's work. A. W. R.

Studies in Life and Sense. By Andrew Wilson, F.R.S.E. (London: Chatto and Windus, 1887.)

PREVIOUS works of this kind by Dr. Andrew Wilson are so well known, that a very few words will suffice to introduce the present one to the notice of our readers. It consists of a re-publication of essays on biological and psychological topics, which the author has from time to time contributed to sundry magazines. Although there is little or no attempt at originality, the collection is well calculated to prove of use and interest to general readers. The style is everywhere entertaining, and the following is a list of the subjects treated:—"Human Resemblances to Lower Life," "Some Economics of Nature," "Monkeys," "Elephants," "Past and Present of the Cuttle-Fishes," "Migration of Animals," "The Problems of Distribution," "Songs without Words," "The Laws of Speech," "Body and Mind," "The Old Phrenology and the New," "The Mind's Mirror," "What Dreams are made of," "Coinages of the Brain," "The Inner Life of Plants," "An Invitation to Dinner."

Fermenti e Microbi. Saggio di Igiene Antimicrobica di Italo Giglioli. (Napoli, 1887.)

THIS book may be considered as marking a new departure in the teaching of hygiene. The enormous advances that have been made of late years in the recognition of pathogenic microbes, their life-history, and the conditions affecting them one way or another, have added a large and important chapter to the study of sanitary science. It is this particular subject in all its bearings on sanitary science which is treated in the volume by Prof. Giglioli. The study of ferments, like yeasts, forms the introduction: their life-history, physiological and chemical action, are described, and, owing to the accurate knowledge that we possess of them—thanks in a great measure to the researches of M. Pasteur—they form a fit starting-point in the study of schyzomycetes, bacteria, or microbes proper.

The book treats of microbes from every aspect, morphological and physiological. The relation of microbes in general to the nutritive media, their chemical products, and the relation of these to the microbes themselves; the production of soluble ferments by them; the influence of light, heat, &c., are passed in review and treated fully.

The pathogenic organisms are next considered. Their relation to the animal body; the means by which they gain access to the animal system; the various influences commonly understood to constitute "predisposition"; the relation of pathogenic bacteria to food, air, soil, and water; the adverse influences, such as heat and light, disinfectants and antiseptics, &c., are all discussed with great lucidity and thoroughness.

There is hardly any aspect under which the study of pathogenic microbes—including the question of attenuation—presents itself, which is not discussed in this volume. The arrangement of the subject-matter is systematic, and the method of treatment does great credit to the author, inasmuch as he is, as far as possible, objective. He carefully weighs and sifts evidence, and does not disdain to make references to the literature of England and France. He has, in fact, carefully read the literature of this country on infectious diseases, and thus attests that he is not guided by that spirit of narrowness which one often meets with in modern German works.

An English translation would, we have no doubt, be a valuable addition to our own literature. E. KLEIN.

Photography of Bacteria. By Edgar M. Crookshank, M.B. (London: H. K. Lewis, 1887.)

SINCE Koch first employed photography in bacteriology ("Biol. d. Pflanzen," 1877, ii. 3) various attempts have been made in this country and on the Continent to advance the methods of photographing microscopic objects, such as Bacteria, with high magnifying powers. About fifteen years ago Dr. Woodward, of Washington, published photographic plates of histological objects taken under tolerably high magnifying power (400 and 500 diameters). These plates were brought out by the Surgeon-General's Office, Army Medical Museum of the United States: they attracted at the time a good deal of attention owing to their comparatively high excellence. That good photographs of histological and other microscopic objects are of great value in themselves, owing to their exactness, and the various advantages for purposes of publication, may be taken as requiring no further proof, and it seems equally obvious that indifferent photographs are of less value than accurate drawings.

Now, comparing Dr. Crookshank's photograms of histological and bacteriological objects, published in the present volume, the former with those of Dr. Woodward, the latter with those of Koch, there can be little doubt that no real advance has yet been made in producing photograms that are to take the place of accurate drawings. By saying this I do not mean to convey the impression that in Dr. Crookshank's volume there are not some good photographs—*vide* his Plate XVI., further his Figs. 7, 8, 30, 35, and 45, all of which are really fine in many respects—but taking photography as a whole, as applied to the representation of microscopic objects under high powers, I think that the time has not yet come when it can be said to have supplanted good and accurate drawings. In connexion with this it must certainly appear remarkable that in the numerous and important publications on Bacteria by Koch and his pupils since 1877 to the present time we do not find a single illustration represented by micro-photography. All their published illustrations are drawings.

With the new apochromatic objectives and projection eye-pieces by Zeiss better results may be looked for, and Dr. Crookshank, with his great skill in, and knowledge of, the technique, will, we have little doubt, be able to produce them.

As a clear and detailed account of practical microphotography, Dr. Crookshank's book is of great merit, and will prove very useful and important. As the first treatise on the subject in any language it is sure to command a high place. E. KLEIN.

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. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

The Sense of Smell in Dogs.

IT is, I think, of some interest to supplement the very striking and exact experiments of Mr. Romanes on the scent of dogs, by an account of some experiments of a like kind made with a very different kind of dog, viz. a pug bitch. She was taught to hunt for small pieces of dry biscuit in a good-sized dining-room. The dog was put out of the room and a small piece, not much bigger than a shilling, of dry Osborne biscuit, was hidden; and as long as the hiding-place was accessible to the dog she never failed to find it. Sometimes the biscuit would be placed under a heap of a dozen or more newspapers on a dinner waggon, sometimes under a footstool, or sofa-cushion, or fire-shovel, and on two or three occasions in the foot of a boot which had been just taken off, the hiding body being always carefully replaced before the dog was admitted into the room, and without exception the biscuit in a very short time was discovered. It was over and over again proved that the dog did not follow the trail of the person who had hidden the biscuit; often the dog went by a different route, and in some cases one person hid the biscuit and another opened the door.

The experiment which has now special interest is the following one. A small piece of biscuit was placed on the floor under the centre of a footstool which was one foot square and six inches high, and standing on feet which raised it one inch from the ground. The dog, from the way in which she would set about moving the stool, not a very easy thing to do, as it stood in an angle of the wall, was evidently certain that the biscuit was beneath, and as scent seemed the only means by which she could have come at this conclusion, I thought to entirely mask this scent and prevent her finding the biscuit by pouring eau-de-Cologne on the stool. I found, however, it had no such effect, the biscuit was as readily and surely found when the eau-de-Cologne was there as when absent. It seems, then, that not only well-worn boots leave behind a recognizable odour, as Mr. Romanes proved, but also that to us at least so odourless a substance as dry plain biscuit emits so much and so characteristic a smell that it immediately spreads, even through considerable obstacles, to a distance of several inches in a few seconds, for in most cases the biscuit was found in thirty to sixty seconds after it had been hidden; thus time was not allowed, one would think, for all the surroundings of the hiding-place to become saturated with the scent.

W. J. RUSSELL.

Units of Mass, Weight, and Force.

MIGHT I venture to suggest to Prof. Greenhill that it would be very interesting to mathematicians, and probably would throw great light on the above subject, if he would give us quotations from some work by a practical engineer in which the idea of *inertia* distinctly appears. Or, failing this, perhaps Prof. Greenhill could give practical instances (other than problems in gunnery) in which *mass* quite apart from weight enters into the engineer's calculations.

It seems to me that many practical engineers never have occasion to deal with acceleration, except that of circular motion, and consequently only need to consider the *weight* of stuff, and have no use for the dynamical unit of force.

Gonville and Caius College, July 23. JOHN B. LOCK.

Chemical Affinity and Solution.

IN continuation of my inquiry into the relation between chemical affinity and solution (NATURE, vol. xxxiii. p. 615, and and vol. xxxiv. p. 263) I would direct attention to some remarkable facts in connexion with the heats of formation of the sulphates. Take H_2SO_4Aq, and assume that SO_3 acts on the O of water with the average energy with which the S acts on O_3, and we have the following :—

$$\begin{array}{lrcl} [H_2,O] & = 68360 & & [H_2S,O_4Aq] = 210770 \\ [S,O_3] & = 103240 & & \\ [S,H_2] & = 4740 & & \\ [SO_3,O] & = 34413 & & \\ \hline & 210753 & = & 210770 \end{array}$$

Now consider $BaSO_4$. We have $[Ba,S,O_4] = 338070$ and—

$$\begin{array}{lrcl} [Ba,O] & = 124240 & & \\ [S,O_3] & = 103240 & & \\ \text{Difference} & = 110590 & & \\ \hline & 338070 & = & 338070 \end{array}$$

The difference 110590 is almost exactly equal to $[Ba,S] = 109600$, so that the heat of combination of BaO with SO_3 is practically equal to $[Ba,S]$, and the whole of the affinity of S is used up so that it has no power to act on the O of water, and hence the salt is insoluble.

Take again in the same manner $SrSO_4$, and the result is even more striking—

$$\begin{array}{lrcl} [Sr,O] & = 128440 & & [Sr,S,O_4] = 330900 \\ [S,O_3] & = 103240 & & \\ \text{Difference} & = 99220 & & \\ \hline & 330900 & = & 330900 \end{array}$$

Difference $99220 = [Sr,S] = 99200$, and again we have an insoluble salt. This seems to me pretty strong evidence that the cause of these combinations is the affinity of S for the metal, and that the S cannot act on the water to cause solution, because of its intense affinity for the metal. Further, the heat of neutralization is the difference between the heat of solution of the oxide and SO_3 on the one hand, and the heat of $[MS]$ on the other, thus :—

$$\begin{array}{lrcl} [SrO,Aq] & = 29340 & & \\ [SO_3,Aq] & = 39153 & & \\ \text{Neutralization} & = 30710 & & \\ \hline & 99203 & = & [Sr,S] = 99200 \end{array}$$

and so on in other cases.

Now examine $CaSO_4$, which is a sparingly soluble salt, and note the difference, we have—

$$\begin{array}{lrcl} [Ca,O] & = 130930 & & [Ca,S,O_4] = 318370 \\ [S,O_3] & = 103240 & & \\ \text{Difference} & = 84200 & & \\ \hline & 318370 & & 318370 \end{array}$$

This difference, 84200, is not equal to $[Ca,S]$, which is $= 92000$, or 7800 units more, and accordingly we find this salt slightly soluble with a heat of 4440 units, because the S is somewhat free to act on water. Further, we have the remarkable fact that $CaSO_4$ combines with $2H_2O$, and evolves in so doing 4740 units of heat, which is exactly equal to $[S,H_2]$. Evidently the whole of the affinity of S for Ca not being used up in CaO,SO_3 the S can act with its full energy on the H of the water. $MgSO_4$, which is a still more soluble salt, shows entirely analogous results, the freedom of the S to act on water being much greater than with $CaSO_4$.

Take now an example of a somewhat different nature ; consider the following :—

$$\begin{array}{lrcl} [Na_2,O] & = 99760 & & [Na_2,S,O_4,10H_2O] = 347810 \\ [S,O_3] & = 103240 & & \\ \text{Difference} & = 144810 & & \\ \hline & 347810 & = & 347810 \end{array}$$

The heat of $[Na_2,S]$ is only 88200 units, but the heat of solution of Na_2O is 55500, and these two make up very nearly the difference of 144810 units. Thus we have the affinity of the S entirely used up, but the affinity of the Na_2 for the oxygen of the H_2O is so great that it can combine as a crystal with ten molecules, in addition to combining with the SO_3.

If space permitted, these facts might be extended and gone into more minutely, and their complete agreement in every particular with my theory of solution pointed out.

I may add further that the amount of salt dissolved in saturated solutions which I have examined is in complete harmony with that theory, as the following example will show :—

Heat of Combination. $[M,Cl_2] - [M,O,Aq]$	Amount of Salt in Saturated Solution. MCl_2]
Ca = 20560	63 grains
Sr = 26770	46 ,,
Ba = 35980	35 ,,

It is evident at once that the amount of salt in solution is almost exactly inversely as the difference of heat of $[M,Cl_2]$ and $[M,O,Aq]$.

WM. DURHAM.

Early Perseids.

FROM my observations in preceding years I found the great shower of Perseids commenced on about July 25, and that the last visible traces of it were seen on August 22, after a duration of 29 days.

This year a series of very clear nights occurred on July 16, 18, 19, 20, 21, 22, 23, 27, 28, and 29, and I watched the sky attentively throughout each one, with the idea of tracing, if possible, the earlier stages of this famous shower. On the 16th there were certainly no Perseids visible, but on the 18th, at 11h. 1m., I saw a brilliant streak-leaving meteor in Andromeda, which must have belonged to this stream. On the 19th I recorded 4 Perseids (2 of which were brilliant), and the radiant-point was sharply defined at 19° + 51°. On the 20th and 21st I noticed several other Perseids, but they were too distant from their radiant, and the paths too few to indicate a good centre. On the 22nd, however, I saw 5 Perseids (one of which was as bright as Jupiter), and the radiant now appeared at 25° + 52°. On the 23rd I registered 4 Perseids, apparently from the same point of the heavens.

The few ensuing nights were cloudy, but on the 27th the sky became partly clear, and in 3 hours I counted 38 meteors, of which 5 were Perseids from a radiant at 29° + 54°. On the 28th in 3¾ hours I saw 47 meteors, though clouds were very prevalent all night. On this occasion 10 Perseids were seen from a centre at 30° + 55°, and there were 15 Aquariads from 337° − 12°. On the 29th the sky was almost uninterruptedly clear, and in 3¾ hours I recorded 52 meteors, including 10 Perseids from 31° + 54½. On the 30th, clouds prevailed.

Between July 16 and 29 I observed 287 meteors, of which 43 were Perseids. These observations prove that the display really begins a week earlier than that (July 25) given in my paper in the *Monthly Notices* of the Royal Astronomical Society, vol. xlv. p. 97. The displacement of the apparent radiant-point as there described is well confirmed by my new observations. During the interval from July 18 to August 22 this point advances from 19° + 51° to 77° + 57°.

I subjoin the observed paths of a few bright meteors seen during my recent observations :—

1887.	h. m.	mag.	Path.		Radiant.
			° °	° °	° °
July 19 ...	11 43 ...	1 ...	358½ + 38 to	351 + 30 ...	streak ... 19 + 51
	11 43 ...	1 ...	298 + 56 ,,	14 + 65¼ ...	slow ... 271 + 21
	12 25 ...	1 ...	350 + 62 ,,	330½ + 64 ...	streak ... 19 + 51
	12 52 ...	1 ...	9 + 20 ,,	17 + 20¼ ...	swift ... 333 + 12
,, 22 ...	10 59 ...	♃ ...	16 + 41 ,,	16½ + 51 ...	streak ... 16 + 31
	12 21 ...	♃ ...	356 + 45 ,,	332 + 29 ...	streak ... 25 + 52
	12 25 ...	♃ ...	344 + 33 ,,	320 + 29 ...	streak ... 16 + 31
	13 15 ...	♃ ...	323 + 37 ,,	355½ + 10½ ...	slowish ... 271 + 48
	13 35 ...	1 ...	111 + 50 seemed stationary.		
,, 27 ...	10 40½ ...	♃ ...	325 + 6 ,,	330 + 8 ...	slow ... 322 + 4
	13 21 ...	♃ ...	319½ + 16½ ,,	308 + 32 ...	slowish ... 337 − 12
,, 29 ...	11 28 ...	♃ ...	66 + 72½ ,,	114 + 70 ...	swift, streak 21 + 57

Many others were seen of 1st mag. A perfectly stationary meteor of the 2nd mag., and sparkling like a star, was visible on July 29 at 14h. 17m. at 337° − 12°, so that it was an Aquariad travelling directly in the line of sight.

On the 22nd I registered some brilliant meteors, of precisely the same visible type as the Perseids, from a radiant at 16° + 31° or 3° south of β Andromedæ. Many meteors have also been falling from the points 269° + 49°, 310° + 9°, 333° + 12°, 335° + 49°, and 351° + 38°. All these are swift and short, and generally devoid either of streaks or trains.

Bristol, July 31. W. F. DENNING.

P.S.—In 1885 and some other years I have seen, on about July 13, a very definite shower of bright streak-leaving meteors from the point 11° + 48°, and it is a very probable conjecture that this radiant represents the earliest manifestation of the stream of Perseids.—W. F. D.

Floating Eggs.

REFERRING to the remarks of Mr. E. E. Prince in NATURE, July 28, p. 294, on the above subject, I wish to add that the spawn found by me had "the light violet-gray tint" and crape-like appearance he describes. I am very much on the water in harbours frequented by *Lophius*, but never saw any of this spawn before.

We found it in a swirl of the tide off Bantry Bay, where the sea was over 40 fathoms deep, and in the midst of innumerable jelly-fish, which seem to congregate wherever the current is most swift. W. S. GREEN.

Carrigaline.

THE "METEOROLOGISKE INSTITUT" AT UPSALA, AND CLOUD MEASUREMENTS.

THE Meteorological Institute at Upsala has gained so much fame by the investigations on clouds which have been carried on there during the last few years, that a few notes on a recent visit to that establishment will interest many readers.

The Institute is not a Government establishment; it is entirely maintained by the University of Upsala. The *personnel* consists of Prof. Hildebrandsson, as Director; M. Ekholm and one other male assistant, besides a lady who does the telegraphic and some of the computing work.

The main building contains a commodious office, with a small library, and living apartments for the assistant. The principal instrument-room is a separate pavilion in the garden. Here is located Thiorell's meteograph, which records automatically every quarter of an hour on a slip of paper the height of the barometer, and the readings of the wet and dry thermometers. Another instrument records the direction and velocity of the wind.

This meteograph of Thiorell's is a very remarkable instrument. Every fifteen minntes an apparatus is let loose which causes three wires to descend from rest till they are stopped by reaching the level of the mercury in the different tubes. When contact is made with the surface of the mercuries, an electric current passes and stops the descent of each wire at the proper time. The downward motion of the three wires has actuated three wheels, each of which carries a series of types on its edge, to denote successive readings of its own instrument. For instance, the barometer-wheel carries successive numbers for every five-hundredth of a millimetre—760·00, 760·05, 760·1, &c.; so that when the motion is stopped the uppermost type gives in figures the actual reading of the barometer. Then a subsidiary arrangement first inks the types, then prints them on a slip of paper, and finally winds the dipping wires up to zero again.

An ingenious apparatus prevents the electricity from sparking when contact is made, so that there is no oxidation of the mercury. The mechanism is singularly beautiful, and it is quite fascinating to watch the self-acting starting, stopping, inking, and printing arrangements.

We could not but admire the exquisite order in which the whole apparatus was maintained; the sides of the various glass tubes were as clean as when they were new, and the surfaces of the mercuries were as bright as looking-glasses.

The University may well be proud that the instruments were entirely constructed in Stockholm, by the skilful mechanic Sörrenson, though the cost is necessarily high. The meteograph, with the anemograph, costs £600, but the great advantage is that no assistant is required to sit up at night, and that all the figures wanted for climatic constants are ready tabulated without any further labour.

But the Institute is most justly celebrated for the researches on the motion and heights of clouds that have been carried on of late years under the guidance of Prof. Hildebrandsson, with the assistance of Messrs. Ekholm and Hagström.

The first studies were on the motion of clouds round cyclones and anticyclones; but the results are now so well known that we need not do more than mention them here.

Latterly the far more difficult subjects of cloud heights and cloud velocities have been taken up, and as the methods employed, and the results that have been obtained are both novel and important, we will describe what we saw there.

We should remark, in the first instance, that the motion of the higher atmosphere is far better studied by clouds than by observations on mountain-tops; for on the latter the results are always more or less influenced by the local effect of the mountain in deflecting the wind, and forcing it upwards.

The instrument which they employ to measure the

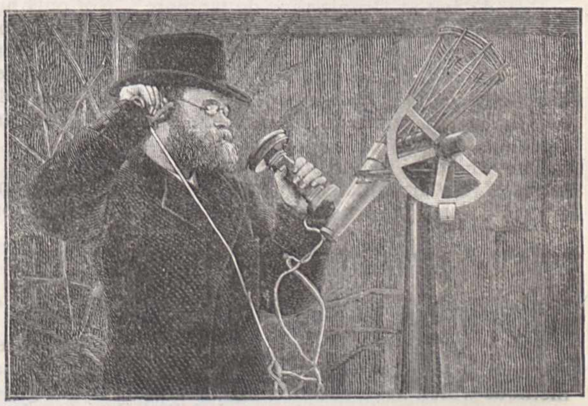

FIG. 1.—N. EKHOLM MEASURING CLOUDS.

This figure shows the peculiar ocular part of the altazimuth, with the vertical and horizontal circles. It also shows the telephonic arrangement.

angles from which to deduce the height of the clouds is a peculiar form of altazimuth, that was originally designed by Prof. Mohn, of Christiania, for measuring the parallax of the aurora borealis. It resembles an astronomical altazimuth, but instead of a telescope it carries an open tube without any lenses. The portion corresponding to the object-glass is formed by thin cross-wires; and that corresponding to the eye-piece, by a plate of brass, pierced in the centre by a small circular hole an eighth of an inch in diameter. The tube of the telescope is replaced by a lattice of brass-work, so as to diminish, as far as possible, the resistance of the wind. The vertical and horizontal circles are divided decimally, and this much facilitates the reduction of the readings.

The general appearance of the instrument is well shown in the figure, which is engraved from a photograph I took of M. Ekholm while actually engaged in talking through a telephone to M. Hagström as to what portion of a cloud should be observed. The lattice-work tube, the cross-wires in place of an object-glass, and the vertical circle are very obvious, while the horizontal circle is so much end on, that it can scarcely be recognized except by the tangent screw which is seen near the lower telephone.

Two such instruments are placed at the opposite extremities of a suitable base. The new base at Upsala has a length of 4272 feet; the old one was about half the length. The result of the change has been that the mean error of a single determination of the highest clouds has been reduced from 9 to a little more than 3 per cent. of the actual height. At the same time the difficulty of identifying a particular spot on a low cloud is considerably increased. A wire is laid between the two ends of the base, and each observer is provided with two telephones—one for speaking, the other for listening. When an observation is to be taken, the conversation goes on somewhat as follows:—First observer, who takes the lead: "Do you see a patch of cloud away down west?" "Yes." "Can you make out a well-marked point on the leading edge?" "Yes." "Well then; now." At this signal both observers put down their telephones, which have hitherto engaged both their hands, begin to count fifteen seconds, and adjust their instruments to the point of cloud agreed on. At the fifteenth second they stop, read the various arcs, and the operation is complete.

But when the angles have been measured the height has to be calculated, and also the horizontal and vertical velocities of the cloud by combining the position and height at two successive measurements at a short interval. There are already well-known trigonometrical formulæ for calculating all these elements, if all the observations are good; but at Upsala they do far more. Not only are the observations first controlled by forming an equation to express the condition that the two lines of sight from either end of the base should meet in a point, if the angles have been correctly measured, and all bad sets rejected; but the mean errors of the rectangular co-ordinates are calculated by the method of least squares.

The whole of the calculations are combined into a series of formulæ which are necessarily complicated; and even by using logarithms of addition and subtraction, and one or two subsidiary tables—such as for log. $\sin^2 \frac{\theta}{2}$, specially constructed for this work—the computation of each set of observations takes about twenty minutes.

Before we describe the principal results that have been attained, it may be well to compare this with the other methods which have been used to determine the height of clouds. A great deal of time, and skill, and money, have been spent at Kew in trying to perfect the photographic method of measuring the height of clouds. Very elaborate cloud cameras, or photo-nephoscopes, have been constructed, by means of which photographs of a cloud were taken simultaneously from both ends of a suitable base. The altitude and azimuth of the centre of the plate were read off by the graduated circles which were attached to the cameras; and the angular measurements of any point of cloud on the picture were calculated by proper measurements from the known centre of the photographic plate. When all this is done, the result ought to be the same as if the altitude and azimuth of the point of the cloud had been taken directly by an ordinary angle-measuring instrument.

It might have been thought that there would be less chance of mistaking the point of the cloud to be measured, if you had the pictures from the two ends of the base to look at leisurely, than if you could only converse through a telephone with the observer at the other end of the base. But in practice it is not so. No one who has not seen such cloud-photographs can realize the difficulty of identifying any point of a low cloud when seen from two stations half a mile or a whole mile apart; and for other reasons, which we will give presently, the form of a cloud is not so well defined in a photograph as it is to the naked eye.

At Kew an extremely ingenious sort of projector has been devised, which gives graphically the required height of a cloud from two simultaneous photographs at opposite ends of the same base, but it is evident that this method is capable of none of the refinements which have been applied to the Upsala measures, and that the rate of vertical ascent or descent of a cloud could hardly be determined by this method. But there is a far greater defect in the photographic method which at present no skill can surmount.

We saw that the altazimuth employed at Upsala had no lenses. The meaning of this will be obvious to anyone who looks through an opera-glass at a faint cloud. He will probably see nothing for want of contrast, and if anything of the nature of a telescope is employed, only well-defined cloud outlines can be seen at all. The same loss of light and contrast occurs with a photographic lens; and many clouds that can be seen in the sky are invisible on the ground glass of the camera. Cirrus and cirro-stratus—the very clouds we want most to observe—are always thin and indefined as regards their form and contrast against the rest of the sky; so that this defect of the method is the more unfortunate.

But even when the image of a cloud is visible on the focusing glass, it does not follow that any image will be seen in the picture. In practice, thin high white clouds against a blue sky can rarely be taken at all, or only under exceptional circumstances of illumination. The reason seems to be that there is very little light reflected at all from a thin wisp of cirrus, and what there is must pass through an atmosphere always more or less charged with floating particles of ice or water, besides earthy dust of all kinds. The light which is scattered and diffused by all these small particles is also concentrated on the sensitive plate by the lens, and the resulting negative shows a uniform dark surface for the sky without any trace of the cloud. What image there might have been is buried in photographic fog.

In order to compare the two methods of measuring clouds, I went out one day last December at Upsala with Messrs. Ekholm and Hagström when they were measuring the height of some clouds. It was a dull afternoon, a low foggy stratus was driving rapidly across the sky at a low level, and through the general misty gloom of a northern winter day we could just make out some striated stripes of strato-cirrus—low cirro-stratus—between the openings in the lower cloud-layer. The camera and lens that I use habitually for photographing cloud-forms—not their angular height—was planted a few feet from the altazimuth which M. Ekholm was observing, and while he was measuring the necessary angles I took a picture of the clouds. As might have been expected under the circumstances, the low dark cloud came out quite well, but there was not the faintest trace of the strato-cirrus on the negative. MM. Ekholm and Hagström, however, succeeded in measuring both layers of cloud, and found that the low stratus was floating at an altitude of about 2000 feet high, while the upper strato-cirrus was driving from S. 57° W. at an altitude of 19,653 feet, with a horizontal velocity of 81, and a downward velocity of 7·2 feet per second. This is a remarkable result, and shows conclusively the superiority of the altazimuth to the photographic method of measuring the heights of clouds.

Whenever opportunity occurs, measures of clouds are taken three times a day at Upsala, and it may be well to glance at the principal results that have been obtained.

The greatest height of any cloud which has yet been satisfactorily measured is only 43,800 feet, which is rather less than has usually been supposed; but the highest velocity, 112 miles an hour with a cloud at 28,000 feet, is greater than would have been expected. It may be interesting to note that the isobars when this high velocity was reported were nearly straight, and sloping towards the north-west.

The most important result which has been obtained from all the numerous measures that have been made is the fact that clouds are not distributed promiscuously at all heights in the air, but that they have, on the contrary, a most decided tendency to form at three definite levels. The mean summer level of these three stories of clouds at Upsala has been found to be as follows: low clouds—stratus, cumulus, cumulo-nimbus, 2000-6000 feet; middle clouds—strato-cirrus and cumulo-cirrus, 12,000-15,000 feet; high clouds—cirrus, cirro-stratus, and cirro-cumulus, 20,000-27,000 feet.

It would be premature at present to speculate on the physical significance of this fact, but we find the same definite layers of clouds in the tropics as in these high latitudes, and no future cloud-nomenclature or cloud-observations will be satisfactory which do not take the idea of these levels into account.

But the refinements of the methods employed allow the diurnal variations both of velocity and altitude to be successfully measured. The velocity observations confirm the results that have been obtained from mountain stations—that, though the general travel of the middle and higher clouds is much greater than that of the surface winds, the diurnal variation of speed at those levels is the reverse of what occurs near the ground. The greatest velocity on the earth's surface is usually about 2 p.m.; whereas the lowest rate of the upper currents is about midday.

The diurnal variation of height is remarkable, for they find at Upsala that the mean height of all varieties of clouds rises in the course of the day, and is higher between 6 and 8 in the evening than either in the early morning or at midday.

Such are the principal results that have been obtained at Upsala, and no doubt they surpass any previous work that has been done on the subject. But whenever we see good results it is worth while to pause a moment to consider the conditions under which the work has been developed, and the nature and nurture of the men by whom the research has been conducted. Scientific research is a delicate plant, that is easily nipped in the bud; but which, under certain surroundings and in a suitable moral atmosphere develops a vigorous growth.

The Meteorological Institute of Upsala is an offshoot of the Astronomical Observatory of the University; and a University, if properly directed, can develop research which promises no immediate reward in a manner that no other body can approach.

If you want any quantity of a particular kind of calculation, or to carry on the routine of any existing work in an Observatory, it is easy to go into the labour market and engage a sufficient number of accurate computers either by time or piece-work, or to find an assistant who will make observations with the regularity of clockwork.

But original research requires not only special natural aptitudes and enthusiasm to begin with, but even then will not flourish unless developed by encouragement, and the identification of the worker with his work. It is rarely, except in Universities, that men can be found for the highest original research. For there only are young students encouraged to come forward and interest themselves in any work for which they seem to have special aptitude.

Now, this is the history of the Upsala work. Prof. Hildebrandsson was attached as a young man to the meteorological department of the Astronomical Observatory, and when the study of stars and weather were separated, he obtained the second post in the new Meteorological Institute. From this his great abilities soon raised him to the directorship, which he now holds with so much credit to the University. M. Ekholm, a much younger man, has been brought up in the same manner. First as a student he showed such aptitudes for the work as to be engaged as assistant; and now, as the actual observation and reduction of the cloud-work is done by him and M. Hagström, the results are published under their names, so that they are thoroughly identified with the work.

Upsala is the centre of the intellectual life of Sweden, and there, rather than at Stockholm, could men be found to carry out original research. It redounds to the credit of the University that it has so steadily supported Prof. Hildebrandsson, and that he in his turn has utilized the social and educational system by which he is surrounded to bring up assistants who can co-operate with him in a great work that brings credit both to himself, to themselves, and to the Institute which they all represent.

RALPH ABERCROMBY.

A NEW COSMOGONY.[1]

I.

THE volume before us is composed of a series of essays, first published in the Catholic periodical, *Natur und Offenbarung*, in 1885-86. By far the greater part of it is, nevertheless, of a purely scientific character. The opening chapter alone enters upon theological considerations, which we cannot here pretend to discuss, recommending merely, in passing, the broad and wise doctrines it contains to the notice of those well-meaning persons who apprehend danger to creeds from speculations as to origins.

That of our planetary system is very actively in debate just now. The nebular hypothesis, as fashioned by Laplace, no longer fits in with all the known facts. There are so many of them that it would be surprising if it did, since the outside of its claim was to the plausible representation of possible truth. It had a part to play in the history of science, which it played with eminent success. This was to show that thought, safeguarded by right reason, might be trusted to run backward towards the beginnings of things—that, without visible discontinuity or breach of known laws, the present fair scheme of creation might have emerged from the brooding darkness of chaos, along paths not wholly inaccessible to human discursive faculties.

But now the reiterated blows of objectors may fairly be said to have shattered the symmetrical mould in which Laplace cast his ideas. What remains of it is summed up in the statement that the solar system did originate, somehow, by the condensation of a primitive nebula. The rest is irrevocably gone, and the field lies open for ingenious theorizing. It has not been wanting.

The newer cosmogonists are divided into two schools by the more or less radical tendencies of the reforms they propose. Some seek wholly to abolish, others merely to renovate, the Kant-Laplace scheme. The first class is best represented by M. Faye, the second by M. Wolf and Dr. Braun. Dr. Braun is, however, a more thorough-going renovator than M. Wolf. The edifice, as "restored" by him, shows, indeed, little trace of its original aspect. Scarcely the invisible foundations are the same; the superstructure is unrecognizable. We will endeavour to sketch its main features.

In widening the nebular hypothesis to embrace the whole sidereal world, our author demands as little as possible in the way of postulates: simply a co-extensive nebula, structureless, motionless, tenuous, its particles endowed with gravity and atomic repulsion. Such a nebula, if perfectly homogeneous, should give birth to one portentous, solitary sun. But, in point of fact, it would possess innumerable, almost imperceptible, local irregularities, which, forming so many centres of attraction, would eventually lead to the breaking-up of the nebula into a vast multitude of separate fragments.

On one of these, the destined progenitor of the solar

[1] "Ueber Cosmogonie vom Standpunkt christlicher Wissenschaft. Mit einer Theorie der Sonne." Von Carl Braun, S.J. (Münster: Aschendorff, 1887.)

system, we are asked to concentrate our attention. The manner of its development is, however, a widely different one from that traced by the great French geometer. Laplace *assumed* the needful rotation, and left the rest to work itself out spontaneously. He permitted no external interference with the tranquil processes which he discerned as progressing through the ages. Dr. Braun, on the other hand, assumes less to begin with, but invokes adventitious aid in emergencies. No single nebula thrown on its own resources sufficed, he finds, for the production of the solar system. The complicated phenomena which it presents demand a complex origin to explain them. The mass of cosmical matter in which they first began to unfold themselves was accordingly but the nucleus of what it afterwards became. It not only grew by the accretion of similar masses falling towards it from space, but acquired its gyratory movement by eccentric collisions with them. The consequences of such events elsewhere are visibly pictured to us in the spiral lines of light of certain nebulæ. The great whirlpool in the Canes Venatici, for instance, betrays and records the fall of a comet, on the gigantic primitive scale, into an embryo sun. Only thus, in our author's opinion, can the strange peculiarities of its structure be accounted for; and only thus can the first impulse to axial rotation in our own system have been given.

The visits of comets, as we now see them, feebly represent, we are told, the colossal in-rushes from interstellar regions by which the machinery of planetary production was set going and modified. But it is difficult to allow to such bodies the independent origin implied in the claim for them of such illustrative significance. Comets can no longer be set down as mere casual intruders upon the solar system. They certainly share its translatory motion, since, if they were either overtaken or encountered, they should seem to come most numerously from near the apex of the sun's path. But they approach him indifferently from all parts of the sky. A further proof of the absence of relative motion is afforded by the shape of the tracks they pursue. M. Faye has remarked that, of 364 cometary orbits calculated, not one is a decided hyperbola ("Origine du Monde," p. 146); and Laplace's view that they are hyperbolic by nature, and elliptical only through perturbations, is thus seen to be the exact reverse of the truth.

A fundamental objection to Laplace's cosmogony is that it implies a far swifter axial movement in the central bodies of our system than they actually possess. For in the theory of annular separation, the rotation of the generating mass is strictly correlated with the revolution of its offspring by the principle of the conservation of areas, which requires that a rotating homogeneous globe should spin quicker, as it contracts, in the proportion of the square of its radius. Thus, if the solar nebula, when it filled the orbit of Neptune, rotated (as on the hypothesis in question it must have done) in Neptune's period of 165 years, the period of the sun shrunken to its present dimensions, should have shortened in the ratio of the square of 2,780,000,000 (the mean distance of Neptune) to the square of 434,000 miles (the solar radius). In other words, the rotation of the sun should be accomplished in 127 seconds, in lieu of 25 days. Similarly, the terrestrial rotation-period corresponding to the lunar revolution in $27\frac{1}{3}$ days, is no more than $10\frac{1}{8}$ minutes! It is true that in both these estimates (the latter taken from Dr. Braun's pages), the effects of central condensation are neglected, although it must inevitably have made some progress before annulation began; but no allowance on this score, however liberal, can possibly reconcile, though it contributes to lessen, the discrepancy.

Dr. Braun adjusts the balance in this way. The solar nebula had never at any time, in his view, a uniform axial movement. He even ventures to consider the present unequal rotation of the sun as a survival of the primitive state of things to which the central deficiency of rotational momentum is due. For the entire mass was, in the beginning, set gyrating by external impacts. Movement was hence generated predominantly in its outer regions, and was only by degrees and imperfectly communicated to the nuclear parts.

The device is marked by considerable ingenuity, and is at any rate preferable to the eddying movements by which M. Faye evades the same embarrassment. It is, however, scarcely needed by Dr. Braun, since the "ring-theory" of planetary formation is almost, and logically ought to be completely, abandoned by him. Difficulties have of late been thickening round it; they reached a climax when the conviction was attained that, apart from the neutralizing effects of tidal friction, it could only result in the retrograde motion of all secondary systems. The plan of centres of condensation is accordingly substituted by our author. This has the advantage of allowing planets to begin to form anywhere in the nebula. It emancipates them from that strict conformity to the equatorial level which was an inconvenient feature of Laplace's hypothesis; and though they necessarily tended, in the course of their growth, to descend towards it, enough may perhaps remain of their primitive divergence to explain the observed slight deviations from the fundamental plane. Yet Dr. Braun's confidence in this *rationale* of the inclinations of the planetary orbits is so far from being unlimited that he holds in reserve, in case of its failure, other means for bringing about the same end.

Each planet is roughly estimated to have started on its career at about five times its present distance from the sun. In condensing, it descended towards it, sweeping up materials as it went, until finally almost the whole of the diffused gaseous stuff was concentrated in sun and planets, and the intervening spaces were void. By that time, too, tangential velocity had come to balance gravity, and the slow inward approach ceased. But the resistance met with in the earlier stages of its history by the growing and falling planet had had one result of vital importance to its future. It had imparted to it a movement of rotation. As it settled down in close spirals towards its present orbit, its velocity must everywhere have exceeded, by a small amount, the velocity in the same direction of the medium in which it moved. The density of that medium must, however, have increased towards the sun; and the embryo-planet consequently experienced a slight excess of resistance on its inner side, resulting in a direct whirling movement. Dr. Braun endeavours to show that the rotation thus set on foot must have belonged chiefly to the external layers of the planetary nebula. His motive is that of conciliating the swift circulation of satellites yet to be born from it with the comparatively sluggish spinning of the parent mass.

Tidal friction he rejects as an agent of planetary development, attributing to it barely the power to have rendered absolute an already approximate coincidence between the periods of rotation and revolution of satellites. Perhaps he might here be induced to reconsider his position. At least in the case of the lunar-terrestrial system, the evidence is overwhelming that tidal friction was largely concerned in bringing about its present condition. We may further assure him that Prof. G. H. Darwin (whom he evidently identifies with the late Charles Darwin, his father) has not committed the blunder he imputes to him of ascribing to the moon a *shorter* period of revolution than that of the earth's rotation, at the time when it began, under the reactive influence of the tidal wave, to travel slowly outward from near its surface. On the contrary, a slightly inferior angular velocity in the satellite is the assumed starting-point of all his subsequent reasoning on the subject.

For the completion of the solar system in its minor details, Dr. Braun is driven to the expedient of collisions with some of the many nebulous fragments which continued to be drawn towards it from unfathomed depths of space. Most of these became incorporated with the sun, but a certain proportion must have been intercepted by the planets, which, in their forming state, as possessing less mass and velocity, were more sensitive to such shocks than when fully formed. Thus, the plane of the ecliptic might have been altered, we are told, 1° by the impact upon the inchoate earth of a body possessing 1/1000 its present mass. Facilities even greater were offered for changing the elements of rotation; that of the earth, when of seventy times its actual radius, might even have been stopped altogether, by collision, under specially favourable circumstances, with a mass only 1/10600 the terrestrial.

But this method of explanation is radically unsatisfactory. It suggests the *Deus ex machinâ* of an unskilled dramatist, and cannot be admitted without protest into scientific speculation. We have learned to regard cometary impacts as the last resource of the distressed cosmogonist. Such events are not impossible, but to resort to them in difficulties is to throw up the game of ordered inference. The conviction remains unalterable that the results visible to us were brought about by means less apparently fortuitous. Dr. Braun, for example, is obliged to suppose not only that, before the separation of the moon, the axis of the lunar-terrestrial nebula was deviated, by extraneous agencies of the kind indicated, to the extent of 5° from its original position of perpendicularity to the plane of the ecliptic; but that, subsequently to the separation, further shocks continued the process upon the earth alone until the inclination attained its present value of $23\frac{1}{2}°$. Still less admissibly, the solar axis is assumed, after the formation of Venus, to have been tilted 5° by a number of successive impacts. A transcendent degree of improbability seems to be reached by this conjecture.

In the order of planetary production, Dr. Braun follows Laplace. Neptune is his oldest planet. And the fact that it revolves very nearly in the "invariable plane," of the solar system is confirmatory of the view that it really was the first body (instead of being the last, as M. Faye supposes) to become severed from the primitive nebula, the rotation of which is likely to have been conducted in that plane (Wolf, *Bull. Astronomique*, t. ii. p. 228). Neptune alone, owing to the distinction of his retrograde rotation, is allowed by our author to have been formed by the detachment and eventual condensation of a nebulous ring. But Prof. Kirkwood has raised an objection to this orthodox mode of genesis which applies with especial force to the remotest planet. The coalescence into a single globe of the fragments of a broken-up ring, if it happened at all (which is uncertain) would, it appears, have been an unconscionably slow process. Thus, two opposite portions of a ring of the dimensions of Neptune's orbit, could scarcely have come together in less than 150,000,000 years. It must be admitted that this is a startling demand on the time-exchequer even of the cosmos.

Uranus is regarded by Dr. Braun as what Bacon called a "limiting instance" between the annular and the nuclear methods of generation. An abortive ring gave place to a centre of condensation, the result (helped, perhaps, by some well-aimed cometary shoves) being the indecisive character of the Uranian rotation on an axis lying prone in the plane of revolution.

These, then, are the main outlines of the last and newest cosmogony. While dissenting from some of its conclusions, we readily admit that it is, in several ways, a noteworthy effort. Its appearance may be said to mark the definitive abandonment, by sound thinkers, of the annular method of planet and satellite formation. The preciseness of the conditions of that celebrated hypothesis lent it its charm, but has proved its ruin. Had they been less definite, it might have lived longer. But it gave, as it were, hostages to the future which it has not been able to redeem.

It is gradually becoming clear that, while the various members of the solar family owned unquestionably a common origin, they can scarcely be said to have had a common history. Each ran through a cycle of development particular to itself, and appointed, without doubt, to adapt it to a special purpose. The biography of the earth and moon, as narrated by Prof. Darwin, is an example. Here influences predominated which, in every other secondary system, were comparatively unfelt.

This growing persuasion of what we may call planetary individuality is reflected in Dr. Braun's vigorous and original chapters. He has honestly, and with no small ability, worked out *ab initio* the problems that they deal with, and he finds them insoluble by the uniformitarian method of treatment. The expedients by which he seeks to obtain a diversity of results by introducing a diversity of vicissitudes, strike us perhaps as arbitrary and awkward; but the admission of their necessity by an inquirer of such acuteness, and so well abreast of contemporary scientific thought, is highly instructive. We shall return later to the part of his interesting work devoted to solar theory.

A. M. CLERKE.

NOTES.

ON Tuesday Lord Hartington introduced to Sir W. Hart Dyke a deputation consisting of Sir Henry Roscoe, Sir Lyon Playfair, Mr. Rathbone, Mr. G. Howell, Mr. Cyril Flower, Sir B. Samuelson, and other gentlemen interested in education. They urged the desirability of the Technical Education Bill being passed, and of certain changes being made in the measure. Sir W. Hart Dyke replied favourably on both points. He was of opinion that the prospects of the Bill were good, both in the House of Commons and in the House of Lords.

THE list of foreign men of science who have accepted the invitation of the Local Committee to attend the Manchester meeting of the British Association is steadily increasing, and now numbers considerably over a hundred. Amongst those whose names have been received since the list we published on July 7, we note Prof. Cremona, of Rome; Dr. Neumayer, Director of the Hamburg Marine Observatory; H. A. Rowland, Baltimore; Dr. Werner Siemens, of Berlin; Prof. Horstman, Heidelberg; Prof. L. Weber, Breslau; Prof. Capellini, Bologna; Prof. Carvill Lewis, Philadelphia; Prof. O. Bütschli, Heidelberg; Prof. Carnoy, Louvain; Prof. Erb, Heidelberg; Dr. Treub, Director of the Botanic Gardens, Java; Capt. Coquilhat, Brussels; Dr. Godefroi, 's Hertogenbosch; Dr. Ludwig Wolf, Leipzig; Signor Bonghi, the late Italian Minister of Education; Signor Luzzati, Rome; Dr. E. Atkinson, Director of the Massachusetts Bureau of Statistics; and Dr. G. Boissevain, Amsterdam. The King of the Belgians has intimated his intention of nominating two representatives of the Congo Free State to attend the meeting, and of these General Strauch, Administrator-General of the Congo Free State, is expected to be one. A correspondent in Paris writes to us that the Emperor of Brazil, who has lately spent some time in the French capital, will probably attend the meeting of the Association.

MR. A. T. ATCHISON, Secretary of the British Association, is authorized to state that at the Manchester meeting space will be provided in the galleries of the Reception Room for the exhibition of specimens and instruments shown in connexion with and in illustration of papers read in the Sections. To secure admission a brief description of the specimens or instruments must be submitted to the Local Secretaries not later than August 10, together with a statement of the dimensions of the table or other fixture required. No motor power will be available. The objects must be exhibited at the risk of the owners;

and the Local Committee, while it will endeavour to meet all reasonable wishes, reserves to itself the right to exclude all exhibits that may appear to it to be for any reason unsuitable. No objects shown for advertising purposes will be admissible.

THE summer meetings of the Institution of Mechanical Engineers were held this week on Tuesday and Wednesday in the University of Edinburgh, under the presidency of Mr. E. H. Carbutt. After the meeting on Tuesday, the members inspected the Forth Bridge and Works.

ON Saturday, July 30, the statue of Paul Broca, close to the Medical School, Paris, was unveiled. Addresses were delivered by different persons connected with the Anthropological School.

A CHAIR of Sanitary Engineering has been established in the Imperial University of Japan. It is believed to be the only Chair of the kind at present in existence. The Professor appointed to fill it is Mr. W. K. Burton, lately senior Sanitary Engineer to the London Sanitary Protection Association.

THE geographical habitat of *Peripatus leuckarti*, Sänger, is said (*Archiv für Naturg.* 1871, p. 407) by Prof. Leuckart to be "New Holland." Owing to the vagueness of our knowledge of the subject, it may be of interest to state that two specimens of what appears to be this species have been found in the Queensland scrubs, near Wide Bay. These specimens have been presented to the British Museum by Mr. E. P. Ramsay, of the Australian Museum, Sydney.

THE announcement of the discovery of more than a dozen new elements appears at first sight rather sensational, and were it not that the names of Krüss and Nilson are sufficient guarantees of its authenticity the intimation would probably be received with more than a little caution. Such, however, is the startling result of the long, laborious, and exceptionally difficult researches of the Swedish chemists upon the nature of the rare earths contained in several sparsely-distributed minerals, and a detailed account of their labours will be found in the number of the *Berichte* just issued. A precise measurement of the wave-lengths of the lines and bands in the absorption-spectra of the nitrates of the rare earths contained in thorite of Brevig and Arendal, wöhlerite of Brevig, cerite of Bastnäs, fergusonite of Arendal and Ytterby, and in euxenite of Arendal and Hitterö, has resulted in the surprising observation that only a particular few of the lines supposed by former observers to belong to the nitrate of any one metal are present in the absorption-spectra of the nitrates derived from certain minerals, the other lines being absent in these, but present in the nitrate spectra of other minerals, while some that are present in the former are absent or very faint in the latter. For example, only one of the lines supposed to belong to the nitrate of holmium is present in any intensity in the spectrum of the nitrates from thorite of Brevig, while in the spectrum of nitrates derived from other minerals it is but an insignificant line among several holmium lines much more intense. The conclusion from most exhaustive spectral measurements is inevitable, that most of the so-called elements in these minerals are compound substances, the various ingredients of which are present in certain minerals and absent in others; further, the fractionation of the nitrates has led to the partial separation of a large number of the components themselves. Holmium, the metal which Soret called X, and which Lecoq de Boisbaudran separated into two components, is now shown to consist of seven distinct elements—$X\alpha$, $X\beta$, $X\gamma$, $X\delta$, $X\epsilon$, $X\zeta$, and $X\eta$; erbium of two—$Er\alpha$ and $Er\beta$, which it is possible to separate by fractionation; thulium, named by Cleve in 1879, also of two—$Tm\alpha$ and $Tm\beta$; didymium which was shown by von Welsbach to consist of two components, praseodym and neodym, must, in the light of these spectral differences, consist of not less than nine distinct elements, while samarium, the name given by Lecoq de Boisbaudran to Marignac's $Y\beta$, is composed of at least two components—$Sm\alpha$ and $Sm\beta$. Hence, in place of holmium, erbium, thulium, didymium, and samarium, we are constrained to accept the existence of more than twenty elements, in the work of completing the isolation of which Drs. Krüss and Nilson urgently invite assistance.

THE United States *Monthly Weather Review* for March and April contains interesting notes, *e.g.* (1) Average storm-tracks over the United States during March, compiled from observations for the years 1873–85. The paths pursued by these storm-centres are divided into four distinct classes, and are traced until the disappearance of the storms at various points on the Atlantic coast. (2) Rain frequency and wind rose for April, with charts constructed from all observations available from the commencement of the records until the end of 1886. The charts have been prepared for use in the current weather predictions of the service. The Reviews also contain descriptions of the storms which occurred over the North Atlantic during each month, and their approximate paths are shown on charts, together with the distribution of icebergs reported.

THE *Jahres-Bericht* of the Central Meteorological Office of the Grand Duchy of Baden, for 1886, contains the results of meteorological observations at sixteen stations of the second and third orders, and twenty-nine rain stations, of which the positions and heights above sea are given. The Central Office has taken part in two Conferences during the year—one at Munich, relative to the investigation of the frequency, direction of propagation, and intensity of hailstorms; and the other at Friedrichshafen, with respect to a physical survey of the Lake of Constance. The proposals made at both Conferences are awaiting the decisions of the various Governments concerned. The Report is accompanied by a chart giving the distribution of the rainfall during the year 1886, and shows three districts with maxima exceeding 55 inches, in positions corresponding with those on the chart of the previous year. The greatest amount was 79·56 inches at Todtnauberg, and the least 30·28 inches at Diedesheim. It also contains hydrological observations at various stations on the Rhine and its larger tributaries.

DR. K. WEIHRAUCH, Director of the Observatory at Dorpat, Russia, has published the mean values of the meteorological observations at that place for the twenty years 1866–85, giving (1) the results of the individual months and years, and (2) the results for the twenty years combined. This Observatory was established in December 1885 by the late Dr. A. von Oettingen, and is one of the few stations that have persistently published wind *components*, under each of the points N. E. S. W. (in addition to the usual components N. – S., and E. – W.), whereby a better knowledge of the general distribution of the wind is obtained than when only two components are given. The highest mean monthly shade temperature, for the twenty years, is 63°·2 in July, the lowest 19°·6 in January, and the mean for the year 39°·9. The greatest mean monthly rainfall is 3·64 inches in July, the lowest 1·02 inches in March, and the total for the year 16·21 inches. The fourth volume of the actual observations, for the years 1881–85, is now being printed, the publication having been delayed hitherto for want of funds.

THE work done at the Melbourne Observatory in connexion with meteorology and terrestrial magnetism expands a little every year. The importance attaching to rainfall and water-supply renders it necessary to spread rain-gauges wherever trustworthy observers can be secured, and we learn from the latest Report of the Observatory that 272 monthly returns are now received, most of the observers being volunteers. A complete register of Victorian rainfall has been prepared, showing at a glance the annual and monthly rainfall, as well as the averages for a series of years and months. The issue of weather-maps and forecasts for Southern and Northern Victoria has been regularly continued, and this service appears to be fully appreciated.

MR. R. H. SCOTT, of the Meteorological Office, sends us some notices of earthquakes observed at North Unst and Sumburgh Lighthouses. These notices he has come across while examining Journals of Shetland Lighthouses, and it may be worth while to put them on record. The following are records from logs at North Unst Lighthouse: 1879, January 4, 5 minutes past 1 p.m. mean time, felt smart shock of earthquake, lasted about 4 seconds; 1880, July 18, 20 minutes after midnight last, a smart shock of earthquake lasted from 34 to 35 seconds, then a second shock not so strong or of so long duration,—whole rock and building oscillated; 1885, September 26, at 10 p.m., we felt the tower shake very suddenly,—men in bed as well as the man on the watch cannot account for it, unless a slight shock of an earthquake,—no heavy sea, and the wind light from north. At Sumburgh Head Lighthouse the following observation was made: 1876, November 28, at 6 p.m., a slight shock of earthquake was felt at this station. For two or three seconds the lamp-glass in the tower shook violently. As my attention was taken up with the lamp, and fearing that the glass would fall, I therefore did not observe any other movement. The wind at the time was north by east, and light, accompanied with showers, and dark clouds hanging about.

SOME time ago (vol. xxxiii. p. 491) we gave an account of some excellent papers on "Technical Education, Applied Science, Buildings, Fittings, and Sanitation," by Mr. Edward Cookworthy Robins. These papers, revised and admirably illustrated, have now been brought together in a handsome volume entitled "Technical School and College Building," and published by Messrs. Whittaker and Co. The book is dedicated to Prof. Huxley. It will be very welcome to all who are engaged, or who look forward to being engaged, in the construction of technical school buildings.

A BOOK on "Canada and Newfoundland," by Ernst von Hesse-Wartegg, is about to be published in Freiburg-im-Breisgau. The author has repeatedly visited the country he describes, and his work is the more likely to be appreciated in Germany, because the northern part of the American continent has hitherto been almost wholly neglected by German writers of books of travel.

THE first number of the American *Journal of Psychology* will appear early in October. The object of the *Journal* is to record psychological work of a scientific as distinct from a speculative character.

PROF. GEO. H. PALMER, of Harvard College, has published the results of some inquiries lately made by him as to the annual expenditure of Harvard undergraduates. Of the members of the graduating class, about one-fourth had spent from 400 to 650 dollars; another fourth, from 650 to 975 dollars; another, from 975 to 1200 dollars; and another, upwards of 1200 dollars. The lowest sum reported was 400 dollars; the highest, 4000. Addressing parents, Prof. Palmer says:—"If your son is something like an artist in economy, he may live at Harvard under 600 dollars a year. If he is able to live closely, carefully, and yet with due regard to all that he requires, he may easily accomplish it on between 600 and 800 dollars. If you wish him to live here at ease, from 800 to 1000 dollars may be well expended. I should be very confident that every dollar given him over 1200 dollars was a dollar of danger."

IT seems, from a paper by M. Jammes, who lives in Camboja, that animals, as well as human beings, are liable to become addicted to opium-poisoning. In countries where the use of opium is prevalent, many animals remain beside their master while he takes a whiff at his pipe. Breathing an air containing a good deal of opium vapour, they become quite intoxicated, and appear to relish the sensation. This has been noticed concerning cats, dogs, and monkeys. The latter, according to M. Jammes, like the sensation so much that some of them take to eating opium.

THE Foreign Department of the Chinese Government (the Tsung-li-Yamên) has just addressed a very striking memorial to the Emperor proposing the introduction of mathematics and physics into the metropolitan and provincial competitive examinations for public employment. It is suggested that this should be done in all the provinces of the Empire, the successful candidates being sent to Pekin to compete for higher grades. They are to be examined in the capital, in addition to the preceding subjects, in civil and military engineering, international law and history. Those who are successful in obtaining the highest degree will receive an honorary official status, equivalent to a Fellowship, in the Foreign Language College at Pekin, together with official appointments in the capital or the provinces. This scheme has now received the Imperial sanction, and it is difficult for those unacquainted with China to appreciate the vast change which it will produce in time. Hitherto the competitive examinations which must be passed by every intending official, have been confined to the ancient Chinese classics, exercises in prose and poetical composition, and Chinese history, and they have been of the same kind for centuries. They were the ark of the covenant, which it was sacrilege to touch; but the Chinese have now introduced mathematical science into the curriculum. It will be interesting to see how the new and old subjects will fare respectively, now that they are brought together for the first time in the long history of China.

THE first volume of a new periodical specially devoted to botany has been issued in St. Petersburg. The periodical is published in connexion with the botanical garden of the St. Petersburg University by Profs. Beketoff and Gobi, under the title of "Scripta Botanica Horti Universitatis Petropolitanæ." The first volume contains an important work by Prof. Beketoff, on the flora of Yekaterinoslav, which not only gives a list of 1032 species of flowering plants (instead of the 536 species formerly found in the province), but contains a most interesting inquiry into the flora of the steppes of South Russia, as compared with those of the Hungarian *pusztas* on the one side, and the Caspian steppes on the other. The same volume contains a note by Prof. Gobi on a new form of fungi, *Cæoma Cassandræ*, which is found in the peat-marshes of Finland as a parasite on the *Andromeda* (*Cassandra*) *calyculata*; and a paper on the vegetation of the Altai, by A. Krasnoff, containing the enumeration of plants found by the author. These plants were found on the *Artemisia* steppes, on the salt steppes, on black-earth, on meadows inundated during the spring, in forests, and on the higher Alps. While comparing the present flora of the Altai with the Pliocene flora of the same area, characterized by the predominance of the beech and other trees of the temperate region, the author points out that only feeble vestiges of the old flora survive in the present flora of the meadows inundated during the spring. The vegetation of the other sub-regions has completely changed since the Tertiary period, and continues still to change. Thus larch forests disappear with astonishing rapidity, and are succeeded by herbaceous steppes, while *Artemisia* steppes have taken the place formerly occupied by the lakes and brackish marshes which covered the bottoms of the valleys. A feature of the "Scripta Botanica," most welcome to European botanists, is that the papers in Russian are followed by short abstracts in French or German. In the bibliographical section there are analyses of botanical works published in Russia since January 1, 1886. The works analyzed under this head are: Prof. Schmalhausen's "Flora of South-West Russia" (Kieff, Volhynia, Podolia, Poltava, Tchernigoff, and neighbouring regions), published at Kieff; Prof. W. Zinger's work on the "Flora of Central Russia;" Prof. Maximowicz's "Diagnoses Plantarum Novarum Asiaticarum," fascicule vi.; M. Kamenski's "Comparative Researches into the Development and Structure of Urticularia;" and several botanical papers published in Russian scientific periodicals.

THE additions to the Zoological Society's Gardens during the past week include two White-eared Bulbuls (*Pycnonotus leucotis*) from North-West India, presented by General W. H. Breton; a Magpie (*Pica rustica*), British, presented by Mr. H. Stacy Marks, R.A., F.Z.S.; two Turtle Doves (*Turtur communis*), British, presented by Mr. N. Brooks; a Daubenton's Curassow (*Crax daubentoni*) from Venezuela, presented by Capt. Rigaud, s.s. *Larne*; a Loggerhead Turtle (*Thalassochelys caouana*) from the Atlantic Ocean, presented by Mr. R. T. Ward; two Green Lizards (*Lacerta viridis*); two Marbled Newts (*Molge marmorata*), European, presented by the Rev. F. W. Haines; a Crested Pigeon (*Ocyphaps lophotes*) from Australia; a Secretary Vulture (*Serpentarius reptilivorus*) from South Africa; an Elliot's Pheasant (*Phasianus ellioti* ♂); a Temminck's Tragopan (*Ceriornis temmincki* ♂) from China; four Spotted Tinamous (*Nothura maculosa*) from Buenos Ayres; two Indian Crocodiles (*Crocodilus palustris*) from India, deposited; eight Ocellated Sand Skinks (*Seps ocellatus*) from Malta, purchased; a Bennett's Wallaby (*Halmaturus bennetti* ♂) born in the Gardens; a Common Crowned Pigeon (*Goura coronata*), a Cockateel (*Calopsitta novæ-hollandiæ*) bred in the Gardens.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1887 *AUGUST* 7–13.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on August 7

Sun rises, 4h. 35m.; souths, 12h. 5m. 33·8s.; sets, 19h. 36m.; decl. on meridian, 16° 27′ N.: Sidereal Time at Sunset, 16h. 40m.

Moon (at Last Quarter on August 11) rises, 20h. 58m.*; souths, 2h. 29m.; sets, 8h. 9m.; decl. on meridian, 6° 14′ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Decl. on meridian. ° ′
Mercury	3 36	11 7	18 38	16 31 N.
Venus	8 44	14 42	20 40	1 14 S.
Mars	1 59	10 13	18 27	23 8 N.
Jupiter	11 32	16 45	21 58	10 3 S.
Saturn	3 3	11 0	18 57	20 37 N.

* Indicates that the rising is that of the preceding evening.

Occultations of Stars by the Moon (visible at Greenwich).

August.	Star.	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image. ° °
8	B.A.C. 81	6½	3 53	4 57	88 353
8	26 Ceti	6½	23 57	1 2†	105 240
9	29 Ceti	6½	3 24	4 5	172 240
13	48 Tauri	6	2 18	3 24	76 244

† Occurs on the following morning.

August.	h.	
8	1	Mercury stationary.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·3	81 16 N.	Aug. 11,	20 49	*m*
R Persei	3 22·8	35 17 N.	,, 9,		*M*
S Virginis	13 27·1	6 37 S.	,, 10,		*m*
U Coronæ	15 13·6	32 4 N.	,, 9,	3 11	*m*
U Ophiuchi	17 10·8	1 20 N.	,, 11,	1 42	*m*
			,,	21 50	*m*
X Sagittarii	17 40·5	27 47 S.	,, 10,	21 0	*m*
T Serpentis	18 23·3	6 14 N.	,, 12,		*M*
β Lyræ	18 45·9	33 14 N.	,, 10,	23 0	*m*
S Sagittæ	19 50·9	16 20 N.	,, 8,	21 0	*M*
S Cygni	20 3·1	57 40 N.	,, 13,		*M*
δ Cephei	22 25·0	57 50 N.	,, 10,	21 0	*M*

M signifies maximum; *m* minimum.

Meteor-Showers.

The present season is generally the richest in the year for meteors, being the season of the *Perseids*, and the neighbouring showers.

	R.A. °	Decl. °	
Near δ Andromedæ	7	32 N.	Swift; streaks.
Perseids	44	56 N.	Swift; streaks.
From Aries	44	25 N.	Swift; streaks.
Near β Persei	48	43 N.	Very swift; streaks.
From Camelopardus	96	71 N.	Slow.
Near θ Cygni	292	52 N.	Rather slow.

THE JUBILEE OF THE ELECTRIC TELEGRAPH.

ON December 12, 1837, William Fothergill Cooke, on behalf of himself and Charles Wheatstone, set his hand and seal to Patent No. 7390, the subject of the specification being: "Certain apparatus or mechanism which is constructed according to our said improvements for giving signals and sounding alarums in distant places by means of electric currents transmitted through metallic circuits." This, the first English patent dealing with the electric telegraph, contains the elements of a thoroughly practical apparatus, as the historical experiment of July 25, 1837, made between Euston and Camden Town, had proved. Unlike many other developments of practical science, the commencement of the epoch when electric telegraphy became a practical success in this country can be sharply defined, and what will become an historical event, viz. the commemoration of July 27, 1887, can strictly be said to be the true jubilee of the electric telegraph.

To say that the invention of which Cooke and Wheatstone were the pioneers has done more to transform the conditions of human existence than any other except the steam-engine, and some would add "gunpowder," is but to restate an acknowledged fact. The electric telegraph has so changed the conditions of our social existence as to become indispensable to the same, and we could almost as soon do without food and clothing as dispense with the power that has annihilated distance. The evolution of the electric telegraph as a means of transmitting intelligence from a distance did not, of course, commence from the year when Her Majesty began her reign. The names of Ronald, Schilling, Watson, Sömmering, Schweigger, Weber, Lesage, and very many others, will at once occur to those who give a moment's thought to the subject, as workers in the field long before Wheatstone made his famous experiment, but few, we think, will question the assertion that electric telegraphy as a commercial success distinctly dates from the year 1837.

The commemorative dinner was held in the Venetian Hall at the Holborn Restaurant on Wednesday evening, July 27, the Right Hon. H. C. Raikes, M.P., the Postmaster-General, being in the chair. A large number of representative men were present (the company mustering about 250), though during the last ten years or so death has sadly thinned the ranks of the "old hands," of the Electric, U.K. and Magnetic Companies. Amongst the men of science and others who attended were Capt. Fonseca Varz, Mr. S. W. Silver, Dr. J. H. Gladstone, Mr. C. B. Bruce, the Marquis of Tweeddale, Mr. William Crookes, F.R.S., Mr. Edward Graves, Prof. W. G. Adams, M. Caël, Mr. Jacob Brett, Mr. H. Weaver, Mr. John Pender, Mr. C. H. B. Patey, Mr. G. Shaw-Lefevre, M.P., Lord Onslow, Prof. Stokes, Sir Lyon Playfair, M.P., Sir William Thomson, Sir Frederick Goldsmid, Sir Frederick Abel, Sir Douglas Galton, Mr. J. C. MacDonald, Mr. Edwin Clark, Sir David Salomons, Sir George Elliott, Colonel Andrews, R.A., Mr. Matthew Gray, Sir James Anderson, Mr. Norman Lockyer, Mr. H. C. Fischer, Prof. Hughes, Mr. W. H. Preece, Sir C. Bright, Major-General Webber, Mr. C. E. Spagnoletti, and Mr. Latimer Clark. Letters expressing regret at non-attendance were received from the Marquis of Salisbury, Lord John Manners, Viscount Cross, Mr. W. H. Smith, Sir H. Holland, Sir W. Grove, Sir D. Gooch, Sir A. Borthwick, M.P., Dr. von Stephan (Berlin), Dr. William Siemens (Berlin), Mr. Cracknell (Sydney), Mr. C. Todd (Adelaide), and others.

The Chairman having proposed the usual loyal toasts, proposed the toast, "To the Memory of the Pioneers of Telegraphy," asking the company to join in an expression of reverence for those great and illustrious men who have ceased to be among us, by drinking the same in solemn silence.

The Chairman then said:—We have most of us perhaps read of that tumultuous sensation which the great Wheatstone confesses to have experienced when the message which he sent on that little journey from Euston Square to Camden Town was sent back to him by Mr. Cooke. I am perhaps not exaggerating the importance of that occasion when I venture to say that that evening as Wheatstone sat in the small cupboard of an office communicating with his colleague at a distance of some two miles, was one of the great epochs in the history of human progress; and if ever a spirit of prophecy has filled a man with something of a divine enthusiasm, it may well be that the man with whose name the system of the electric telegraph must ever be inseparably connected, may have felt his heart throb with something almost supernatural when he realized that the great work had been achieved, that the demonstration had been reached, and that the future of the science was assured. I venture to believe that when we look back upon the progress of those fifty years, we shall find in them the materials for a greater hope of the future of humanity than in almost any other record of any other period in the history of our race. I would remind you that the instrument which was employed by Wheatstone and Cooke displayed five needles, and that it was from the movements and combinations of those five needles that the whole of the alphabet was made up. Those five needles, we are told, were united by means of five copper wires laid in a groove in a triangular block of wood, and I am sure you will be interested to know that a piece of that block is in my hand at the present time, and well deserves to be preserved among the archives of science. Well of course we are with the experience of this half century well aware that this system in the first instance was crude and imperfect. Demonstrations had been arrived at, but perfection had to be reached. The difficulty that was immediately encountered in popularizing the system was obviated by the development of the railway enterprise of this country and the necessity which arose for rapid and certain communication along our lines of railway. However, some time elapsed before the real development of telegraphy in this country began. The London and Blackwall Railway was, I believe, the first to utilize the system in a practical way. In 1844 the Government of Sir Robert Peel were the first to realize how far the telegraph might be applied to the service of the State; and that year saw the establishment of a telegraph line from Waterloo to Gosport, and that I think you may say constituted the first public recognition of the value of the electric telegraph. In 1846 the first telegraph company was formed—the Electric Telegraph Company. In 1850 the first attempt was made to lay a submarine telegraph cable. A gutta-percha wire, without any metallic covering, was laid between Dover and Calais in August of that year, and you will be interested to know that I have also here a portion of that cable, which was fished up by a ship in the Channel so recently as the year 1875. In 1851 a cable was laid in substitution of this gutta-percha cable, which was protected by iron wires, and which was the commencement of a regular system of inter-communication between England and the Continent, and this cable I believe I am not wrong in associating with the name of one of those gentlemen who is happily still spared to be among us—I mean Mr. Crampton—and it must indeed be a great satisfaction to anyone who has been connected with great works of this sort to have lived, as Mr. Crampton has done, to witness their enormous development in the service of mankind. The first Atlantic cable was laid in 1858, and other companies arose during those years to compete with the first electric telegraph company, and multiplied throughout the length and breadth of England the agency of the telegraph. In 1870 the multiplication of the companies had become so great that their competition, though in some respects advantageous to the public, was yet so imperfectly regulated by State requirements, that the Government of the day determined to acquire the whole of their enterprises, and to place the telegraphs of the Kingdom under the direction of the Post Office. Now, I should like to say one or two words with regard to the instruments of telegraphy. We are aware that the first telegraphic apparatus employed by Wheatstone and Cooke was one which required five wires through which to transmit their message. It was found gradually that two wires would suffice to forward a message, and after that the progress of science enabled the operators to depend upon one. But after a time it became ascertained that a wire could be used for sending messages in two directions, and after that time four messages came to be transmitted on a single wire, two in either direction; and, as I dare say most gentlemen who are present to-night are aware, at the present time a single wire is being used at the central telegraph station in such a manner as to admit of six messages being sent in one direction, or one in one direction and five in the other, or any other combination of six messages. The first five-needle instrument was succeeded by the double needle, and the double needle by the single needle; all those systems were visual. Then came in the system of printing on a band of paper. At first the signs representing the letters were embossed on the band. This was further improved by Prof. Hughes's printing instrument, by which the actual letters were printed in ink. Then came yet another—the sounder instrument, by which messages are transmitted by sound without any record. With regard to speed, when the first electric telegraph was established the speed of transmission was from four to five words a minute on the five-needle instruments. In 1849 the average rate of transmission of a certain number of messages addressed to the *Times* newspaper was 17 words a minute. The present pace of the electric telegraph between London and Dublin, where the Wheatstone automatic instrument is employed, amounts to 462 words a minute, and thus what was regarded as miraculous fifty years ago, has multiplied a hundredfold in the course of one half century. Now you may perhaps like to know, though it is rather descending from the higher walk of this great subject, the number of telegrams which were sent through the Post Office in the United Kingdom last year. The number was 51,500,000; that is nearly 1,000,000 per week, and that number is still steadily increasing. 41,000,000, or rather more than that number, of these were inland messages, and of course a very great proportion of them were Press messages. I think you should realize the immense boon which the electric telegraph has bestowed upon the Press. I gather from such information as I have been able to obtain that the rate for Press messages, which as everybody is aware is very much less than the rate for other messages, is on the average not much more than 2*d.* per 100 words; and it is owing to these extraordinary facilities, afforded by the Post Office to the transmission of Press news, that the whole of the United Kingdom is put in possession at its breakfast table every morning of everything which it is necessary or important for anybody to know, as well as of a great many things which are neither necessary nor important. I believe that I am not wrong in saying that the cost to the public revenue of this reduced rate to the Press is not less than £200,000 a year, and that the newspapers of this country practically receive a subsidy of £200,000 a year in order to enable them to assist in the diffusion of intelligence. I imagine that the country is well satisfied that this should be so, and that there are very few people who would wish to abridge that privilege, having regard to the enormous importance to all classes of the community of being placed at the earliest moment in possession of the fullest knowledge of what is going on. But it is a fact that, owing to the recent reduction in the tariff of telegrams, the value of the telegram on the average to the State is now only 8*d.*, whereas two years ago it was 1*s.* 1*d.*; and before the State took over the telegraphs it amounted to as much as 2*s.* 2*d.* I think you may measure something of the enormous gain which the public has achieved by the acquisition by the State of the telegraph system when you look at these figures and reflect that the average price of a telegram at the present time is only about a third of what it was only seventeen years ago. I am saying this as if I were one of the public; but as Postmaster-General you must be aware that I have to regard this result with somewhat mixed feelings, and I am endeavouring, as far as I can, to denude myself of any official prepossession in putting before you from the popular side the advantages which you have obtained by the State employment of the telegraphs. I would add that if you would wish to obtain further knowledge of this most interesting subject, put in the most terse and pregnant way, you cannot do better than study a paper communicated to the Society of Arts by my friend Mr. Preece. The great agency of telegraphy which seems to form the vital principle of this planet upon which we live, this great agency which has not only gone so far towards annihilating space, but which seems at the present time to be regenerating light and revolutionizing motion, has all the future before it. Those who are enrolled in its service are probably the disciples and the apostles of a new and absolutely beneficial dispensation, and with them rests the future, in no small degree, of the human race, and the means of linking not merely ourselves to our distant colonies—and my noble friend who is beside me (Lord Onslow) reminds me how important is the connexion between England and the younger Englands beyond the sea—but by going forward in connecting the various races of mankind by binding island to island and continent to continent. The telegraph is doing in its own quiet, its own noiseless, and its own unobtrusive way, more than all the noisiest missionaries of peace and universal brotherhood have ever accomplished.

Mr. Edwin Clark, in acknowledging the toast on behalf of "The Past," described the origin of the Electric Telegraph Company, in the organization of which he took a prominent part, the difficulties he had to encounter in curtailing expenditure, and in putting into a thoroughly sound state the wires and the system of insulation which then prevailed. He pointed out that the railway companies were really in its early stages the greatest benefactors of the telegraph. He had been deputed to examine the telegraph system prevalent in Europe in those early years in connexion with the railways, and he had recommended what had now become universal on the railway system of this country—the block system.

Mr. John Pender, on behalf of "The Present," said :—My mission to-night is to tell you what submarine telegraphy has done. I am one of those few commercial men who at an early period of telegraphy saw that there was in it the promise of a beneficent instrument for the future progress of the world. I have gone into submarine telegraphy, not as an expert, but as one of those who have come forward and taken science by the hand, and led it up to the glorious results which we have seen. Twenty years ago there was only something like 2000 miles of submarine cables ; now there are 115,000 miles ; and it has cost something like £38,000,000 or £39,000,000 sterling to put that amount of telegraph cable to the bottom of the sea. There was a prophecy long ago that the earth was to be girdled round in forty minutes. Why, we have got as much submarine telegraph cable as will go round the world five times, and we can send a message round the world in twenty minutes at the present moment. You ask me where does all this submarine telegraphy extend, and I reply in those beautiful lines of the poet :—

"Far as the breeze can bear the billows' foam
Survey our Empire, and behold our home !"

Wherever the British ship goes, or the British flag flies, there we have the submarine telegraph ; and at the present moment, while I am speaking to you, human thought is travelling like lightning to every part of the world. The future of that no man can tell. Of the 100,000,000 words which are now carried by submarine telegraphy, nine-tenths are for commercial purposes. When the history of these fifty years of Her Majesty's glorious reign is written, telegraphy, and more especially submarine telegraphy, will be shown to have done more than anything else to federate the great colonies with the mother country, to spread civilization throughout the world, and to make this great world of ours as near as possible one common country.

Sir William Thomson (who was warmly received) said :—I feel that when the telegraph has been so important a bond for all the nations of the world we ought to go even beyond our fifty years jubilee and think for a moment of the great names from other countries to whom the possibility of the jubilee of the electric telegraph has been due—the great apostles of electric science in France, Coulomb and Ampère,—Ampère, whose work and whose discoveries constitute the foundation of the most important of modern telegraphic and electrical instruments generally ; Ampère, whose name has become Anglicized and is invariably used in measuring the currents which produce the electric light. Then Gauss and Weber, who made the first electric telegraph. The telegraph of Gauss and Weber, and the Munich telegraph of Steinheil, and the Steinheil key, which is the manipulating telegraph key of the present day—those were the elements of telegraphy. We justly rejoice that in England so much was made of the work of those grand pioneers in science. In America the race of practical work commenced almost simultaneously with our own in the splendid telegraph of Morse. In speaking of the telegraph we almost forget time and space, and I must go back to the previous work of Henry, who anticipated in some points some of the finest discoveries of Faraday, and laid a large part of the theory of current induction, which is at the very root of some of the most splendid realizations of modern electric science, not merely for the electric telegraph, but for electric lighting. By the work of 1857—a few years before the half of the jubilee—the two brothers, Edward and Charles Bright, and Whitehouse, those three men, with Mr. Cyrus Field, reduced to practice that brilliant dream of Cyrus Field to connect England and America by means of submarine telegraphy. Then there were the brothers Werner and William Siemens working in the same direction, and the great navigator Moriarty, who was out in the *Agamemnon* in 1857, and navigated the *Agamemnon* in 1868, and was on the *Great Eastern* as navigator with Sir James Anderson. In 1865 he picked up the cable where it was broken, and in 1866, coming back a year after to the same place, hit upon it just a quarter of a mile away, by his splendid navigational powers. Canning and Clifford were also engaged in the work ; then there were Varley and Jenkin (who was my special partner) with both of whom I worked for many years. I alone am here to speak for the three. Willoughby Smith, who did such fine work in 1865–66 in testing the cable, applying the newest developments of science, many of them his own inventions, to do what had never been done before, to test a submarine cable with the delicacy that was necessary under circumstances so peculiar, so utterly new. I am exceedingly sorry he is not with us this evening. But I can never forget that we scientists alone could not have done what has been done. To two men, I believe, is due the existence of the 1865 cable, and all the consequences that followed from the 1865–66 cable—John Pender and Cyrus Field. But I must remember that there are other things besides ocean telegraphs. You have told us how splendidly the land telegraphs are worked ; you have pointed out how admirably under the influence of the Government system, the application of science to telegraphy has been developed. I think you may feel proud, sir, in knowing that under Government management within these last seventeen years the applications of science to telegraphy have not stood still, but, on the contrary, have been pushed forward with every possible energy and with the most marvellous success. You have told us that the rate of working between Dublin and London has reached 462 words per minute, I think we may say 500 words per minute, and that is ten times what it was ten years ago. That is something for a Government department to be proud of, and for a Government I must say there is some little political importance in the fact that Dublin can now communicate its requests, its complaints, and its gratitudes, to London at the rate of 500 words per minute. It seems to me an ample demonstration of the utter scientific absurdity of any sentimental need for separate Parliaments in Ireland. I should have failed in my duty in speaking for science if I had omitted to point out this, which seems to me a great contribution of science to the political welfare of the world.

Sir Lyon Playfair, M.P., proposed "The Scientific Societies." The scientific Societies, he pointed out, did not profess invention ; they professed to lay down the laws of science and to advance natural knowledge. Men who had contributed to the advancement of natural knowledge, like Oersted and Ampère, were as much discoverers of the electric telegraph as if they had themselves actually aided in the invention. The duty of the scientific Societies was to cultivate the tree of knowledge. A great invention never came, as Minerva did, panoplied in complete armour from the brain of Jupiter. But even the brain of Jupiter could not produce this wonderful product of evolution until he had swallowed her mother, Metis, while in the first month of gestation. Our great inventors swallowed science, the mother of invention, and then produced their progeny, always, however, in a state of infancy. The steam-engine, which had done so much for human progress, has had so many inventors that a Court of law, reviewing the steps of invention, came to a solemn decision that Watt had done nothing for the improvement of the steam-engine. Scientific Societies, looking to the advancement of science for its own sake, laid the surest foundations for industrial applications. Oersted and Faraday were as true discoverers of the electric telegraph as Wheatstone, Cooke, or Morse. It was not the annihilation of space and time which was the most wonderful result of the telegraph, but it was the profound change which it had produced in our systems of government and of commerce. Who at its introduction would have supposed that the whole system of commerce would have been transformed by it, that capital would have to change the channels of its usefulness, and that the system of exchanges would undergo such profound alterations ? If telegraphy had one lesson which we should lay to heart it was this—that science should be studied for the sake of knowledge, because discoveries in natural knowledge led inevitably to industrial inventions. We should, in regard to all discoveries, however unimportant they seemed, do everything in our power to promote their growth, and the growth of natural knowledge throughout the world.

Prof. Stokes said :—Scientific men know well how fascinating is the pursuit of science. Some have even gone so far as to consider that it would be polluted, if I may so speak, by being applied practically. An eminent foreign mathematician delighted in the theory of numbers because one could not conceive that it could have any practical application. An eminent English mathematician I heard express a somewhat similar sentiment. All honour be to those who are so immersed, if I may so speak, in abstract science, that they disregard and even dislike its application. They pursue science with all the more zest, they pursue it in directions which possibly otherwise might not have been followed out, and possibly in the end their own investigations may admit of applications which they never dreamt of. But for my own part my tastes do not quite lie in that direction. I like to see science connected with applications thereof, no matter to what purpose. Now, when we apply abstract science to physical subjects, we are not only enabled to investigate natural phenomena in a manner which could not otherwise be done, but the study reacts on the most abstract parts of science, and enables us sometimes to see, as if it were by intuition, truths of an abstract nature, such as, for example, propositions in pure mathematics, which we perhaps should never have arrived at if we had not viewed them through the spectacles, so to speak, of their physical application. But this is not all. When science comes to be applied to the wants of life, scientific men are placed by the practical man in the condition of making experiments which oftentimes would otherwise be impossible. When science comes to be applied to commercial purposes, it then becomes possible to construct instruments on a scale the expensiveness of which would have been utterly prohibitory to the purely scientific man. But when these instruments are constructed, it may be, for commercial purposes, the scientific man on his part is able by the favour of those who have constructed them, or of those for whom they have been constructed, to make experiments with them which oftentimes are of great interest from a purely scientific point of view.

Dr. Gladstone, responding on behalf of the "Royal Institution, said :—At the Institution there were not merely memories binding them to all those who had passed away, but they had also many relics. They preserved the log-books of Davy, Faraday, and others, and not only that, but there a great number of pieces of wire, sealing-wax, and card, all damaged, and many other things which Faraday especially used to delight to work with and to carry out in the first experiments which were suggested by the ideas that were working in his brain, and these were preserved as germs of some of their great discoveries. And here he wanted to point out one of those germs connected with telegraphy. Those relics preserved at the Royal Institution were only worth originally a few pence or shillings. How different the monster wealth which had now become the capital of those great enterprises! As far as the Royal Institution was concerned, its connexion with the electric telegraph was to a certain extent not direct, and yet it was very real. Sir Humphry Davy worked there of course largely on galvanic electricity, but he belonged to the pre-telegraphic era. Faraday himself commenced to work early on these matters, and continued to try and image in his own mind what was taking place in these phenomena. It was an important point with him that the glories of science should conduce to the benefit of man. They knew his influence with Sir Charles Wheatstone was very great, and he got him into the dark chambers at the Royal Institution and talked to him about his investigations, and in the theatre brought before him some of those experiments which were afterwards performed with so much success in public. In one way and another Faraday had to do with the industrial applications of electricity, as well as with scientific discovery. They had the combination of the purely scientific man with the practical man, and then the two going together with slow, careful, conscientious investigation, followed by the energetic carrying out of the discovery in a form which benefited mankind.

Mr. Shaw-Lefevre, M.P., in proposing the "Societies representing Applied Science," said :—When I was a boy, at Eton, I recollect well the extension of the telegraph from London to Slough, and an incident of which you are all aware, the arrest of Tawell, which I believe brought the telegraph more into notice than anything else at that time. It might have been expected that the authorities of Eton, seeing a great invention of this kind brought to their door, might have been desirous of explaining it to the boys of the school, and might have been drawn out of their deep slumber of ages and done something for the scientific education of the boys then at Eton. But no thought of the kind ever entered into their minds, and the only notice taken of it at the time was that they set it as the subject for Greek verses. I and all the boys of my class commenced racking our brains to write some Greek iambics upon a subject of which we knew nothing, and to bring it into connexion with the mythology of the ancients, of which we knew a good deal. I refer to this for the purpose of showing you how little scienee was promoted then at our public schools. I am glad to say that things have been changed since then, but much has to be done in this direction, and there cannot be a doubt that if this country is to hold its own in the great industrial competition it must give a greater place to science in our schools, and equalize the endowments between the classical and scientific studies; and it is only by doing that I am persuaded that we can hold our own.

Mr. Bruce, President of the Institute of Civil Engineers, and Mr. Latimer Clark, past President of the Society of Telegraph Engineers, replied; and the latter, after alluding to the work done by the brothers Brett in submarine telegraphy, said :—I feel that we, as the representatives of applied science, have been somewhat neglected by the world. I feel that the politicians, some of whom have honoured us by coming here this evening, have very much neglected us. I don't allude to honours, for we shall be very well content with the position of things in that regard; but I feel that they have robbed us of much of our credit for the fact that the great effects of the jubilee which we are now assembled to commemorate have been due to the agency of the applied sciences. I do feel that politicians have been permitted to claim for themselves the credit for the wondrous benefits civilization has received from the efforts of applied science. We hear that so many shillings have been taken off a quarter of wheat, we hear that all the prosperity of the country has been due to free trade, but I say it is not so; I say they have robbed us of our honours in saying that; I say as guild and craft we ought to proclaim loudly to the world that to our efforts most of all the prosperity of the last fifty years has been due. The population of this great city and of this country when it eats its breakfast to-morrow morning will be consuming food a very large proportion of which has been brought to this country by means of applied science. It is that which has given us our prosperity. They have not taken 5*s*. or 10*s*. a quarter off wheat and corn and eatables, but they have taken off 60*s*., 80*s*., and 100*s*. Wheat will be brought to-morrow from places from which it could not have been brought fifty years ago for ten times what it now costs. As a guild and craft we ought to proclaim loudly that the benefits which we have conferred are the real cause of the prosperity of the great Victorian era which we meet here to celebrate.

The Earl of Onslow having proposed the health of the Chairman in a suitable address, and the Chairman having responded, the proceedings terminated.

The Postmaster-General, during the evening, despatched the following telegram to Sir Henry Ponsonby, at Osborne :—"A large dinner-party celebrating the jubilee of the electric telegraph, remember with gratitude and pride that all the progress has taken place in the happy and prosperous reign of Her Majesty and under her fostering care."

To this the following reply was received :—"The Queen thanks you for your telegram. It gives Her Majesty much pleasure to reflect on the improvements which have been made in Wheatstone's great invention, which was first practically tested in her reign."

THE CASE FOR A LONDON TEACHING UNIVERSITY.

THE questions connected with the proposal for the establishment of a Teaching University in London were discussed in a speech delivered by Sir George Young at the distribution of prizes in the Medical Faculty of University College, London, on June 1, and in a speech delivered by Dr. J. E. Erichsen at the distribution of prizes in the Faculties of Arts and Laws and of Science, at the same institution on June 30. As the case for a London Teaching University was stated by these two eminent authorities, we reprint part of what they had to say on the subject from the point of view of University and King's Colleges.

I.

Having referred to the drawbacks connected with "the system of separation between institution and institution concerning University matters" in London, Sir George Young went on to say:—

I will touch upon some of these drawbacks—drawbacks which, as I have said, I do not impute as matters of fault to any man or to any set of men, but to the mischiefs of the system; and I will draw my instances (and you are to consider that I could give you many others), as in duty bound upon this occasion, from the medical side of the question.

Well, gentlemen, in the first place we are brought face to face with a very serious and very unpleasant condition of things in the fact that several of our students, we find, are in the habit of leaving us from time to time in order to finish their course of study at other institutions where degrees are conferred, in order to qualify for those degrees. We have always been, as Broke said of the *Shannon*, "an unassuming ship," and I am not going to boast. Let us admit that there may be elsewhere teachers as eminent as those I see around me. Let us admit that there may be elsewhere possibilities of study comparable to those which are to be obtained in this place. But I will not admit—it is my duty to deny, and the point is conceded by others outside our limits—that there is anywhere a more eminent body of professors and instructors than that which has now, for two generations, led the van in matters of medical instruction of a university kind in this College. I will not admit that there is anywhere, in any part of the world, a field of study presenting greater opportunities to the student than that of London with its numerous medical schools, and with its numerous facilities for scientific study.

Next let me mention an evil, for which the University of London is not responsible—for which nothing, indeed, can be said to be responsible except the non-existence of that university which the University of London is not. Not only are medical schools, as we know well, dependent upon hospitals, but also, what is not so generally known to the public, hospitals are dependent for their administration upon medical schools. As London has spread and as one general hospital after another has been founded, each has attached to itself its own separate medical school. Each school must provide, in order to satisfy professional requirements, not merely that clinical teaching for the sake of which it is founded, but also scientific teaching of a multifarious and expensive character. In some of these schools, as is well known, it has been found impossible to provide this scientific teaching in a manner sufficiently effective for the purposes of the school. There need be no delicacy, gentlemen, in mentioning this, because, in fact, it has been most honourably acknowledged by several of these medical schools in their recent action. It was lately brought to our notice that in the case of several of them, they have practically, in some branches, given up the scientific training of their students, and have entered into an arrangement with the Government school at Kensington, by which their students should there receive that teaching which they found themselves unable to give. Well, gentlemen, at the Council of this College we had something to say—we had some objection to take—to that arrangement. With that I need not trouble you further than to say that we thought a Government department ought not to lend itself to an exclusive arrangement of this kind. We thought that it would have been better for the students themselves and for the public if the matter had been left open whether they should go to South Kensington for their chemistry and physiology, or come, if they so preferred it, to ourselves. But at the same time, gentlemen, you must not consider me in this matter to be impeaching the conduct of the other schools. As men of the world, we are quite aware that medical schools are to a certain extent rivals, and we cannot expect, merely because we asked it, that the natural jealousies of rivals should be allayed, and that a medical school in so delicate a matter should freely accept our offers of instruction for its students who, they might suppose, would perhaps be detached from their affection for their own school by frequenting this place. Well, gentlemen, what is the remedy that we should look to? I think that you will agree that we ought not, as a Council, to sit down and seek no remedy for such a state of things as this. Why, surely the remedy is that some central authority should be provided—some institution where we can meet our sister schools upon equal terms, not that wholesome emulations should be extinguished, but that the mischiefs which arise from their excess should be obviated, where, in fact, teachers and administrators might meet together for the purpose of arranging for improvements in medical education upon a common footing and without fear of mutual injury.

This, among other instances of the same kind, many of which I could give you, led some of us, as much as three years ago, into a long inquiry into the matter, and eventually into a movement for the foundation of a Teaching University in and for London. The year before last, at the meeting of the sister Faculties, the Dean of the Faculty of Science, Prof. Graham (whom, I hope, we shall see before long among us restored to health), called our attention to the movement and expressed his sympathy with it on the part of the Faculty. The President of the College, Lord Kimberley, expressed also his warm sympathy with the movement, and said (I am quoting his words, which will be found in the Report of the College of that date):—"There is nothing more dissatisfying to the minds of students and of educational men, than that in this great city there should not be some more complete establishment of some universal system. We may not see it accomplished. I do not suppose that anyone sees at present how the end is to be attained, but I am quite certain that it would be for the benefit of all the institutions of this great city that they should be gathered together and the teachers and managers brought into a close and immediate contact." With that encouragement we, many of us, took up the movement warmly, and it has now been brought to the practical stage of definite proposals and of a formal programme. We ask, in short, that the same privilege which has already been conceded to country colleges through the Victoria University shall be conceded also to us in London. Gentlemen, we cannot go to Manchester. We cannot so far ignore our position and our history as to seek for admittance from the offspring of our offspring. Besides, we ought not to be compelled to go to Manchester. The system of the Victoria University, as I have indicated, is that of an imperfect university, arising from its being scattered over several cities at great distances from each other. There is within our reach the more complete system of a localized university; for who will have the face to say that in this great population of something like four millions residing within limits admitting of daily intercourse there is not material enough—there is not ground enough—to support a local teaching university of its own?

There were working with us for a long time, in the course of this inquiry, several active members of the Convocation of the University who had themselves been interested in similar movements, and who desired to see the development of that University in the direction to which our hopes and wishes also pointed. By their exertions, and as a consequence of our movement, the Convocation and the Senate of the University of London have been brought separately to consider this matter, and have put forward from time to time certain proposals for what I must call a compromise. Those proposals have been officially communicated to us through our President, and have been, I need not say, carefully considered by the Council. They do not amount to that which we desire. They did not cover that which we claim. They are limited to this: in the first place, that there should be introduced in the Senate of the London University eight representatives of the four Faculties of the University—two to each—such representatives to be chosen by assemblies of the Faculties, and the assemblies of the Faculties to consist of representatives of the institutions throughout England which send up candidates to the University. There is also a provision for Boards of Studies to be constituted out of these same Faculties to exercise purely consultative and deliberative functions. I think that is all—all, that is to say, of a practical character. There is no doubt—and it is an important matter considered from the point of view of our argument—a proposal that in the preamble, in the objects of the University, there should be included a statement, that its purpose is to encourage education, especially in London; and there is also a proposal that as time and opportunity are given, it shall have power to found Professorships, provided that such Professorships shall not compete unfairly with our own. But, passing over these two latter proposals as not exactly of a practical character, I say that the proposal to admit eight gentlemen, representatives of assemblies the constitution of which is not very clear to me, upon the Senate of the University, and to appoint these Boards of Studies composed of representatives so gathered from all parts of England, is not what we want. It does not meet our views; and for several reasons.

In the first place, the representation which would be given to this College on such a system in the governing body of the

University, if it is to be a teaching university and not merely an examining one, is entirely insufficient. In the next place, the Boards of Studies, the Faculties, and the new members of the governing body, would represent, not London, but all England, and therefore would constitute an organization entirely different from that effective organization which we contemplate, which is to meet often, to take count of teaching-matters in London, and to do a great deal of work in the development of University education in this place. We therefore are obliged to reject these proposals, and to proceed, without any feelings of hostility and without any bitter words towards the University of London, to ask for that which we consider necessary for the effective carrying on of our own work.

Let us keep clearly before us what a teaching university really is. We need not go farther than this institution to see it, provided only that were added to us which we want—the power to confer degrees. It has not merely to provide for—it has to commend to its students the best possible teaching under the best teachers obtainable in all the subjects of the University. A university which by its very constitution is indifferent to methods of teaching and does not care how a man has obtained his knowledge, cannot be said to commend to its students any particular methods or ways of obtaining that knowledge. It rather has a contrary effect. Under these circumstances it is useless to try to set up an institution which shall combine a mixture of two principles—the principle which considers degrees merely as the marks of a liberal education, tested, no doubt, by an examination, but covering very much more than the mere showing of knowledge in examination, and the other, the rival system, which, giving up the testing of methods of education—giving up the marking of a regular education as beyond its scope—confines itself to the setting of a mark upon performance in the answering of examination-papers.

No, gentlemen, if the University of London were to move in this direction, it would spoil its own thoroughly good and honest work without doing ours. There will still remain when we have obtained this Charter, plenty of candidates for its degrees—plenty of work for it to do—plenty of honour to those who obtain them. But surely there is room for us by the side of it. There is room for an institution which shall comprise not merely this College, but King's College and the Medical Schools of London, and which, organizing them together as a Teaching University, shall give us that which we want for the efficiency of our work—an institution in which the teaching which we give is duly honoured—is not placed in an inferior position beside the teaching which is given by other universities and in other university colleges.

We ask, therefore, that a Charter to confer degrees upon all persons who have undergone a regular course of study in a college or medical school of the University and have passed the required examinations, shall be granted to a suitable governing body, upon which the governing bodies of this College and of King's College shall be properly represented, and upon which the teaching staff not merely of this College and of King's College, but also of the other Medical Schools of London, shall have their representatives. In order that the interests of the medical profession may be properly considered—in order that we may not seem to claim that which it is by no means our intention to claim—an unfair position for our own medical schools, we ask an alliance with the Royal College of Physicians and the Royal College of Surgeons, the official representatives of the medical profession in London, in order that by their means that representation may be secured upon the governing body of the University. It is obvious that from their number it would be difficult to represent directly the separate interests of eleven medical schools upon the governing body of a University; but, in so far as the teachers of the various schools have their voice—and that voice, I can assure you, we do not intend should be a small one, in the councils of the University—in so far, we shall consider the teachers of other medical schools entitled to rank on equal terms with our own. . . .

There is a movement at present on foot in the College of Physicians for a single-facultied University in London, or an institution in the nature of a University, for conferring medical degrees alone. That movement appears to us to be a part of our movement. By itself, and if the movers insist upon its being considered as essentially a separate movement, we could not look upon it with approval; for we believe that it would be fatal to the prosperity of our medical school. I will put it to you, gentlemen, How would you, the students in this College, regard a state of things under which you were called upon to work for a degree, either at the University of London at Burlington House, or at the Royal College of Physicians? If it were the case, as seems to be indicated, that the degree at Burlington House is to be connected with a very high, a somewhat unusually high, standard, and if the degree which is contemplated by the College of Physicians is to be that creditable average degree which I have indicated as one which, personally, I think ought to be established, do you not see that those medical schools, which like our own, aim at the highest teaching, would have serious difficulties in the matter? Here would be two systems in neither of which we had the least voice, two systems of examining Universities outside us competing for our students; and what would our Professors do? They would be called upon, now to train for one system and now for another, and perhaps to keep up double sets of classes, so constituted as to fit the arrangements of two rival bodies.

That is the position in which we should be placed. But if the movement on the part of the Royal College of Physicians (the Royal College of Surgeons joining in it) can be brought into accord with our own, then we shall have already obtained a part of what we seek. I will just mention one reason why I think it most desirable that you, the members of the medical profession, should take this matter seriously into consideration, and should exercise your influence with your colleagues in other institutions, in order that this point may be pressed home to them at the present stage. The visit which it was recently my duty to pay to the Privy Council Office, in order to obtain the forms necessary for carrying out our own proposals, revealed to me the fact that there exists already in that office a pile of petitions as high as this table against the proposal of the Royal College of Physicians. Now, gentlemen, against our proposals there is no petition and there is no movement. So far as I know, there is no objection in the world.

We do not conceal from ourselves that it is possible opposition may be offered as we go on. That opposition which above all others we should deprecate would be the opposition of the University of London. I have endeavoured to preserve a tone of friendship, such as I sincerely feel, towards that University. I most earnestly deprecate opposition on the part of that distinguished body to the movement which is now on foot for obtaining a University in and for London such as London ought to have. I trust it will not be led into the fatal track of the older Universities, which, by their interference, did not prevent, indeed, the foundation of the University of London, but undoubtedly spoiled it, fifty years ago. That such opposition may be apprehended by some of us we cannot ignore in consideration of the very serious matter to which I have lastly to call attention, the resignation, namely, of our President and of several members of the Council among us. Gentlemen, that these resignations have been to some extent a surprise to us, that they have been a serious cause of anxiety to us, must be obvious; but I think that they have been partly due to a misunderstanding of our aims. I think that the objections which have led to them will, to a large extent, vanish when our proposals come to be more carefully looked into. In the meantime, as for us who remain, we are not disheartened, we are not discouraged. We have at least the satisfaction, such as it is, that the Council of this College is now unanimous in the matter. We have the source of satisfaction which is afforded to us by the unanimous support of the general meeting of the College. We have at our backs the unanimous support of our distinguished body of Professors. We have at our side the unanimous assistance of the great College once our rival, but now our cordial ally. Besides King's College we have friends in every medical school in London, who are corresponding with us and working in the same direction as ourselves. We have friends and well-wishers, I may say further, in every University in England. We have friends in the Press, and we have supporters in the public, and we have received the most encouraging intimations that it will not be long before we are able to fill our depleted ranks in the Council with names which will inspire confidence, and which will materially assist us in carrying our work to a conclusion.

Finally, gentlemen, we have this more than any other as a source of encouragement—that we see our way—that we know the work that has to be done and realize the way in which we hope to effect it. Three years of study and perhaps scores of meetings and conferences have not left us entirely ignorant of the ground. We intend to make this institution greater, more splendid, more efficient, than it has been hitherto, and we expect to succeed, because we are labouring not merely for our own aggrandisement, but for the foundation of a University in and for London which will be of incalculable benefit to University education in this mighty centre of population where we live.

II.

Dealing with the objection that a new University cannot be necessary in the capital, since we have already got the University of London, Dr. Erichsen said :—

I wish to speak with the very greatest respect of the University of London, and I entertain the highest respect for the work that has been done in that great institution during the half century that it has been in existence ; and I think that everyone connected with University College must always speak and think of the University of London with that affection with which a parent looks at his child, the University of London being the outcome of University College. We may sometimes look upon it with that feeling of mixed affection and regret with which we contemplate a child that we think has not always been so grateful as it might have been for the favours received in its early life. But, however that may be, we all speak of the University of London with, and we all feel towards it, the greatest respect and a certain affection.

But the University of London is, in truth, not a university in any sense of the term. The title is misleading and is a misnomer. By a "university" is meant an association of teachers and of students, properly organized, destined for the increase and the transmission of all learning, of knowledge in all its branches, and containing complete Faculties of Arts and Laws, Science and Medicine, and empowered to grant degrees to those of its pupils who are found to be sufficiently qualified for such a distinction. The University of London never has pretended to be a teaching institution, and, so far as its present constitution is concerned, never can be a teaching institution. If it were to become a teaching university it would require to become so completely altered in its constitution as practically to become a new institution. The University of London has only performed one of the functions of a university—that of examining candidates for its degrees. It has performed that function admirably well. The examinations have been carried to a very high standard, so much so that the ordinary pass-examination in some subjects is almost an honours examination. Yet it is only a degree-giving institution, and not a university in the sense in which a university is generally known.

Nor is it "of London" ; for, as was truly said by the Dean of the Faculty of Science in that admirable report that we listened to at the commencement of these proceedings, it is an Imperial University, which draws its candidates from almost every part of the habitable world. It has nothing to do with London except that its head-quarters are situated in Burlington House ; but, so far as London is concerned, it might just as well be situated anywhere else. The University of London, then, does not, in any way, supply the want that we wish to fill. With regard to London itself, I may say this, that even as an examining institution the University of London does not supply the desire that has sprung up of late years for academic distinctions. It does not supply the desire amongst the inhabitants of London itself. I can speak of my own profession. Of late years there has been a craving in the medical profession for the possession of degrees. As Sir George Young stated very truly in distributing the prizes in the Medical Faculty about a month ago, if there were no degrees at all we should be none the worse for it ; but one may also state something like the converse of that proposition, that if everybody has got a degree nobody is a bit the better for it, and what is common to all can be an honour to none. However, that there exists a great desire for degrees and for academic distinctions there can be no doubt. Well, do the students of the medical schools in London go to the University of London for those degrees ? Not at all. They go elsewhere. They go to Edinburgh ; they go to Cambridge ; they go to Oxford. At the present moment there are about nineteen hundred medical students at the University of Edinburgh, and nearly seven hundred of them are English. They are attracted there not so much by the superiority of teaching, because—and I say it with all respect to the University of Edinburgh, to which I have reason to be very grateful—the teaching, high as it is, and excellent as it is in all its departments, is not better than the teaching in four or five of the principal medical schools in London ; but the students go there simply in order to obtain a degree, because at the end of their studies, instead of coming out as simple Mr. So-and-So, they come out as Dr. So-and-So. Well, the others who do not go to Edinburgh, go to Cambridge or go to Oxford ; and there is a very large medical school now at Cambridge also, frequented by young men who are desirous of obtaining the degree of that distinguished University. The following incident will show how little the University of London supplies the need for degrees which is felt by London medical men. A few weeks ago there was a vacancy at one of the large hospitals of London for an assistant physician. There were no less than twelve or fourteen candidates. They were all graduates of British universities, and out of this large number of candidates, all London men, educated more or less in London, and practising in London, and attending hospitals in London, there was only one candidate who was a graduate of the University of London. All the others were graduates either of Oxford or of Cambridge. I say, therefore, that men go away from London to get their degrees at the present day. They go to Edinburgh, they go to Oxford, they go to Cambridge, they go elsewhere ; but the vast majority do not go to the University of Loudon. That, as a degree-giving institution, does not supply the needs of London itself.

The proposed establishment of this new teaching and degree-giving university has been termed an act of hostility, a kind of declaration of war, against the University of London. Now I can say truly, speaking in the name of my fellow-members of the Council here, that there is no such feeling whatever. No such feeling has animated, I believe, any one of the Council or any person connected with this institution. This proposed university will compete, probably, to a certain extent, with the University of London, but it will compete much more with other universities. It will compete much more with the University of Edinburgh ; it will compete much more with the University of Cambridge. There is no direct competition intended with regard to the University of London. There is no reason why a new university should not be established. There is no more reason to complain of competition in the establishment of a new university than there is in the establishment of a new school. Every new school competes with every other school in existence. There is no more reason to complain of it than to complain of the introduction of a new member into any of the learned professions. Every man who becomes a lawyer or becomes a doctor may be said to compete with every existing lawyer or doctor. In the same sense the new university, if established, might be said to compete, more or less, with every existing university in the kingdom. In this case there is a competition of friendly rivalry, but nothing else ; and beyond that I cannot admit that there is any special competition with regard to any existing university.

If the University of London does not supply the want felt for higher education, how is that want to be supplied in London? There are only one or two methods. You must either take existing institutions, or you must create a *tertium quid*, and what that *tertium quid* may be I know not. But what existing institutions are we do know, and we do know that there are two institutions in this metropolis which for the last half century have been doing the only work in London that approaches to the higher education, or approaches in any way whatever to university education. They have done that work diligently and well under great difficulties and great disadvantages, but with a fair share of success. I mean this College and King's College. Those are the two institutions ; and by the combination of those two institutions we may fairly look for the establishment of a new university in London fully capable of discharging the functions of such an institution.

I happen, from circumstances, to be personally acquainted with, I believe, every university in the kingdom ; and I can say that so far as the equipment of universities is concerned, in the way of museums, laboratories, libraries, lecture-rooms, and all other appliances—what may be termed the "plant" of a university—these two institutions taken singly are equal to most ; this one certainly is, and taken in conjunction they are superior to almost all, except the old Universities of Oxford and of Cambridge. I put them aside ; but these institutions, University College and King's College, taken in conjunction, are fully equal in all the requirements of a university to the other universities in Great Britain, the Scottish universities and the two universities of this country—one in the north and the other in the midland counties.

I cannot speak with any precision of detail of King's College, but I can speak with precision of this College ; and it may be interesting to you to know what this College really is, and what it can present to the public in the way of supplying the requirements of a great teaching institution of university rank.

This College, in the first place, has complete Faculties of Arts, Laws, Science, and Medicine, and a School of Fine Arts, as well as a Boys' School. This College has fifty-eight professorial chairs in operation. In addition to the fifty-eight professors, there is a large teaching staff both on the general and on the medical side,—teachers, lecturers, demonstrators, and so on,—bringing up the whole members of the teaching staff to something like one hundred. Last session this College had between nineteen hundred and two thousand students. There were five hundred and fifty boys in the school. The buildings of this College, containing, as I have said, museums, libraries, lecture-halls, laboratories, and all the appliances of a university, are spread over seven acres. They cost £300,000 in construction. This College holds on trust no less a sum than £200,000, chiefly devoted to prizes, scholarships, and other objects of that kind; and it holds, besides, in trust a sum of £135,000 for hospital purposes. Its income is between £33,000 and £34,000 a year. Taking, therefore, this College alone, so far as its buildings, the contents of its buildings, and its pecuniary resources are concerned, it stands on an equal footing with several of the universities in Great Britain; and, taken in conjunction with King's College, it stands undoubtedly superior to some.

To this College, therefore, in combination with King's College, we may fairly look to the attainment of our object of establishing a Teaching University in London which will bring the higher education of London to the doors and within the pecuniary resources of the less wealthy classes of the metropolis, so that the disgrace that has hitherto attached to the metropolis of not affording a higher education, and the discredit that university education in England is to a very great extent a privilege of the wealthier and of the well-to do classes, may be wiped away. It should be within the reach of all, even of the student of the most humble means; and it would be well if this country were to take the example of Scotland in that respect, and to follow it. . . .

In this new Teaching University there are two requirements that we insist upon. One is that the candidates for its degrees should have spent a certain specified time in attendance on lectures and instruction within its walls; and the other is that the examinations should be superintended and conducted by the teaching body of the University.

With regard to the first of these two points I wish to say a few words. . . There is something more than mere knowledge that is acquired in academic instruction. There is a culture of mind and a development of the moral and social nature that cannot be acquired by solitary study; and it is for these reasons amongst others that those who are in favour of this movement are desirous that the candidates for the degrees of the new University should prosecute a portion, at all events, of their studies within the walls of the institution, so that they may imbibe something of the spirit, and that they may be in some way, too, impregnated by the *genius loci*. This has been stigmatized as retrograde; but surely there can be nothing retrograde in that which has been found by universal experience to be the better system of education, and which is adopted in every teaching university in the country.

There is another point, and that is in regard to examinations, and it is a very essential point. We feel, and we feel very strongly indeed, that the examinations should not be directed by an outside body on which there are perhaps no examiners and no teachers, but that the examinations should be conducted by the teachers themselves in the institution in which the candidate learns. I do not say by the individual teacher of each class, but by the general body of the teachers, and that is a very different thing. And, as there would be more Colleges than one in the new Teaching University, a candidate need not in any way be examined by his own teacher, although he would be examined under the direction, superintendence, and control of the general body of the teachers. In every university now, I believe, throughout the Kingdom the teachers are assisted in their examination by assessors or by extra-professorial aid, whenever it is needed, and such, of course, would be the case in the new University. We feel that examinations ought not to lead teaching, and that if examinations are allowed to lead teaching, the teaching is fettered by the examination, and you get to a system of "cram"; the higher education and the higher teaching are apt to be neglected. I recollect many years ago a circumstance illustrating this, occurring in this College in connexion with Prof. Sharpey, one of the most distinguished men ever connected with this College, the first Professor of Physiology here and, indeed, in London. There was no course, properly speaking, of Physiology given in London until Prof. Sharpey began his lectures here in the year 1836-37. Prof. Sharpey gave an elaborate course of Physiology. From the commencement he attracted crowds of students. At that time there was connected with this College a most estimable and most amiable and most excellent old surgeon, who had grown grey within the walls, as it were, of the unreformed College of Surgeons, Mr. Samuel Cooper. He was an examiner of the College of Surgeons, and I speak of him with the greatest respect; but he was never able to raise his mind beyond the requirements of the examinations of that institution. When he heard of what Prof. Sharpey was doing, he said, "What is the good of Sharpey teaching them all this kind of stuff? We do not want it at the College of Surgeons. We have never asked for it at the College of Surgeons. Why should he teach it to them?" He had no conception beyond that, and that is the frame of mind that affects every mere examiner. He has a tendency to fetter and tie down the teaching to the level of his own examinations, and it is impossible to bring him or an examining institution above that level. We therefore wish that the instruction should lead the examination, and that the examination should follow in the wake of the teaching, and not the teaching in the wake of the examination.

SCIENCE AND REVELATION.[1]

ON the present anniversary, which is the conclusion of my first year of office as President of this Institute, I propose to address a few words to you bearing on the object of the Institute, and on the spirit in which, as I conceive, that object is best carried out.

The highest aim of physical science is, as far as may be possible, to refer observed phenomena to their proximate causes. I by no means say that this is the immediate, or even necessarily the ultimate object of every physical investigation. Sometimes our object is to investigate facts, or to co-ordinate known facts, and endeavour to discover empirical laws. These are useful as far as they go, and *may* ultimately lead to the formation of theories which in the end so stand the test of what I may call cross-examination by Nature, that we become impressed with the conviction of their truth. Sometimes our object is the determination of numerical constants, with a view, it may be, to the practical application of science to the wants of life.

To illustrate what I am saying, allow me to refer to a very familiar example. From the earliest ages men must have observed the heavenly bodies. The great bulk of those brilliant points with which at night the sky is spangled when clouds permit of their being seen, retain the same relative positions night after night and year after year. But a few among them are seen to change their places relatively to the rest and to one another. The fact of this change is embodied in the very name, planet, by which these bodies are designated. I shall say nothing here about the establishment of the Copernican system: I shall assume that as known and admitted. The careful observations of astronomers on the apparent places, from time to time, of these wandering bodies among the fixed stars supplied us, in the first instance, with a wide basis of isolated facts. After a vast amount of labour, Kepler at last succeeded in discovering the three famous laws which go by his name. Here, then, we have the second stage; the vast assemblage of isolated facts are co-ordinated, and embraced in a few simple laws. As yet, however, we cannot say that the idea of causation has entered in. But now Newton arises, and shows that the very same property of matter which causes an apple to fall to the earth, which causes our own bodies to press on the earth on which we stand, suffices to account for those laws which Kepler discovered—nay, more, those laws themselves are only very approximately true; and, when we consider the places of the planets, at times separated by a considerable interval, we are obliged to suppose that the elements of their orbits have slowly undergone slight changes. But the simple law of universal gravitation, combined, of course, with the laws of motion, not only leads to Kepler's laws as a very close approximation to the actual motions, but also accounts for those slight changes which have just been mentioned as

[1] Presidential Address delivered by Prof. Stokes, P.R.S., at the annual meeting of the Victoria Institute, on Tuesday, July 19.

necessary to make Kepler's laws fit observation exactly. We are inevitably led to regard the attraction of gravitation as the *cause* which keeps the planets in their orbits.

But it may be said, what is the difference in the two cases? Is not the law of gravitation merely a simpler mode of expressing the observed facts of the planetary motions just like the somewhat less simple laws of Kepler? What right have we to introduce the idea of causation in the one case more than in the other?

The answer to this appears to be that in the one case, that of Kepler's laws, supposing them to be true, we have merely a statement of what, on that supposition, would be a fact regarding the motions of the planets, whereas in the other case the observed motions are referred to a property of matter of the operation of which in other and perfectly different phenomena we have independent evidence.

I have purposely omitted to mention the important difference between the two cases, which lies in the circumstance that Kepler's laws require correction to make them applicable to long intervals of time, whereas the law of gravitation shows no sign of failure; because, even if the former had been perfectly exact, however long the interval of time to which they were applied, I doubt if they would have carried with them the idea of causation.

To take another simple illustration, let us think of the propulsion of a bullet in an air-gun. We speak of the motion of the bullet as being *caused* by the elasticity of the compressed air. And the idea of causation comes in because we refer this particular instance of motion to a property of gas, of the existence and operation of which we have evidence in perfectly independent phenomena.

It is thus that in scientific investigation we endeavour to ascend from observed phenomena to their proximate causes; but, when we have arrived at these, the question presents itself, can we in a similar manner regard these causes in turn as themselves the consequences of some cause stretching still further back in the chain of causation? If the motion of the bullet in an air-gun be caused by the elasticity of the compressed air, can we account for the elasticity of a gas? If the retention of the planets in their orbits be due to the attraction of gravitation, can we explain how it is that two material bodies should attract one another across the intervening space?

Till a time well on in the present century, we could only take the elasticity of gases as a fact, and deduce the consequences which flow from it. But the researches of Joule and Clausius and Maxwell and Crookes and others have accumulated so much evidence in favour of the general truth of the kinetic theory of gases, that we are now disposed not to rest in the elasticity of gases as an ultimate property beyond which we cannot go, but to regard it as itself a consequence of the molecular constitution of bodies, and of the motions and mutual collisions of the ultimate molecules of a gas. Respecting the attraction of gravitation we have not at present made a similar advance. Speculations, indeed, have not been wanting on the part of those who have endeavoured to account for it. But none of these so fits into the known phenomena of Nature as to carry with it a conviction of its truth. Yet there is one indication that though we cannot at present explain the cause of gravitation, yet it *may* be explicable by what are called second causes. The mass of a body is measured by its inertia; and, though we commonly think of a body of large mass as being heavy, and though we compare the masses of two bodies most easily and accurately through the intervention of weight, yet the idea of mass may be acquired, and means might easily be suggested by which the ratio of the masses of two bodies might be experimentally determined, without having recourse to gravitation at all. Now, according to the law of gravitation, the force with which a given body attracts another at a given distance is strictly proportional to the mass of the latter. If we suppose the attracting body to be the earth, and the attracted bodies to be in one case a brass weight, and in the other a piece of marble, it follows that if they make equilibrium when placed in the pans of a true balance—I make abstraction of the effect of the buoyancy of the air—their masses are strictly equal, and, accordingly, that weight is a true measure of mass. But there is no reason *a priori*, so far as with our present knowledge we can see, why this should be so. We know that if the bodies in the scale-pans were formed, one of brass and the other of iron, and there were a magnet concealed under the table on which the operator placed his balance, the masses would not be equal when there was equilibrium. But that the law is true, and that, accordingly, weight is a true measure of mass, follows with the highest probability from the third of Kepler's laws, and was proved experimentally by Newton, by experiments with pendulums. Newton's experiment has since been repeated by Bessel, with all the refinements of modern appliances, with the result that so far as the most exact experiments enable us to decide the law is strictly true. This is perhaps the only instance, as Sir William Thomson remarked to me in conversation, in which there is an exact agreement between two quantities, and yet we are unable to give any reason why they should agree. That such is the case, holds out some prospect of scientific men being able some day to explain gravitation itself—that is, to explain it as the result of some still higher law.

Such is the nature of our progress in scientific investigation. We collect facts; we endeavour to co-ordinate them and ascertain the laws which bind them together; we endeavour to refer these laws to their proximate causes, and to proceed step by step upwards in the chain of causation. Presently we arrive at a stage at which, even after long trial, we do not see our way to going further. Yet we are not able to demonstrate that further progress in the same direction—that is, along the chain of secondary causation—is impossible. Science conducts us to a void which she cannot fill.

It is on other grounds that we are led to believe in a Being who is the Author of nature. A conclusion so important to mankind in general is not left to be established as the result of investigations which few have the leisure and ability to carry out. Doubtless, where it is accepted, the study of science enlarges our ideas respecting the greatness of that Being, and tends to keep in check notions of too anthropomorphic a character which we might form concerning Him. Still, the subject-matter of scientific study is not, at least directly, theistic, and there have not been wanting a few instances of eminent scientists who not merely rejected Christianity, but apparently did not even believe in the being of a God.

The religious man, on the other hand, who knows little or nothing of science, is in the habit of contemplating the order of Nature not merely as the work of God, but in very great measure as his *direct* work. Of course, the concerns of everyday life present innumerable instances of the sequence of cause and effect; and few are now so ignorant of the very elements of science as not to allow that the sequence of day and night, of summer and winter, is proximately due to the rotation of the earth about its axis, and the oblique position of that axis with reference to the plane of the earth's orbit. But when we get beyond the region of what is familiarly known, still more when we get outside the limits of well-ascertained scientific conclusions, and enter a region which is still debatable ground, where men of science are attempting to push forwards, and are framing hypotheses with a view to the ultimate establishment of a theory in case those hypotheses should stand the test of thorough examination,—when, I say, we get into this region, a man such as I have supposed may feel as if the scientists who were attempting to explore it were treading on holy ground; he may mentally charge them with irreverence; perhaps he may openly speak of them in a manner which implies that he attributes to them an intention to oppose revealed religion.

To take a particular example. I can imagine that a man such as I have supposed may have always been in the habit of regarding each one of the thousands and tens of thousands of species into which naturalists have divided the animal and vegetable kingdoms as having originated in an independent creative act; that the supposition may have become entwined among his religious beliefs. Such a man would be apprehensive of any attempt to introduce second causes in explanation of the observed fact of the great multiplicity of species.

Akin to the feeling which I have attempted to describe is another, against which we must be on our guard. The religious man is strongly impressed with the truth of certain things which lie outside the discoveries of reason or the investigations of science, and which bear on the whole conduct of his life here, and on his hopes regarding a life hereafter. He believes these truths to be divine, and, accordingly, that no legitimate deduction of human reason is liable to come in conflict with them. But the precise mode in which a conviction of the truth of these things was arrived at depends, to a considerable extent, on each man's idiosyncrasy. His natural bent of mind, his early training, his later associations, have all a good deal to do with it. Divine truth is one thing; our own apprehension of it, and the steps by which in our own minds it has been arrived at, are another. These are liable to human imperfection, and we may not attribute to them the infallibility which belongs to that which is divine. We are not to confound the scaffolding with the building; nor, if we are anxious for the safety of the edifice, need we therefore fear that, if the scaffolding were tampered with, the whole might come tumbling down, nor should we regard as a dynamiter a fellow-workman who would remove a pole or two.

That truth must be self-consistent, come from where it may, is an axiom which nobody would dispute; the only question can be, What is truth? Now, there are truths which we know by intuition, such as the axioms of mathematics; and there are others, again, which, though we do not perceive them by intuition, yet demonstrably follow from what we do so perceive; such, for example, are the propositions of mathematics. Then there are other conclusions which we accept as the result of the application of our reason to a study of Nature. Here the evidence is not demonstrative, and the conclusion may have all degrees of support, from such overwhelming evidence as that on which we accept universal gravitation, to what hardly raises the conclusion above the rank of a conjecture. On the other hand, there are conclusions which we accept on totally different grounds; namely, because we think that they have been revealed. Why we accept a revelation at all, is a very wide question which I cannot here enter into. That we do accept it is implied in the membership of this Institute. But, granting the acceptance of revelation, the question remains, What and how much is involved in revelation? That is a question respecting which there are differences of opinion among those who frankly accept a revelation, and with it the supernatural.

Now, the primary object of the establishment of the Victoria Institute was to examine the questions as to which there was a *primâ facie* appearance of conflict between the conclusions of science and the teachings of revelation. In order that such examination may be usefully carried out, it must be undertaken in a thoroughly impartial spirit, with a readiness honestly to follow truth wherever it may lead. It will not do to assume that the immunity from error which belongs to the divine belongs also to our apprehension of what constitutes the divine, and that therefore, if a conflict there be, the error must be on the side of science. It is true, that many statements which are really little more than scientific conjectures are represented, at least by those who take their science at second or third hand, as if they were the well-established conclusions of science. But it is true also that the progress of science has corrected the assertions of a crude theology. We are disposed nowadays to smile at the idea of any opposition between the Copernican system and the teaching of revelation; but we need not go back to the days of the persecution of Galileo to find an example of a well-supported scientific conclusion having met with a similar opposition, issuing in a similar result.

To gauge thoroughly the amount of evidence on which an asserted scientific conclusion rests, one ought to be well acquainted with the branch of science to which it relates. Still one can get a fair general notion of the evidence by an amount of reading which is by no means prohibitive, or by conversing with those who have made that branch a special study. It may be that the impression thus left on the mind will be that the votaries of science, carried away by an excess of zeal in the attempt to discover the causes of natural phenomena, have really, though honestly, overestimated the evidence. It may be, on the other hand, that the inquirer will perceive the evidence to be weighty and substantial, in which case it behoves him to reconsider the supposition with which he started, that the conclusion was opposed to the teaching of revelation.

One should always bear in mind the great responsibility one incurs, and the mischief one may do, by representing as bound up with revelation that which really forms no part of it. Being by hypothesis no part of it, but only erroneously tacked on to it, it may be false, and being false, it may be in opposition to a conclusion supported by the weightiest evidence, it matters not of what kind, but say scientific. What, then, will be the effect of the error committed by the upholder of revelation? The educated man of science may see through the fallacy; but will it not put a weapon into the hands of the infidel lecturer wherewith to attack revealed religion?

But whether we can agree or cannot agree with the conclusions at which the scientific investigator may have arrived, let us, above all things, beware of imputing evil motives to him; of charging him with adopting his conclusions for the purpose of opposing what is revealed. Scientific investigation is eminently truthful. The investigator may be wrong, but it does not follow that he is other than truth-loving. If on some subjects which we deem of the highest importance he does not agree with us—and yet it may be he agrees with us more than we suppose—let us, remembering our own imperfections, both of understanding and of practice, bear in mind that caution of the Apostle: "Who art thou that judgest another man's servant? To his own master he standeth or falleth."

SCIENTIFIC SERIALS.

Rendiconti del Reale Istituto Lombardo, June 16.—On the importance of the qualitative bacteriological examination of potable waters, by Prof. Leopoldo Maggi. Attention is directed to the mistake made by many chemists, who occupy themselves exclusively with the *quantitative* examination of potable waters, neglecting the much more important question of the specific quality of the germs, owing to the greater difficulty of distinguishing between the various forms of these organisms. Waters largely charged with harmless Bacteria are condemned, although perfectly drinkable, while others apparently pure, but really containing deadly germs in small quantity, are declared to be quite safe, often to the great danger of the public health. It is in fact far more a question of *quality* than of *quantity*, as shown especially by the recent researches of Chantemesse and Vidal on the Bacillus of typhus. On the other hand, Leone has experimentally shown that comparatively pure water is itself a medium of culture, so that a small quantity of innocuous Bacteria may largely increase in it without rendering its use dangerous. Some instructions are added for distinguishing between harmless organisms normally present in water as their natural element, and pathological germs, which render it quite unfit for human consumption.—Meteorological observations made at the Brera Observatory, Milan, during the month of May.

SOCIETIES AND ACADEMIES.

London.

Chemical Society, June 16.—Mr. William Crookes, F.R.S., President, in the chair.—The following papers were read:—A study of the thermal properties of a mixture of ethyl alcohol and ethyl oxide, by Dr. William Ramsay and Dr. Sydney Young.—Derivatives of hydrindonaphthene and tetrahydronaphthalene, by Dr. W. H. Perkin, Jun.—The synthetical formation of closed carbon chains in the aromatic series, by Dr. F. S. Kipping.—The product of the action of ethylene bromide on ethylic acetosodacetate, by Dr. P. C. Freer and Dr. W. H. Perkin, Jun.—The synthesis of hexamethylene-derivatives, by Dr. P. C. Freer and Dr. W. H. Perkin, Jun.—An attempt to synthetize heptamethylene-derivatives, by Dr. P. C. Freer and Dr. W. H. Perkin, Jun.—The composition of shale-spirit, by Dr. A. K. Miller and Mr. T. Baker.—The magnetic rotatory power of the ethyl salts of maleic and citraconic acids and their isomers, by Dr. W. H. Perkin, F.R.S.—The temperatures at which various sulphates undergo decomposition, by Dr. G. H. Bailey.—The reaction between sulphites and nitrites of metals other than potassium, by Dr. Edward Divers, F.R.S., and Mr. Tamemasa Haga.—The action of acetyl chloride on acetoximes, by Mr. Victor Meyer and Mr. A. Warrington.—Sulphinic compounds of carbamide and thiocarbamide, by Mr. George McGowan.—Anarcardic acid, by Dr. S. Ruhemann and Mr. S. Skinner.

Edinburgh.

Royal Society, July 4.—Mr. J. Murray, Vice-President, in the chair.—Prof. Tait communicated a paper by Mr. A. C. Mitchell on the thermal conductivity of iron, copper, and German silver. Mr. Mitchell made his experiments upon the same bars as were used by Prof. Forbes and Prof. Tait, but the surfaces were nickelized so as to prevent oxidation. The results agree well with those of Prof. Tait, and are probably as correct as the method admits of.—Mr. T. B. Sprague read a paper on the probability that a marriage, entered into by a man of any age, will be fruitful.—Dr. A. B. Griffiths read a paper on the nephridia of *Hirudo medicinalis*, and communicated a paper by Mrs. Griffiths on degenerated specimens of *Tulipa sylvestris.*—Mr. J. T. Cunningham and Mr. Rupert Vallentin described the photosphæria of *Nyctiphanes norvegica*, Sars.—Mr. C. J. Burton read a paper on a Daniell cell for use as a standard of electromotive force.—Prof. Tait read a paper on glories. He showed that the observations made upon glories on Ben Nevis make it certain that Young's explanation of these phenomena (colours of thin plates) is not adequate. He considers that they are produced by diffraction of light reflected from the drops of water.—Mr. J. Murray submitted a report by Prof. Milnes Marshall and Mr. G. H. Fowler on the *Pennatulidæ* dredged by H.M.S. *Porcupine.*

PARIS.

Academy of Sciences, July 25.—M. Janssen in the chair.—Note on M. Gosselin's scientific labours, by M. A. Richet. This memoir on the life and work of the distinguished anatomist and pathologist, who died at the end of last April, is intended to supply the place of the customary obituary notice, M. Gosselin having expressed a desire that no discourse should be pronounced in connexion with his funeral obsequies.—Obituary notice of M. Alfred Terquem, Corresponding Member of the Section for Physics, by M. Mascart. A rapid sketch is given of the brilliant career of this physicist, who was born at Metz on January 31, 1831, and died on July 16, 1887. His numerous scientific publications deal mainly with acoustics, capillary phenomena, and heat. He is the author of an important treatise on "Roman Science in the Age of Augustus," and of a more comprehensive work on the history of physical sciences from the earliest times down to Galileo.—Note on the earthquake of February 23 at Nice, by M. Bouquet de la Grye. The diagram of the curve of the maregraph here figured as taken at the time of the seismic disturbance presents some points of considerable interest. It clearly indicates a rapid upheaval of the ground, followed by a slow subsidence, the sea returning to its normal level in about two hours after the first shock. The maximum of upheaval at Nice was 55 mm., which can scarcely have exceeded the natural elasticity of the earth's crust.—On the meteorite which fell at Jati-Pengilon, Java, on March 19, 1884, by M. Daubrée. The analysis of this meteorite, which weighed 166 kilogrammes, shows bronzite 39, olivine 33·4, iron with nickel and traces of cobalt 21·3, troïlite (sulphur of iron) 5·1, chromite, 0·1; mean density 3·747. The breakage presents some exceptional features, being especially remarkable for the myriads of minute cleavage facets with a sparkling brightness like that of mica. In its general appearance it may be compared to certain very fine-grained feldspar rocks, such as leptynite, and it evidently belongs to the extremely rare category represented by the meteorites of Ensisheim (1492), Erxleben (1812), Cabarras, North Carolina (1849), Morbihan (1869), and one or two others.—Fluorescences of manganese and bismuth: general remarks and conclusions, by M. Lecoq de Boisbaudran. In concluding these protracted studies the author shows in a general way that the observations made with manganese and bismuth are also applicable to other fluorescences. He also concludes that two substances more or less active on a solvent may at times neutralize each other, reducing the two fluorescences to *nil.* A similar result has been obtained by Mr. Crookes with the rare earths.—Solar observations made at Rome during the first quarter of the present year, by M. Tacchini. In supplement to his communication of April 18, the author shows that the faculæ as well as the protuberances were most frequent in the northern solar hemisphere. The maximum of faculæ corresponds to the equivalent zone $\pm 10°$; the solar spots were confined to $\pm 20°$, while the protuberances reached $\pm 80°$.—Solar observations made at Rome during the second quarter of the present year, by M. Tacchini. During this period there was a perceptible increase of all the solar phenomena, and some metallic eruptions were also recorded.—On the determination of the coefficient of elasticity of steel, by M. E. Mercadier. In a recent communication the author proved that in the relation $\frac{\lambda}{\mu}$ of the constants of elasticity λ is very nearly $= \mu$ for glass. Here he shows that for cast steel $\lambda = 2\mu$.—Danger of infection from tuberculous substances, by M. Galtier. The experiments here described fully confirm previous conclusions regarding the great resisting power of the virus of tuberculosis. It retains its activity after being subjected to temperatures ranging from 71° C. to 7° or 8° below freezing-point. It also resists the action of water and the desiccating process, as well as strong pickle, so that the consumption of fresh or corned beef from animals affected by pulmonary diseases is always attended with some danger.—On *Colochirus lacazii*, by M. Edgard Herouard. A full description is given of this new species of the genus Colochirus of the Holothurian family, found by the author in the neighbourhood of Roscoff, and by him named *C. lacazii*, in honour of M. Lacaze-Duthiers.—Contribution to the study of the evolution of the fresh-water Peridiniums, by M. J. Danysz. From his researches on the development of these organisms, as well as of the distantly-allied genera Gymnodinium and Glenodinium, the author concludes that they should be regarded rather as plants than animals. A close study of their successive phases of development, and of the nature of their substance, shows that they are true members of the vegetable kingdom.—Appearance of black rot in the neighbourhood of Agen, by M. Prillieux. An examination of some diseased grapes from this district shows clearly that they have been attacked by black rot which had already made its appearance in the Upper Hérault Valley two years ago, but which it was hoped would die out or spread no farther.—A sealed paper deposited by M. A. Leduc on May 9, 1887, and now opened at his request, describes two experiments showing that the calorific conductibility of bismuth is considerably reduced when this metal is placed in a magnetic field.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Hand-book to Government Situations: B. D. K. (Stanford).—The Conic Sections: G. Heppel (Baillière, Tindall, and Cox).—Formal Logic, Second Edition: J. N. Keynes (Macmillan).—Psychology; The Motive Powers: J. McCosh (Macmillan).—Romantic Love and Personal Beauty: H. T. Finck (Macmillan).—Crown Forests at the Cape of Good Hope: J. C. Brown (Oliver and Boyd).

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