THURSDAY, NOVEMBER 14, 1889.

SCIENCE AND THE FUTURE INDIAN CIVIL SERVICE EXAMINATIONS.

THE following memorial, signed by a numerous and highly-distinguished body of resident graduates of the University of Cambridge, has been presented to the Civil Service Commissioners :—

"We, the undersigned resident graduates of the University of Cambridge interested in the study of natural science, understanding that a reorganization of the open competitive examination for the Civil Service of India is under the consideration of the Civil Service Commissioners, beg respectfully to urge on the Commissioners the desirability of widening the range of the examination so as to include the several branches of natural science. We think it especially important that the maximum number of marks obtainable by a candidate in natural science in the examination should be the same as that obtainable by a candidate in classics or in mathematics. In support of this opinion we venture to point out that the Natural Sciences Tripos, both from its numbers and from the rewards assigned by the Colleges to those of their members who distinguish themselves therein, is now of equal importance with the Classical or Mathematical Tripos.

"We have the honour to append a statement of the numbers who have during the last five years taken honours in natural science, classics, and mathematics. We inclose a copy of the *Cambridge University Reporter* of June 12, 1888, containing a report to the Senate and a schedule of the numbers examined in each branch of natural science in the years 1883–87.

"We would desire to call attention to the acknowledged educational value of the study of natural science, and to point out that the training which it affords, combining as it does both theory and practice, is such as peculiarly to fit a student for the pursuits of practical life.

"We beg to state that a deputation would be happy to wait on the Commissioners to explain more fully our views on the subject should it be their pleasure to receive them."

This memorial is signed, among others, by two Heads of Houses, thirteen Professors, and twenty Fellows. The memorialists, as will be seen, urge that in future competitions the position of a candidate offering natural science shall be not less favourable than that of those who offer classics or mathematics. And in a highly instructive schedule they show how important a place the study of the natural sciences has now attained in the University of Cambridge.

It may be unknown to many of our readers that the subject to which this memorial relates has lately become one of great importance, in consequence of a proposed reorganization of the higher branches of the public services in India. A Commission, which we believe sat in India, known as the Public Service Commission, has advised that the following changes should be made with the object of admitting natives of India to higher and more extensive employment in the public services :—

(1) That the strength of the Covenanted Civil Service should be reduced to what is necessary to fill the chief administrative appointments of the Government, and such a proportion of smaller appointments as will secure a complete course of training for junior Civilians. This branch of the service to continue to be recruited by means of open competitions in England, at which natives of India should be allowed to compete unreservedly, and for which the maximum age of the Native candidates, and therefore presumably of the English candidates, should be raised to twenty-three years.

(2) That a certain number of appointments should be transferred from the Covenanted Civil Service to a local Civil Service, which is to be recruited, locally, from Natives and resident Europeans who satisfy certain prescribed preliminary conditions.

We do not know how far these proposals have been adopted by the home authorities, though we understand that they have received the general approval of the Indian Government. We will therefore only say, in passing, that they appear to be open to two serious objections.

First, that it seems a dangerous thing to select so limited a number of young men for the higher branch of the service by open competition, since doing so will give to each one of those who succeed almost the certainty of the reversion of one of the prizes of the public services. Under such a condition there will be far too little inducement for zeal in the service, and too little opportunity for selection and rejection when age and experience have developed the administrative powers of the selected men.

Secondly, unless care be taken to regulate the previous training of the candidates, as, for example, by requiring that every candidate shall have taken a University degree in England or India before presenting himself at the competitive examination, it is likely that well-taught rather than well-educated men will be selected, and that an inferior order of men will offer themselves, since many of the ablest men would be unable to submit to some years of private tuition, and to give up, as they would probably have to do, a University education for the chance of obtaining an appointment in India.

Whatever decision may have been made, however, it is of the utmost importance that the representatives of Cambridge who have addressed themselves to the Civil Service Commissioners should be supported in every possible way, and at once, by all those who have the interest of science and education at heart. For there is reason to fear that the Commissioners have contemplated the complete withdrawal of science from these examinations ; and unfortunately many of the various regulations for the Army examinations which have been brought forward with their sanction in recent years give an air of probability to this suggestion. This is in no way weakened when we consider the extremely unfortunate position that science candidates for the Indian Civil Service have occupied under the administration of the Commissioners for many years past. This position, it should be said, has been due, not so much to the marks allotted to science in the present scheme, as to the methods adopted by the Commissioners in conducting their examinations, which have long caused it to be recognized by those who are engaged in the instruction of Civil Service candidates that, as a rule, only those candidates who are excellent either in classics or mathematics, or those who are distinctly good in both, have a really good chance of success.

But though all these facts give reason for regarding the rumour we refer to as very possibly correct, they need by no means prevent those who are interested in the question from entertaining strong hopes of averting such a national disaster as that which we fear. We have only to remind them of the very considerable degree of success that followed the efforts recently made by Sir Henry Roscoe and other leaders in science in the case of the examinations for admission to the Royal Military Academy at Woolwich. These efforts, we may remind our readers, not only resulted in an advantageous revision of the Woolwich examinations, but brought about satisfactory changes in the case of the Sandhurst competitions. In connection with this result it is satisfactory to observe, in the Report of the Civil Service Commission for 1888, that the Commission, in a letter directed to the Director-General of Military Education on July 10 in that year, have described the changes that had been submitted to them as likely to influence beneficially the education of officers in the army before they begin their professional studies.

Whatever difficulties there may be in the way of obtaining just treatment for science candidates under the new scheme for the selection of Indian civil servants, it has, we fear, become again imperative that men of science should unite to protest against the assumption that natural science studies are in themselves inferior as a mental training to the classical languages and mathematics, and to insist, so far as they may, upon such studies being placed upon a proper footing in this particular examination. This should be done in the interests of education, and still more of our Indian fellow-subjects, whose administrators should be men of as wide and liberal an education as possible, as has, indeed, been recognized in more than one public investigation of the regulations for these appointments.

THE LUND MUSEUM IN THE UNIVERSITY OF COPENHAGEN.

E Museo Lundii: En Samling af Afhandlinger om de i det indre Brasiliens Kalkstenshuler af Professor P. V. Lund udgravede Dyre-og Menneskeknogler. Udgivet af Dr. Lütken. (Kjöbenhavn: H. Hagerup, 1888.)

THIS work, as its title indicates, consists of various monographs, descriptive of the collections made by Dr. Lund in his interesting exploration of the limestone caverns in the interior of Brazil. These important finds are the fruits of nearly ten years' unremitting labour in the neighbourhood of Lagoa Santa, on the Rio das Velhas, in the province of Minas Geraes, where Dr. Lund prosecuted his researches from 1835 to 1844. On the completion of his cave explorations he presented the whole of his incomparable collections to the Danish nation. The gift has been duly appreciated, and now constitutes, under the name of the "Lund Museum," one of the most important palæontological sections of the Zoological Museum in the University of Copenhagen.

Dr. Lund inspected as many as 800 of the Brazilian *lapas*, or bone-caves, of which he had discovered 1000. Of these only sixty yielded any very interesting results, while scarcely half that number contained a sufficient quantity of bones to demand any very prolonged investigation. In some instances, on the other hand, the mass of broken bones was so enormous that from the earth collected in a packing-case whose dimensions did not exceed half a cubic foot, he extracted 400 half jaw-bones of a marsupial and 2000 belonging to different rodents, besides the remains of innumerable bats and small birds. This discovery led to further research, and, after fifteen weeks' continued exploration, he found that one cave, which he had at first estimated to be about 25 feet deep, had a depth of nearly 70 feet, and was so densely packed with bones that the yield of 6500 barrels, of the size of an ordinary butter-firkin, justified the assumption that this special *lapa* contained the remains of seven and a half millions of animals, belonging for the most part to Cavia, Hystrix, and small rodents and marsupials, the estimate being based on the numbers of half jaw-bones extracted from the mould.

In these enormous cave deposits we have, according to Dr. Lund, and his biographer Dr. Reinhardt, a prehistoric ornithological *kökken mödding*, birds of prey having resorted to the *lapas* of Brazil as suitable retreats in which to devour their innumerable victims, whose fractured bones, belonging in almost equal proportions to extinct and living animals, have revealed to us many long-hidden secrets in the history of the changes which the Brazilian fauna has experienced in the course of ages. Comparatively few remains of the larger living mammals have been found, three caves only having yielded evidence of the presence of bears, of which, moreover, the bones of only five individuals were recovered. But while various groups, as *e.g.* the Ungulata, were sparsely represented, several families among the Edentata have contributed so largely to the bone remains of the Brazilian *lapas* that this order would appear to have constituted the most important section of the local fauna, both in past and recent times. Among the cave armadillos, Lund recognized several forms, differing only by their larger size from *Dasypus punctatus*, and *D. sulcatus;* but besides these he found one of colossal dimensions, which, with a body of the size of an ox, and a tail 5 feet in length, exhibited differences of dentition which induced him to assign it to a special genus, to which he gave the name Chlamydotherium. A peculiar characteristic of this fossil animal, whose food he believes was leaves, and not insects, was the fusion or overlapping of several of the vertebræ into nodes, or tangles. In this respect it resembles the still more remarkable armadillo, of whose scales and bones he found enormous quantities, and which he described under the name of Hoplophorus. This animal, of which the different species varied from the size of a hog to that of a rhinoceros, was described about the same time by Prof. Owen, to whom various specimens of its bones had been sent from La Plata, and who established a new species for its reception, to which he gave the name of Glyptodon. The extraordinary rigidity of the shields of some of the Brazilian armadillos, the apparent immobility of the head, and the interlocking of the vertebral bones, make it difficult to understand how these unwieldy animals could have obtained their food. The most probable solution of the problem seems to be supplied by a study of the short massive hind legs, which, with their sharp and powerful claws, may have served to grub up roots and tubers, and tear off the branches of trailing plants. There is no evidence that our living tardigrades had appeared among the cave fauna of Brazil, where their place was supplied by gigantic gravigrades, resembling the Megatherium.

The results yielded by a careful study of the enormous and varied materials obtained by Dr. Lund in his explorations would appear, generally, to indicate that in post-Pliocene ages the Mammalian fauna of Brazil was richer than in recent times, entire families and sub-orders having become extinct in the intervening ages, or at all events greatly reduced as to the numbers of their genera and species. This is more especially the case in regard to the Edentata, Ungulata, Pachydermata, and Carnivora, which still continue to be characteristic representatives of the South American fauna. In two cases only there is evidence that species which are now exclusively limited to the Old World once inhabited the American continent. A far more marked difference between extinct and living animals is to be observed in the western than in the eastern hemisphere. Thus while the existing Brazilian fauna comprises very few large animals, the predominant forms being almost dwarf-like when compared with their Eastern analogues, the post-Pliocene Brazilian Mastodons, Macrauchenians, Toxodons, and gigantic armadillos and tardigrades, may rank in size with the elephant, rhinoceros, and hippopotamus, which were their contemporaries in Europe at that period of the world's history.

There is no ground for assuming that the change in the South American fauna was due to any natural cataclysm, and it would rather seem to be the result of some regular and slow geological changes, which, by affecting the then existing climatic relations, may have disturbed the conditions of animal life, and thus brought about the destruction, or deterioration, of the larger mammals, which, according to Owen, succumb where the smaller ones adapt themselves to altered conditions.

It was not till near the close of his explorations that Dr. Lund succeeded in finding human bones in such association with fossil remains as to justify the conclusion that man had been the contemporary in Brazil of animals long since extinct in South America. Only seven of the 800 *lapas* examined by him contained any human bones, and in several instances these were either not associated directly with fossil bones, or there were grounds for suspecting that they might have been carried into the caves in comparatively recent ages with the streams that traverse them. In one of these, however, the Sumidouro Lapa, remains of as many as thirty individuals of all ages were found so intermingled with the bones of the gigantic cave jaguar, *Felis protopanther*, and the monster Cavia, *Hydrochærus sulcidens*, together with several extinct ungulates, that whatever may have been the reason of their presence, there seems to be no ground for doubting that primæval man was contemporaneous with these animals.

The crania, of which admirably drawn illustrations are given, are of a dolichocephalic type, characterized by strongly-marked prognathism, and remarkable for the excessive thickness of the cranial walls. The first communication by Lund of his discovery of human remains in the Lapa di Lagoa do Sumidouro was made (in 1840) in a letter addressed to Prof. Rafn, in which his fear of being accused of recklessness in attaching too high an antiquity to man in Brazil is shown by the pains he takes to indicate every possible means by which these bones might have found their way into the cave. Thus it remained for his annotator, the late Dr. Reinhardt, whose descriptive history of the caves and their exploration has added largely to the interest of the volume before us, to be the first to accept without reservation the co-existence of man with extinct animals which, according to Lund himself, occupied parts of South America more than 5000 years ago.

The monograph treating of the human remains found by Lund is from the pen of Dr. Lütken, the editor of the present work, who also supplies a *résumé* in French of the treatises contributed by his colleagues, Drs. O. Winge and H. Winge, the former of whom writes on the birds of the Brazilian *lapas*, and the latter on the living and extinct rodents of the Minas Geraes district. Besides these important contributions to the work, the reader is indebted to the late Dr. Reinhardt for a detailed description of the situation and geological character of the Brazilian bone-caves, and for an interesting biographical notice of Dr. Lund.

We learn from the preface that this collection of monographs owes, if not its publication, at any rate the complete and elegant form in which it has been produced, to the liberality of the directors of the Carlsberg Trust, at whose cost, with the sanction of the Danish Royal Society, it now forms one of those *éditions de luxe* which have of late years so largely enriched the scientific literature of Denmark. The objection that may be advanced against this, as well as others of the series, is that the writers appear to be moved by an uncalled-for impulse to write down to the level of the general reader, and to explain the origin and progress of each special branch of natural history they are concerned with. Such efforts to popularize the subject lead only to an inconvenient addition to the bulk of the volumes, and are wholly at variance with the scientific aim and object of such publications.

HYDRAULIC MOTORS.

Hydraulic Motors : Turbines and Pressure Engines. By G. R. Bodmer, A.M.I.C.E. "The Specialist's Series." (London : Whittaker and Co., 1889.)

THE essential detail which lifts the mere water-wheel to the rank of a turbine consists, according to the author, in some arrangement for directing the water over the buckets in the most advantageous manner, instead of allowing the water merely to follow its own course. Again, in a water-wheel only a small part of the wheel is really at work at a time, the buckets of the remaining part being empty ; while a turbine is arranged, as a rule, with a vertical axis, and all parts of the wheel are simultaneously taking their fair share of the work. In this respect there is a great resemblance and analogy to the distinction between the two chief instruments of ship propulsion by steam—the paddle-wheel and the screw propeller. In the paddle-wheel only a few of the floats act on the water at a time ; while in the screw propeller, completely submerged, all parts are equally at work, implying a great saving of weight in the propelling instrument. Mr.

Thornycroft, with his turbine propeller, is able to emphasize this economy of weight still further, and, but for difficulties of going astern not yet surmounted, would be able to save considerable weight and space in sea-going steamers with this contrivance.

As regards their construction, turbines are divided into three classes (p. 24)—the radial, axial, and mixed-flow—according to the mode in which the water enters and passes through the turbine; but as regards the dynamical principle on which the turbines work, they are divided into two classes (p. 25), the *reaction* and the *impulse* turbine.

In the reaction or Jonval turbine, described in chapters iii. to vi., the passages are completely filled with water, and the changes of pressure play an important part in the work performed. This turbine possesses the advantage of being able to work when drowned by the tail race, or when elevated above the tail water to a height anything less than the height of the water barometer, a suction tube of properly adjusted shape being fitted below the turbine to carry off the water at pressure gradually increasing downwards to the atmospheric pressure. Against this are the disadvantages of imperfect regulation for varying load, and that with a high fall this turbine must be made so small and must run so fast as rapidly to wear out, as in the Fourneyron turbines at St. Blaise (p. 422); but this disadvantage the author professes (p. 263) to avoid by compounding the turbine, just as we compound the steam-engine with high-pressure steam.

The impulse or Girard turbine, on the other hand (chapters vii. and viii.), derives its power entirely from the change of momentum of the water without change of pressure; the buckets are freely ventilated, and consequently this turbine can only work in communication with the surrounding air. It possesses, too, the great advantage of complete regulation of power by merely altering the supply of water. Girard turbines are divided into outward flow (Fourneyron) turbines, and inward flow (James Thomson); the latter, although more weighty and costly, possessing the advantage of greater stability of motion.

In their difference of action we may compare the Jonval turbine with the screw propeller, which works entirely immersed, and derives its reaction partly from the change of pressure in the water; while the Girard turbine resembles the paddle-wheel in working at the surface of separation of the water and air, so that no appreciable change of pressure is manifest. Against this analogy, however, we find the screw propeller far less susceptible to changes of immersion than the paddle-wheel, whence the manifest superiority of the screw for long voyages.

In chapters ix. to xi. the author gives a very valuable collection of numerical applications of his theories to actual turbines on a large scale. In designing a turbine to utilize a fall, the first important measurement is that of the quantity of the stream of water; the speed of the turbine is next determined from the consideration that the best theoretical speed is half (or a little more than half) the speed at which the turbine would run if unloaded; and then various practical considerations intervene in deciding whether the turbine should be reaction or impulse, outward, inward, or mixed flow.

At Holyoke, Mass., the Water-Power Company, under Mr. James B. Francis, controlling the falls of the Connecticut, undertake the commercial testing of turbines submitted to them, and have checked to some extent the wild claims of efficiency, reaching and even exceeding 100 per cent., which American turbine makers are said to have claimed in their advertisements. There is still, however, an efficiency claimed for American turbines which has not been rivalled in Europe: this cannot be attributed to defect in our designs, and the author thinks must be attributed to the less care bestowed in America on the measurement of the quantity of water consumed. It is noticeable that the American turbines are generally of the reaction Jonval type, which is more suitable for their unlimited supplies of water by reason of its smaller weight and cost; here in Europe, where water is scarcer, the impulse Girard turbine is more in favour.

For mining purposes, especially in California, with great falls of 400 or 500 feet and small quantities of water, the hurdy-gurdy or Pelton wheel (p. 419) is a favourite, and in a paper by Mr. Hamilton Smith, Jun., of the American Society of Civil Engineers, the efficiency of this wheel and its practical advantages are declared to be very high. Similar small impulse turbines seem likely to come into general domestic use.

The author concludes (chapter xiii.) with a description of the various hydraulic pressure engines and motors of Armstrong, Rigg, and others. These engines act by pressure only, like the steam-engine, with the disadvantage of using the same quantity of water whether working at high or low power, except in the case of Mr. Rigg's motor. Such motors are, however, coming into great use on ships, not only for working the guns, but for steering, loading, and discharging cargo.

Although designed, and amply fulfilling its purpose, as a practical treatise on hydraulic motors, this book will provide the pure theorist with some of the most elegant applications of relative velocity, aberration, dynamical principles, and of hydromechanics; and it is instructive to notice that, as in all practical mechanical treatises, gravitation units of force only are employed, even in the hydrodynamical equations of Borda and Carnot, or of Bernoulli, as we think they should be called. All this is in direct opposition to the theoretical text-books; theorist or practical man, which is to give way?

A. G. G.

PHYSIOLOGY OF EDUCATION.

Physiological Notes on Primary Education and the Study of Language. By Mary Putnam Jacobi, M.D. (New York and London: G. P. Putnam's Sons, 1889.)

THIS is a remarkable book. The authoress is an original thinker who knows how to express her thoughts clearly and strongly. It is worthy of being read by all interested in the science of education, though few perhaps even of the advocates of the present educational renaissance would be prepared to receive every one of her conclusions.

The work consists of four distinct essays. The first two are entitled "An Experiment in Primary Education," and describe the way in which Dr. Mary Jacobi taught her own little girl. She commences the account with some very valuable remarks on the right order of studies.

"The first intellectual faculties to be trained are perception and memory. The subjects of the child's first studies should therefore be selected, not on account of their ultimate utility, but on account of their influence upon the development of these faculties. What sense is there then in beginning education with instruction in the arts of reading and writing? . . . From the modern standpoint, that education means such an unfolding of the faculties as shall put the mind into the widest and most effective relation with the entire world of things—spiritual and material,—there is an exquisite absurdity in the time-honoured method. To study words before things tends to impress the mind with a fatal belief in their superior importance."

As forms and colours are the elements of all visual impressions, Dr. Jacobi began to teach her child geometrical forms before she was four years of age. At four and a half the little girl began elementary colours. Afterwards she made acquaintance with the points of the compass, the main ideas of perspective, and then maps and geography. The study of number, of course by concrete illustrations, followed that of form and outline. The observation of natural objects, especially that of plants and plant-life, was then commenced. The growth of beans and hyacinths was carefully watched, and the daily observations made by the child were written down by the mother, till she attempted them herself, and became gradually initiated into the mysteries of writing. This led her on easily to the art of reading when she was about six years of age. The progress of the child's mental development during these early years is fully described, with many pleasant recollections of her sayings.

The third part consists merely of a criticism of Miss Youman's views on the teaching of botany, and an argument in favour of commencing in a child's education with the flower rather than the leaf.

Half the book, however, is occupied by the fourth essay, in which the authoress treats of "The Place for the Study of Language in a Curriculum of Education." Of course she places it after the mind has been trained to deal with sense perceptions of external objects; but she contends earnestly for the importance of the study of words, especially for the power it possesses of enabling the child to form abstract conceptions. The authoress enters largely into the brain action involved in the use of verbal signs or complex ideas, and illustrates her views of the matter by means of physiological diagrams. She also describes a little device for the comparison of verbal roots, which she terms "language tetrahedrons," and which are intended to show the relation between Latin, French, German, and English. She would devote to literary studies, including English, the best part of the time between the Kindergarten training and the age of fourteen.

"To the study of words may be brought the scientific methods used in the study of things—observation, analysis, comparison, classification; and the child may thus begin to be trained for physical science at a time when the pursuit of most physical sciences is impossible."

It may be that Dr. Mary Jacobi claims too much time for the study of language, but the old-fashioned educationalists will get little consolation from her concessions; for she not only places the study of words after that of things, but she would have several forms of Aryan speech studied simultaneously, and she would postpone the study of grammar till two years after the serious study of language has commenced. She believes that the power of abstraction and the general mental training gained by these philological studies will enable the young person at an early age to enter upon more serious matters of study or those of more immediate practical utility.

J. H. G.

OUR BOOK SHELF.

Steam-Engine Design. By Jay M. Whitham, Professor of Engineering, Arkansas Industrial University. (London: Macmillan and Co., 1889.)

In this work the author treats of the application of the principles of mechanics to the design of the parts of a steam-engine of any type or for any duty. He acknowledges that he has culled as much information as he has required from well-known sources, both English and American; and he has embodied, as a sort of foundation for his work, a course of lectures given to his class at the United States Naval Academy by P.A. Engineer John C. Kafer, U.S.N.

After careful study, we can say that the book appears to be well suited for its purpose. The arrangement of information, both principles and details, is much the same as that in Mr. A. E. Seaton's excellent work on marine engineering; but the field covered is of far less extent, and the boiler and its accessories are not included. The author being a Professor of Engineering in an American University, we expected to find some variations from our own practice in steam-engine design. In this, however, we were disappointed. A few of the woodcuts represent parts of engines differing in insignificent details from those used in this country, but the main design is practically the same. It is gratifying to find many of our own engineers quoted as authorities in the volume—viz. D. K. Clark, A. E. Seaton, R. Sennett, and many other well-known English authorities.

It must not be supposed that there is no original work in this book. Chapters ii. and iii. for instance, on the design of slide valves and reversing gears, are ample evidence of hard work on the part of the author: his descriptions and diagrams of the various motions are excellent. Chapter iv. deals with the general design and proportions of the steam-chest, valves with their various connections. Chapters v. and vi. are on compound and triple-expansion engines, and contain also a theoretical treatment of indicator diagrams of a compound engine. These chapters are well written, and contain much useful information, but as a whole they do not teach anything new. To chapters vii. and viii., written by P.A. Engineer Asa M. Mattice, U.S.N., the same remarks will apply. The remaining chapters deal with the design of the various other parts of a steam-engine. The methods used are those well understood in every drawing-office worthy of the name, and they need not be further noticed here.

Taken as a whole, the book deserves praise for good and careful work; and we may especially call attention to the theoretical considerations, which are always clearly expressed. Although published by Messrs. Macmillan, the work is from an American press, that of Messrs. Ferris Bros., New York. The printing and woodcuts are excellent—far better, as usual, than English work of the same class.

N. J. L.

Coloured Analytical Tables. By H. W. Hake, Ph.D., F.I.C., F.C.S. (London: George Phillip and Son, 1889.)

Novelties in text-books of elementary qualitative analysis are usually conspicuous by their absence, but the book before us takes an entirely new departure. The idea of representing the various coloured reactions by tinted imitations is, so far as we know, quite new. Apart from this, the usual well-worn paths are followed. The tables are of the simplest character, and are only sufficient for the detection of common bases in salts or oxides, no attempt being made to separate the members of the various groups. The second part is devoted to reactions for the detection of a few acids and organic substances.

The book is apparently primarily intended for the use of students preparing for the preliminary examination of the Conjoint Board of the Royal College of Physicians and Surgeons, but it will no doubt have a much wider field of usefulness if it survives the test of experience. The new method of representation seems excellently adapted for young students, and certainly no harm can be done by giving it a fair trial.

The reactions illustrated include precipitates, charcoal reactions, borax beads, and flame colorations, most of which are fairly well represented.

The Story of a Tinder Box. By Charles M. Tidy, M.B. M.S., F.C.S., &c. (London: Society for Promoting Christian Knowledge, 1889.)

POPULAR lecturers have discovered for some time that the history of the methods that have been used for obtaining a light is an excellent subject wherewith to please the public mind, and this book contains the reports of three such lectures delivered to a juvenile auditory last Christmas. An attempt has also been made to describe the experimental portion of the lectures, and the author has not committed the common error of giving a multiplicity of pretty but irrelevant experiments conveying a paucity of information. In fact, in some parts the reverse seems the case, for we must confess our inability to discover why a consideration of the allotropic modifications of carbon should necessitate a detailed description of the manufacture of black lead pencils. This digression, however, does not detract from the interest and general merit of the work, which certainly contains the explanation in simple language of some elementary physical and chemical phenomena.

Magnetism and Electricity. Part I. Magnetism. By Andrew Jamieson, M.I.C.E. (London: Griffin and Co., 1889.)

ALTHOUGH elementary text-books of physics continue to increase in number, there is still room for one of such general excellence as Prof. Jamieson's elementary manual. The book is specially arranged for the use of first year Science and Art Department and other electrical students. Numerous questions and specimen answers are distributed throughout the book, and though this may be rather suggestive of cram, there is nothing in the text to justify such a suggestion. It is unnecessary to go into details, but it may be stated that the arrangement of subjects is as good as it well can be, and on the whole the descriptions are very clear. The numerous diagrams are also excellent, those of the mariner's compass being especially good; indeed, the whole chapter on terrestrial magnetism is the best elementary account of the subject which has come under our notice.

The subject is throughout considered as an essentially practical one, and very clear instructions are given for the making of magnets, and compass and dipping needles.

If the succeeding parts of the book confirm the good opinion created by the first, teachers of the subject are to be congratulated on having such a thoroughly trustworthy text-book at their disposal.

Time and Tide: A Romance of the Moon. By Sir Robert S. Ball, LL.D., F.R.S. (London: Society for Promoting Christian Knowledge, 1889.)

THE ability of the author of this work to give a lucid exposition of an abstruse subject is a matter of common knowledge; and hence the fact that the book contains two of his lectures delivered at the London Institution last November is in itself sufficient commendation. However, be this as it may, we have no hesitation in saying there could hardly be a clearer explanation of Prof. George Darwin's theory of tidal evolution than that contained in the work before us. The hypothesis being accepted, every feature of the past and future condition of our satellite is described in a most comprehensive manner. It is first shown how, when the earth was rotating on its axis with an enormous velocity, the tidal action set up by the sun caused a portion to become detached and form our satellite. The employment of the term "conservation of spin" facilitates considerably the demonstration of the fact that as by tidal action the spin of the earth decreases—as our day lengthens—so must the dimensions of the moon's orbit be increased, and the length of the month therefore become proportionally greater. The application of Prof. Darwin's theory to other members of our system is also inquired into; and although the author does not attempt to go back to the first stage in the evolution of celestial species, he shows that tidal evoluti on is an extension of the hypothesis that does so. Indeed, the book is replete with information, and by the general scientific reader will be found exceedingly interesting.

LETTERS TO THE EDITOR.

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Specific Inductive Capacity.

PERHAPS a better mode of performing the experiment quoted by Mr. Rudge (p. 10) is to have two insulated parallel metal plates, one connected with an electroscope, the other with a slightly-charged Leyden-jar. On now interposing a thick slab of paraffin or ebonite (recently passed through a flame) between the plates, a very decided increase of divergence will be perceived. Unless, indeed, the electroscope should happen to have overflowed to earth during the charging of the jar, in which case it will be oppositely charged and a decreased divergence will be caused. To interpose the slab is, in fact, virtually to diminish the distance between the plates, and its effect is therefore the same as that of pushing the plates closer together.

The advantage of the Leyden-jar is that it keeps the potential practically constant. If an isolated plate or sphere is used as the charged body, the circumstances are not so simple, for the insertion of the slab reduces the potential and slightly increases the charge on the near face of the plate, so that, whether the divergence of the leaves is increased or diminished depends on several unimportant considerations, of which the size of the slab may be one. A slab of area comparable to that of the plates between which it is put would in this case be the most suitable; and in any case it should be supported by a long insulator, so that the operator's arm, as it approaches, shall not complicate and mask the effect.

OLIVER J. LODGE.

University College, Liverpool, November 9.

"La Pietra Papale."

ABOVE Stresa, on the western bank of Lago Maggiore, there is an enormous granite boulder, which deserves the attention of geologists. It lies on the left slope of an old moraine, near the little village of Gignese, and not far from the Hotel Alpino, at an elevation of about 2500 feet above the sea-level. It is roughly oblong in shape, and measures some 75 feet in length, and perhaps half as much in breadth and thickness. The projected mountain railway from Stresa to the summit of Monte Motterone will pass close to the spot where it lies, and the masons are already engaged in converting the smaller boulders into building-stones. It is to be hoped, however, that *la pietra papale*, as this splendid example of the carrying powers of ice is called by the villagers, will not suffer the like fate. The Italian Alpine Club, will, we may trust, interest themselves in this matter. P. L. SCLATER.

Hotel du Parc, Lugano, October 21.

Who discovered the Teeth in Ornithorhynchus?

AS Dr. Hart Merriam's letter on the above subject in your issue of the 7th inst. (p. 11) will be read by many who have not access to Sir Everard Home's "Lectures on Comparative Anatomy," allow me to point out that the description and figures in that work referred to by Dr. Merriam have no bearing whatever upon the very interesting discoveries recently made. They represent, not the real teeth of the young animal discovered by Mr. Poulton, and fully described by Mr. Oldfield Thomas, but the well-known horny plates which functionally take their place in the adult, and which are called "grinding teeth" by Sir Everard only in a very general sense. W. H. FLOWER.

British Museum (Natural History), November 9.

THE account of the teeth of Ornithorhynchus, given by Sir Everard Home in "Lectures on Comparative Anatomy," vol. i. p. 305, explanatory of Tab. lix. vol. ii., referred to by Mr. Hart Merriam in your last issue (p. 11), shows, even more clearly than the figures, that the *true* teeth had not been noticed at that time (1814). The passage is as follows:—"In the posterior portion of the mouth, both in the upper and lower jaw, are placed grinding teeth with broad flattened crowns, four in number, one on each side of each jaw. *They are composed of a horny substance* (the italics are my own), only embedded in the gum, to which they are connected by an irregular surface in the place of fangs. When cut through, the substance appears fibrous, like that of nail; the direction of the fibres being perpendicular to the crown, similar to that of the horny crust of the gizzard. The teeth in the young animal are smaller, and two on each side, so that the first teeth are probably shed, and the two small ones replaced by one large one."

It is perfectly evident that here no reference is made to the *true* teeth, and, moreover, the figure of the two smaller "teeth" of young specimens represents merely the immature horny plates. The honours, therefore, still remain with Mr. Poulton and Mr. Oldfield Thomas. OSWALD H. LATTER.

Anatomical Department, The Museum, Oxford, November 8.

On a Mite of the Genus *Tetranychus* found infesting Lime-trees in the Leicester Museum Grounds.

ABOUT the 13th of last September my attention was called to the strange appearance of a row of lime-trees standing in front of the School of Art buildings in Hastings Street. On examination I found that the whole row, with, I think, only one exception, were almost entirely devoid of leaves, the trunks and branches being covered with a fine web, very closely spun, giving them the appearance of being coated with a thin layer of ice, this glazed look being specially noticeable when standing in such a position as to catch the reflected rays of the sun. At first sight I imagined that I was examining the work of a spider, though I was unable to recollect any whose webs would accord with the character of those under observation. However, a close inspection revealed the webs to be tenanted by an innumerable number of yellowish or orange-coloured mites which were in some places associated together in dense masses or clusters, and more or less abundant over the whole of the trunks and branches.

These mites appeared, on being subjected to a careful microscopical examination, to be identical with *Tetranychus tiliarum*, Mull., a species which it seems that Claparède considers to be only a variety of *T. telarius*, the common "red spider." However that may be, they are at any rate closely allied forms—members of the family *Trombidiidæ*, which possess, as one of their distinguishing characteristics, a pedipalpus with a claw and a lobe-like appendage. In the genus *Tetranychus* the palpi are chelate, the mouth is furnished with a barbed sucking apparatus for the extraction of plant juices, and spinning organs are usually present. It is needless to comment upon their destructiveness to vegetation, for most keepers of gardens and hothouses are familiar with their ravages in one direction or another, and the difficulty experienced in thoroughly extirpating them.

In connection with the species which forms the subject of the present communication, I notice that Murray, in his work on the "Aptera," says: "It occasionally occurs in such numbers as almost to denude the trees of their foliage; and it has been noted that the stems and branches of such trees seemed covered with a bright glaze. Can this be a fine web?" It was so, most certainly, in the present instance, which afforded me a most favourable opportunity for examination. Again, it appears that the mites are normally found on the under-surface of the leaves, which they cover with a fine web of silk, on which (to again quote Murray) "they are sometimes crowded together in vast numbers; for example, we have seen them so thick on the leaves that they looked as if they were not merely sprinkled with a yellow orange-coloured powder, but as if it was actually in parts heaped up on them, so that none of the green colour of the leaf was visible." Their presence is of course highly injurious, causing the leaves to shrivel and drop; and it seems to me that the fact of their occurrence on the bare bark of the trunks was attributable to the death of the leaves causing them to retreat to that position, uncongenial though it would seem to be. Such trees as preserved their foliage presented no abnormal appearance on the branches, &c., notwithstanding which, in one or two instances, I believe the parasites were present on the leaves, though seemingly not in such extraordinary profusion.

Dugès, writing of *T. telarius*, states his belief that that species passes the winter under stones, and instances the finding of several active individuals so situated in a garden near Paris in the month of October. Regarding this point I may say that my specimens of *T. tiliarum*, which I placed in a box immediately after removal from the trees, speedily ensconced themselves in the most convenient nooks and crannies, in which they spun fine webs. It may be worth noting that the days on which my observations were made were warm and damp, with scarcely any wind, quite typical early autumn days in fact. F. R. ROWLEY.

Leicester Museum.

Retarded Germination.

I SHALL be much obliged to any of your readers who can give an explanation of the probable cause of the above phenomenon, which I have remarked this year. I sowed a number of patches of seeds of various hardy annuals in the garden in the last week of April; about half of them came up after the usual interval, strongly and regularly. Such were *Calendula Pongei*, *Convolvulus minor*, *Lavatera trimestris*, *Collinsia bicolor*, *Iberis* white and red, *Specularia speculum*, *Linum rubrum*, &c., &c. Then there were some of which a few scattered seedlings made their appearance at this time, and after an interval of about six weeks the greater part of them also came up; among these were *Eutoca viscida*, *Nigella damascena*, *Sphenogyne*, and *Clarkia pulchella*. Thirdly, there were some of which I quite despaired; mignonette, however, appeared thinly about the end of June, and at intervals till August; and in the middle of June a few plants (in proportion to the seed sown, a few) of *Linaria bipartita*, *Madia elegans*, and *Xeranthemum* came up—one consequence being that the last named has not yet flowered. Some of the seeds were obtained this spring from seedsmen, some were my own collection of the last year or two—of the latter were *Calendula*, *Lavatera*, *Convolvulus*, *Specularia*, *Eutoca*, *Nigella*, *Sphenogyne*, and mignonette—so that cannot be said to give any clue. The conditions for germination and growth were favourable, and the season also. I have never remarked before any *annuals* so long in appearing above ground; though in some herbaceous plants I have noticed it, *e.g.* *Gaillardia*, *Myosotis alpestris*, and *Anemone coronaria*. E. A.

Herefordshire, September 19.

The Relation of the Soil to Tropical Diseases.

AS a humble subscriber to and student of NATURE, will you bear with me while I ask your help, as shortly and plainly as I can? I am in a very secluded corner of one of the Native States of Rajpootana, and I am collecting facts and making observations on the relation of the *soil* to tropical diseases; my ambition being to discuss it not so much from a statistical and geographical standpoint, as from the geological, in its chemical and biological aspects; though, as I conceive, the geographical, climatological, and geological elements in the problem are not to be arbitrarily distinguished. Now I am far away from all books of reference, and it is of course essential that I make myself acquainted with what has already been done in these subjects, and I venture to ask for any hints as to the bibliography of them. Can you tell me if anyone has done for geology what Hirsch, of Berlin, has done for geography (in his work on the distribution of disease)? Is there any authority on the chemistry of soils, and what I roughly call their physiology and pathology, their structural and functional changes under influences—climate notably—and their own intrinsic, and the deeper geological interactions?

A. ERNEST ROBERTS.

Meywar Bheel Corps, Kherwara, Central India, September 9.

The Earthquake of Tokio, April 18, 1889.

DR. VON REBEUR-PASCHWITZ'S letter, which appeared in NATURE, vol. xl. p. 294, is of special interest to us in Japan, countenancing as it does the conjecture that the very peculiar earthquake felt and registered here on April 18 was the result of a disturbance of unusual magnitude. It was my good fortune on the day in question to be engaged in conversation with Prof. Sekiya in the Seismological Laboratory at the very instant the earthquake occurred. We at once rushed to the room where the self-recording instruments lay, and there, for the first time in our experience, had the delight of viewing the pointers mark their sinuous curves on the revolving plates and cylinders. At first sight it seemed as if the pointers had gone mad, tracing out sinuosities of amplitudes five or six times greater than the greatest that had ever before been recorded in Tokio. There was not much *sensation* of an earthquake; indeed, after the first slight tremor that attracted our attention, we felt nothing at all, although in the irregular oscillations of the seismograph pointers we had evidence enough that an earthquake was passing. Very few in Tokio were aware that there had been an earthquake till they read the report of it in the next day's papers. Thus the motion, though large, was too slow to cause any of the usual sensations that accompany earthquakes, and suggested a distant origin and a large disturbance, with a consequent wide extension of seismic effect. Excepting the slight tremors recorded at Potsdam and Wilhelmshaven, there has been, so far, no evidence of any such far-reaching action.

My object in writing this note, however, is to correct an error of calculation which Dr. von Rebeur-Paschwitz has unwittingly made. He has assumed that Tokio standard time is mean local time. On the contrary, the standard time for all Japan is the mean solar time for longitude 135° E.,—that is, nine hours in advance of Greenwich mean time. Hence, instead of the Tokio earthquake having preceded the German disturbance by 1h. 4·3m. it preceded it by only 45m. This correction increases the velocity of transmission to 3060 metres per second. We must assume, then, either that large disturbances in the heart of the earth travel with exceptionally high speeds, or that the origin of the disturbance was a considerable distance from Tokio. The latter assumption seems sufficiently satisfactory, if in other respects Dr. von Rebeur-Paschwitz's views meet with approval.

CARGILL G. KNOTT.

Imperial University, Tokio, Japan, September 25.

A Brilliant Meteor.

YESTERDAY evening, November 4, at 7.55 p.m., I was fortunate enough to observe a very brilliant meteor. It became visible almost exactly at the zenith, or a little west of it, and moved, as nearly as I could judge, due east, magnetic; it remained visible for about from one to two seconds, disappearing, finally, rather low down on the eastern horizon. For the first half of its journey it was of a dazzling white brightness, and then it suddenly became a dull red spark. The light emitted from it when brightest reminded me of the light from an arc lamp, and was very much brighter than any of the fixed stars.

As it was so short a time in view, and there were no stars visible, I could only approximately estimate its point of appearance and path. There were a few clouds about, mostly in the west, and the moon was behind them. PAUL A. COBBOLD.

Warwick School, November 5.

ON THE HARDENING AND TEMPERING OF STEEL.[1]

II.

THE following considerations appear to have guided Osmond in beginning his investigations (see *ante*, p. 16). Bearing in mind the fact that molecular change in a body is always accompanied by evolution or absorption of heat, which is, indeed, the surest indication of the occurrence of molecular change, he studied with the aid of a chronograph what takes place during the slow cooling and the slow heating of masses of iron or steel, using, as a thermometer to measure the temperature of the mass, a thermo-electric couple of platinum and of platinum containing 10 per cent. of rhodium, converting the indications of the galvanometer into temperatures by Tait's formulæ.

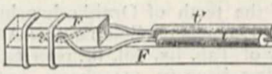


FIG. 5.

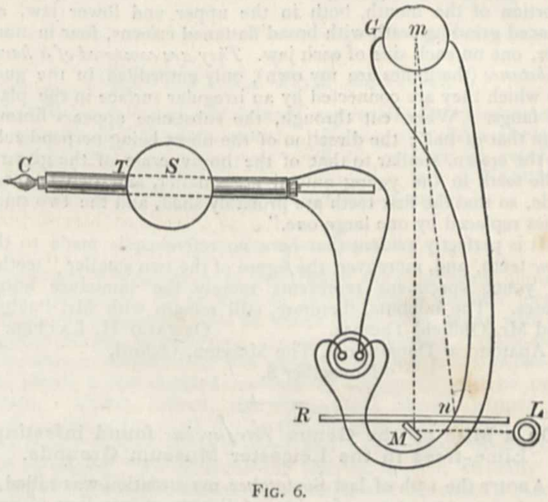


FIG. 6.

FIGS. 5 and 6 show the actual mode of conducting the experiments. F (Fig. 5) is a piece of steel into which a platinum and platinum-rhodium couple, *t*, *t'*, is fixed. It is inclosed in a glazed porcelain tube and heated to bright redness in the furnace, S (Fig. 6). This tube, T, may be filled with any gaseous atmosphere. C is a bulb filled with chloride of calcium. The metal under examination is slowly cooled down. The wires from the thermo-couple pass to the galvanometer, G. The rate of cooling of the mass is indicated by the movement of a spot of light from the galvanometer mirror at *m*, on the screen, R, and is recorded by a chronograph. The source of light is shown at L; M is a reflector.

In the next diagram (Fig. 7) temperatures through which a slowly-cooling mass of iron or steel passes, are arranged along the horizontal line, and the intervals of time during which the mass falls through a definite number (6·6) of degrees of temperature are shown vertically by ordinates. See what happens while a mass of electro-deposited iron (shown by a dotted line), which is as pure as any iron can be, slowly cools down. From 2000° to 870° it falls uniformly at the rate of about 2·2° a second, and the intervals of temperature are plotted as dots at the middle of the successive points of the intervals. When the temperature falls down to 858°, there is a sudden arrest in the fall of temperature, the indicating spot of light, instead of falling at a uniform rate of about 2° a second, suddenly takes 26

[1] A Lecture delivered on September 13, by Prof. W. C. Roberts-Austen, F.R.S., before the members of the British Association. Continued from p. 16.

seconds to fall through an interval of temperature which hitherto and subsequently only occupies about 6 seconds. Turn to the diagram, and see what actually happens when the iron contains carbon in the proportion required to constitute it mild steel (shown by thin continuous line, Fig. 7); there is not one, but there are two such breaks in the cooling, and both breaks occur at a different temperature from that at which the break in pure iron occurred. As the proportion of carbon increases in steel, the first break in cooling travels more and more to the right, and gradually becomes confounded with the second break, which, in steel containing much carbon, is of long duration, lasting as much as 76 seconds in the case of steel containing 1·25 per cent. of carbon (thick continuous line, (Fig. 7).

[In the experiments shown to the audience the spot of

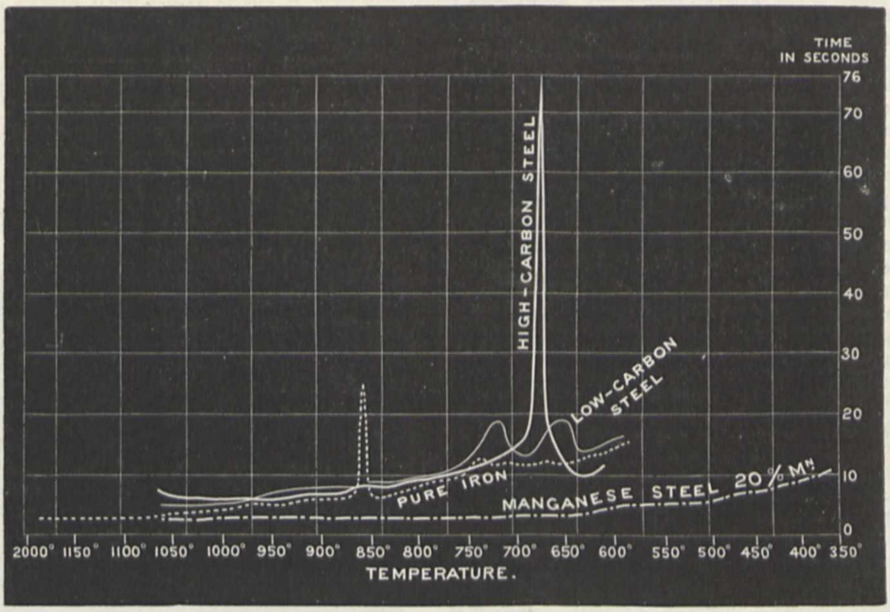


Fig. 7.—The curves in this diagram show how the rate of movement of the spot of light varies with different samples of steel. The stoppage of the movement of the spot of light of course indicates the evolution of heat from the cooling mass of steel, F (Fig. 5).

light moved slowly and uniformly along a screen ten feet in length. It halted for a few seconds as the temperature of the cooling mass of steel fell to about 850° C., and when the metal was at dull redness, the spot of light remained stationary for 68 seconds, and then resumed its course.]

Now, it may be urged, evidently the presence of carbon has an influence on the cooling of steel when left to itself: may it not affect molecular behaviour during the rapid cooling which is essential to the operation of hardening? We know that the carbon, during rapid cooling, passes from the state in which it is combined with the iron into a state in which it is dissolved in the iron; we also know that, during slow cooling, this dissolved carbon can re-enter into combination with the iron so as to assume the form in which it occurs in soft steel. Osmond claims that this second arrestation in the fall of the thermometer corresponds to the recalescence of Barrett, and is caused by the re-heating of the wire by the heat evolved when carbon leaves its state of solution and truly combines with the iron.

If it is hoped to *harden* steel, it must be rapidly cooled before the temperature has fallen to a definite point, not lower than 650°, or the presence of carbon will be unavailing. But what does the first break in the curves mean? You will see that a break occurs in electrotype iron which is free from carbon (thin dotted line, Fig. 7); it must then indicate some molecular change in iron itself, accompanied with evolution of heat—a change with which carbon has nothing whatever to do, for no carbon is present; and Osmond argues thus:—There are two kinds of *iron*, the atoms of which are respectively arranged in the molecules so as to constitute *hard* and *soft iron*, quite apart from the presence or absence of carbon. In red-hot iron the mass may be soft but the molecules are hard—let us call this β iron; cool such red-hot pure iron, whether quickly or slowly, and it becomes soft; it passes to the α soft modification—there is nothing to prevent its doing so. It appears, however, that if carbon is present, and the metal be rapidly cooled, the following result is obtained: a certain proportion of the molecules are retained in the form in which they existed at a high temperature—the hard form, the β modification—and hard *steel* is the result.

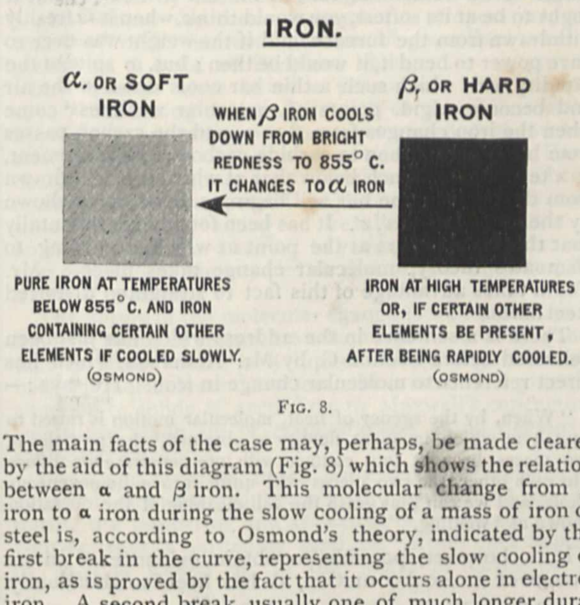


Fig. 8.

The main facts of the case may, perhaps, be made clearer by the aid of this diagram (Fig. 8) which shows the relation between α and β iron. This molecular change from β iron to α iron during the slow cooling of a mass of iron or steel is, according to Osmond's theory, indicated by the first break in the curve, representing the slow cooling of iron, as is proved by the fact that it occurs alone in electro-iron. A second break, usually one of much longer duration, marks the point at which carbon itself changes from the dissolved or hardening carbon to the combined carbide-carbon. It follows that, if steel be quickly cooled *after the change from β to α* has taken place but before the carbon has altered its state—that is, before the change indicated by the second break in the curve has been reached—then the iron should be soft, but the carbon, hardening carbon; and as such, the action of a solvent should show that it cannot be released from iron in the black carbide form. This proves to be the case, and affords strong incidental proof of the correctness of the view that two modifications of iron can exist.

It will be seen, therefore, that, although the presence of carbon is essential to the hardening of steel, the change in the mode of existence of the carbon is less important than has hitherto been supposed.

The α modification of iron may be converted into the β form by stress applied to the metal at temperatures below a dull red heat, provided the stress produces permanent deformation of the iron,[1] but the consideration of this question would demand a lecture to itself. I am anxious to show you an experiment which will help to illustrate the existence of molecular change in iron.

Here is a long bar of steel containing much carbon. In such a variety of steel, the molecular change of the iron itself, and the change in the relations between the carbon and the iron, would occur at nearly the same moment. It is now being heated to redness, but if you will look at this diagram (Fig. 9), you will be prepared for what I want

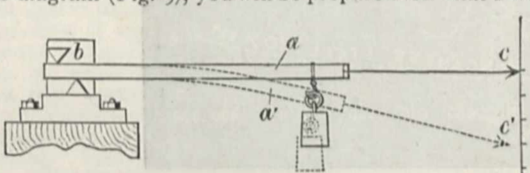


FIG. 9.—The bar of steel, *a*, ½ inch in section and 18 inches long, heated to bright-redness and firmly fixed in a vice or other support at *b*. A weight of about 2 pounds is rapidly hung on to the free end, and a light pointer, *c*, is added to magnify the motion of the bar. It remains perfectly rigid for a period varying from 30 to 40 seconds, and then, when the bar has cooled down to very dull redness, it suddenly bends, the pointer falling from 6 to 8 inches to the position *c'*.

you to see in the actual experiment. One end of the red-hot bar *a* will be firmly fixed at *b*, a weight *not sufficient to bend it* is slung to the free end, which is lengthened by the addition of a reed, *c*, to magnify any motion that may take place. Now remember that as the bar will be red-hot it ought to be at its softest, you would think, when it is freshly withdrawn from the furnace and if the weight was ever to have power to bend it, it would be then; but, in spite of the rapidity with which such a thin bar cools down in the air and becomes rigid, points of molecular weakness come when the iron changes from β to α, and the carbon passes from hardening carbon to carbide-carbon; at that moment, at a temperature much below that at which it is withdrawn from the furnace, the bar will begin to bend, as is shown by the dotted lines *a'*, *c'*. It has been found experimentally that this bend occurs at the point at which, according to Osmond's theory, molecular change takes place. Mr. Coffin takes advantage of this fact to straighten distorted steel axles.[2]

There is a sentence in the address which has just been delivered before Section G, by Mr. Anderson, which has direct reference to molecular change in iron. He says:—

"When, by the agency of heat, molecular motion is raised to a pitch at which incipient fluidity is obtained, the particles of two pieces brought into contact will interpenetrate or diffuse into each other, the two pieces will unite into a homogeneous whole, and we can thus grasp the full meaning of the operation known as 'welding.'"

It is, however, possible to obtain evidence of interchange of molecular motion, as has been so abundantly shown by Spring, even at the ordinary temperature, while, in the case of steel, it must take place far below incipient fluidity—indeed, at a comparatively low temperature, as is shown by the following experiment on the welding of steel. Every smith knows how difficult it is to weld highly carburized hard tool-steel, but if the ends of a newly-fractured 3/16-inch square steel rod, *a* (Fig. 10), are placed

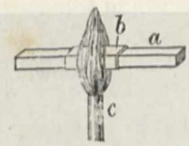


FIG. 10.

together and covered with platinum foil, *b*, so as to exclude the air, and if the junction is heated in the flame of a Bunsen burner, *c*, the metal will weld, without pressure, so firmly that it is difficult to break it with the fingers, although the steel has not attained a red-heat.[1]

The question now arises, What is the effect of the presence of other metals in steel, of which much has been heard recently? (1) Manganese. Osmond has shown that this metal enables steel to harden very energetically, as is well known. If much of it be present, 12 to 20 per cent., in iron, *no break whatever* is observed in the curve which represents slow cooling (see line marked "manganese steel" (Fig. 7). That is, the iron never shows such a change as that which occurs in other cooling masses of iron. Then you will say such a material should be hard however it is cooled. So it is. There is one other important point of evidence as to molecular change connected with the addition of manganese to submit to you. Red-hot iron is not magnetic. Hopkinson[2] has shown that the temperature of recalescence is that at which iron ceases to be magnetic. It may be urged that β iron cannot therefore be magnetized. Steel containing much manganese cannot be magnetized, and it is therefore fair to assume that the iron present is in the β form. Hadfield[3] has given metallurgists wonderful alloys of iron and manganese in proportions varying from 7 to 20 per cent. of manganese. This core of iron round which a current is passing, attracts the sphere of iron, but if nothing is changed, except by replacing the core of iron with a core of Hadfield's steel, it is impossible to make a magnet of it. [Experiment shown.]

Prof. Ewing, who has specially worked on this subject, concludes that, "no magnetizing force to which the metal is likely to be subjected in any of its practical applications would produce more than the most infinitesimal degree of magnetization" in this material.

It has been seen that quantities of manganese above 7 per cent. appear to prevent the passage of β iron into the α form. In smaller quantities manganese seems merely to retard the conversion, and to bring the two loops of the diagram nearer together.

Time will not permit me to deal with the effect of other elements on steel. I will only add that tungsten possesses the same property as manganese, but in a more marked degree. Chromium has exactly the reverse effect, as it enables the change of hard β iron to α soft iron to take place at a higher temperature than would otherwise be the case, and this may explain the extreme hardness of chromium steels when hardened in the same way as ordinary steels.

There are a few considerations relative to the actual working of steel with which I can deal but briefly, notwithstanding their industrial importance. The points *a* and *b*, adopted in the celebrated memoir of Chernoff to which

[1] "Études Métallurgiques," par Osmond, p. 6 (Paris: Dunod, 1888.)
[2] Trans. American Soc. Civil Engineers, xvi., 1887, p. 324.

[1] Trans. American Society Mechanical Engineers, ix., 1888, p. 155.
[2] Proc. Roy. Soc., xlv., 1889, pp. 318, 445, and 457.
[3] Proc. Inst. Civil Engineers, xciii. Part iii., 1888.

I have referred already, change in position with the degree of carburization of the metal. It is useless to attempt to harden steel by rapid cooling if it has fallen in temperature below the point (in the red) *a*, and this is the point of "recalescence" at which the carbon combines with the iron to form carbide-carbon: it is called V by Brinell. In highly carburized steel, it corresponds exactly with the point at which Osmond considers that iron, in cooling slowly, passes from the β to the α modification. Now with regard to the point *b* of Chernoff. If steel be heated to a temperature above *a*, but below *b*, it remains fine-grained however slowly it is cooled. If the steel be heated above *b*, and cooled, it assumes a crystalline granular structure whatever the rate of cooling may be. The size of the crystals, however, increases with the temperature to which the steel has been raised.

Now the crystalline structure, which is unfavourable to the steel from the point of view of its industrial use, may be broken up by the mechanical work of forging the hot

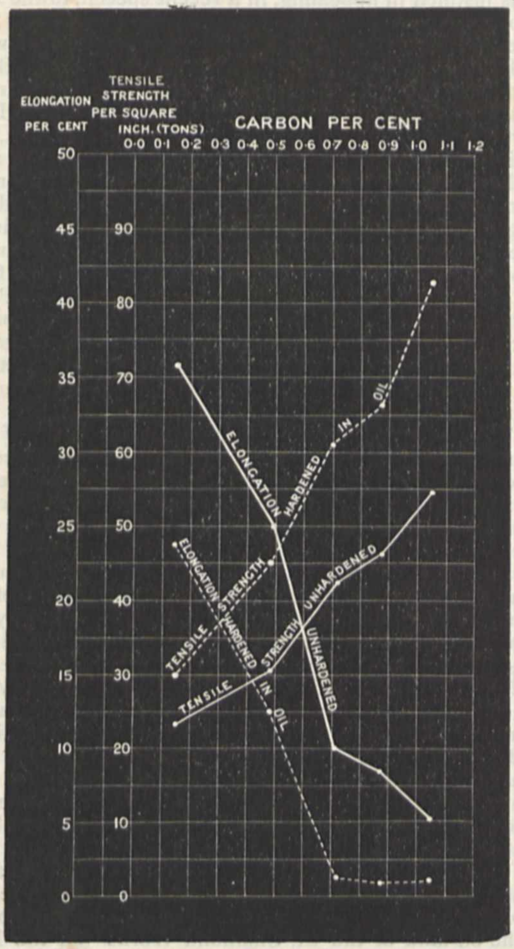


FIG. 11 shows the way in which the tenacity of steel containing varying amounts of carbon is increased by oil hardening,[1] while at the same time the elongation rapidly diminishes.

mass; and the investigations of Abel, of Maitland, and of Noble, have shown how important "work" on the metal is. When small masses of hot steel are quenched in oil, they are hardened just as they would be if water were used as a cooling fluid. With large masses, the effect of quenching in oil is different. Such cooling of large hot masses appears to break up this crystalline structure in a manner analogous to mechanical working. If the mass of metal is very large, such as a propeller shaft, or tube of a large gun, the change in the relations between the carbon and the iron, or true "hardening" produced by such oil treatment is only effected *superficially*—that is, the hardened layer does not penetrate to any considerable depth, but the innermost parts are cooled more quickly than they otherwise would have been, and the development of the crystals, which would have assumed serious proportions during slow cooling, is arrested. It depends on the size of the quenched mass, whether the tenacity of the metal is or is not increased, but its power of being elongated is considerably augmented. This prevention of crystallization I believe to be the great merit of oil quenching, which, as regards large masses of metal, is certainly not a true hardening process.

There has been much divergence of view as to the relative advantages of work on the metal, and of oil-hardening, but I believe it will be possible to reconcile these views, if the facts I have so briefly stated be considered.

The effect of annealing remains to be dealt with. In a very complicated steel casting, the cast metal probably contains much of its carbon as hardening carbon, and the mass which has necessarily been poured into the mould at a high temperature is crystalline. The effect of annealing is to permit the carbon to pass from the "hardening" to the "carbide" form, and, incidentally, to break up the crystalline stucture, and to enable it to become minutely crystalline. The result is that the annealed casting is far stronger and more extensible than the original casting. The carbide-carbon is probably interspersed in the iron in fine crystalline plates, and not in a finely divided state. It would obviously be impossible to "work"—that is, to hammer—complicated castings, and the extreme importance of obtaining a fine crystalline structure by annealing, with the strength which results from such a structure, has been abundantly demonstrated by Mr. J. W. Spencer, whose name is so well known to you all in Newcastle.

The effect of annealing and tempering is in fact very complicated, and I can only again express my wish that it were possible to do justice to the long series of researches which Barus and Strouhal have conducted in recent years. They consider that, annealing is demonstrably accompanied by chemical change, even at temperatures slightly above the mean atmospheric temperature, and that the "molecular configuration of glass-hard steel is always in a state of incipient change, . . . a part of which change must be of a permanent kind." Barus says "that during the small interval of time within which appreciable annealing occurs, a glass-hard steel rod suddenly heated to 300° is almost a viscous fluid."[1] Barus considers that glass-hard steel is constantly being spontaneously "tempered" at the ordinary temperature, which, he says, "acting on freshly quenched [that is hardened] steel for a period of years, produces a diminution of hardness about equal to that of 100° C., acting for a period of hours."

The nature of the molecular change is well indicated in the long series of researches which led them to conclude that in steel "there is a limited interchange of atoms between molecules under stress, which must be a property common to solids, if, according to Maxwell's conception, solids are made up of configurations in all degrees of molecular stability."

Barus and Strouhal attach but little importance to the change in the relations between the carbon and the iron during the tempering and annealing of hard steel. They consider that in hardening steel the "strain once applied to steel is locked up in the metal in virtue of its

[1] This was well shown in Prof. Akerman's celebrated paper on "Hardening Iron and Steel," Journ. Iron and Steel Institute, 1879. Part ii. p. 501.

[1] *Phil. Mag.*, xxvi., 1883, p. 209.

viscosity"; tempering is the release of this molecular strain by heat.

Highly carburized steels harden very energetically by very slight modifications in thermal treatment, and it will be evident that a very hard material is unsuitable for industrial use if the conditions of its employment are such as to render it desirable that the material should stretch. To turn to very "mild" steel which does not harden, it is certain that, although wrought iron passes almost insensibly into steel, there can be no question that not merely the structural but the molecular aggregation of even steel containing only $\frac{2}{10}$ per cent. of carbon is profoundly different from that of wrought iron. Formerly, as Sir F. Bramwell pointed out in a lecture delivered at the Royal Institution in 1877, "by the year 1830 . . . from small beginnings in Staffordshire and at Birkenhead sprang a wonderful *wrought-iron* navy, but steel was a luxury: it was made in small portions sold at high prices, as much as a shilling or eighteenpence a pound. It was employed for swords, cutlery, and tools, needles and other purposes where the quantity used was but trifling, and where the importance of the superior material was such as to justify the large expenditure incurred. It was felt in those days that steel was worth paying for because it was trusted; indeed its trustworthiness had passed into a proverb"—"as true as steel."

The class of steel which was formerly employed, as I have just indicated, for weapons and tools belonged to the highly carburized, readily-hardening class. It was the "mild steel" containing but little carbon which was destined to replace wrought iron, and when attempts were made to effect the general substitution of steel for iron, fears as to its character and trustworthiness unfortunately soon arose, so that from about the year 1860 until 1877 steel was viewed with suspicion. We can now explain this. Doubts as to the fidelity of steel, even when it was obtained free from entangled cinder, arose from ignorance of the fact that, on either side of a comparatively narrow thermal boundary, the iron in steel can practically exist in two distinct modifications. The steel was true enough, but from the point of view of the special duties to be intrusted to it, its fidelity depended on which modification of iron had to be called to the front. Artificers attempted to forge steel after it had cooled down below the point *a* of Chernoff, at which recalescence occurs, and they often attempted to work highly carburized steel at temperatures which were not sufficiently low.

Steels may be classified from the point of view of their industrial use according to the amount of carbon they contain, and I have attempted to arrange in this trophy certain typical articles, grouped under certain definite percentages of carbon ranging from $\frac{2}{10}$ to $1\frac{1}{2}$ per cent. [This was a trophy 18 feet square, with various typical articles of steel arranged in order according to the amount of carbon they contained. I am greatly indebted to Mr. J. W. Spencer, of Newcastle, who kindly lent me the fine series of specimens of which the "trophy" is built up.] Each class merges into the other, but the members at either end of the series vary very greatly. It would be impossible to make a razor which would cut from boiler plate; and conversely, a boiler made of razor steel would possibly fracture at once if it were superheated and subjected to any sudden pressure of steam. Speaking generally, if the steel contains, in addition to carbon, $\frac{5}{10}$ per cent. of manganese, each class of steel, as at present arranged, would have to be shifted a class backwards towards the left of the trophy.

At the present day, instead of steel being manufactured and used in small quantities, about 4,000,000 tons are annually employed in this country. Let us see how it is used. A steel fleet, the finest fleet in the world, has recently assembled at Spithead. The material of which it was made contained $\frac{15}{100}$ to $\frac{2}{10}$ per cent. of carbon, and when steel faces are used for the armour plates, the material contains $\frac{7}{10}$ to $\frac{8}{10}$ per cent. of carbon.

It has been pointed out that the crews of the fleet at Spithead numbered no less than 21,107 men. This it has been shown is "a remarkable figure, considering the great economy in men which prevails in a modern navy as compared with the navy of Nelson's day. A hundred years ago the normal requirements of a fleet were one man to a little over four tons, but now, thanks to the part played by steel and hydraulic power, we require but one man to every seventeen tons. Thus it may roughly be said that an aggregate of 20,000 men at the present day corresponds to an aggregate of 80,000 men in the days of Nelson." The latest type of battle-ship weighs, fully equipped, about 10,000 tons, there being about 3400 tons of steel in the hull, apart from her armour, which, with its backing, will weigh a further 2800 tons.[1]

From the use of steel in the Royal Navy and in the mercantile marine, let us pass on to its most notable use in construction. If the President of the French Republic was justified in appealing, in a recent speech, to the Eiffel Tower as "a monument of audacity and science,"[2] what are we to say of the Forth Bridge, the wonders of which will be described by Mr. Baker on Saturday? By his kindness I am able to place in the position in the trophy justified by the carbon it contains, a plate from the Forth Bridge, which fell from a height of some 350 feet, and, being of excellent quality, doubled itself on the rocks below. A single span of the Forth Bridge is nearly as long as two Eiffel Towers turned horizontally and tied together in the middle, and the whole forms a complicated steel structure weighing 15,000 tons, erected without the possibility of any intermediate support, the lace-like fabric of the bridge soaring as high as the top of St. Paul's. The steel of which the compression members of the structure are composed contains $\frac{23}{100}$ per cent. of carbon and $\frac{69}{100}$ per cent. of manganese. The parts subjected to extension do not contain more than $\frac{19}{100}$ per cent. of carbon.[3]

Time will not permit me to pass the members of each class in review. I can only refer to very few. Steel for the manufacture of pens contains about $\frac{3}{10}$ per cent. of carbon, and 16 to 18 tons of steel are every week let loose on an unoffending world in the shape of steel pens.

Steel rails contain from $\frac{3}{10}$ to $\frac{4}{10}$ per cent. of carbon, and, in this class, slight variations in the amount of carbon are of vital importance. An eminent authority, Mr. Sandberg, tells us that in certain climates a variation of $\frac{1}{10}$ per cent. in the amount of carbon may be very serious. The great benefit which has accrued to the country from the substitution of more durable steel rails for the old wrought-iron ones may be gathered from the figures which Mr. Webb, of Crewe, has given me, which show that "the quantity of steel removed from the rails throughout the London and North-Western system by wear and oxidation is about 15 cwt. an hour, or 18 tons a day."

Gun-steel contains $\frac{3}{10}$ to $\frac{5}{10}$ per cent. of carbon, and it may contain $\frac{8}{10}$ per cent. of manganese. It is in relation to gun-steel that oil-hardening becomes very important. The oil-tank of the St. Chamond Works (on the Loire) is 72 feet deep, and contains 44,000 gallons of oil, which is kept in circulation by rotary pumps, to prevent the oil being unduly heated locally when the heated mass of steel is plunged into it.

Now with regard to projectiles. To quote some recent remarks of Lord Armstrong,[4] "the heaviest shot used in the *Victory* was 68 pounds, while in the *Victoria* it will be 1800 pounds; and, while the broadside-fire from the

[1] Address by Mr. Baker, Section G, British Association Report, 1885, p. 1182.
[2] *Times*, August 19, 1889.
[3] Journal of the Iron and Steel Institute, 1888, ii. p. 94.
[4] *Times*, August 3, 1889.

Victory consumed only 325 pounds of powder, that from the *Victoria* will consume 3000 pounds. The most formidable projectiles belong to the highly carburized class of steel. Shells contain 0·8 to 0·94 per cent. of carbon, and, in addition, some of these have 0·94 to 2 per cent. of chromium. The firm of Holtzer shows, in the Paris Exhibition, a shell which pierced a steel plate 10 inches thick, and was found, nearly 800 yards from the plate, entire and without flaw, its point alone being slightly distorted. Compound armour-plate with steel face, which face contains 0·8 per cent. of carbon, is, however, more difficult to pierce than a simple plate of steel.

[A prominent feature in the "trophy," among the class of highly carburized steels which contain over $\frac{7}{10}$ per cent. of carbon, was a fine suspended wire $\frac{8}{100}$ of an inch diameter, of remarkable strength, supporting a weight of $2\frac{1}{2}$ cwt., or a load of nearly 160 tons to the square inch. The strength of the same steel *undrawn*, would not exceed 50 tons to the square inch. A similar wire manufactured by the steel company of Firminy attracted much attention in the Paris Exhibition by supporting a shell weighing 1800 lbs., or a load of 158 tons per square inch.]

Lastly, I will refer to the highly carburized steel used for the manufacture of dies. Such a steel should contain 0·8 to 1 per cent. of carbon, and no manganese. It is usual to water-harden and temper them to a straw colour, and a really good die will strike 40,000 coins of average dimensions without being fractured or deformed; but I am safe in saying that if the steel contained $\frac{1}{10}$ per cent. too much carbon, it would not strike 100 pieces without cracking, and if it contained $\frac{2}{10}$ per cent. too little carbon, it would probably be hopelessly distorted, and its engraved surface destroyed, in the attempt to strike a single coin.

The above examples will be sufficient to show how diverse are the properties which carbon confers upon iron, but as Faraday said, in 1822, "It is not improbable that there may be other bodies besides charcoal capable of giving to iron the properties of steel." The strange thing is that we do not know with any certainty whether, in the absence of carbon, other elements do play the part of that metalloid, in enabling iron to be hardened by rapid cooling. Take the case of chromium, for instance: chromium-carbon steels can, as is well known, be energetically hardened, but Busek[1] has recently asserted that the addition of chromium to iron in the absence of carbon does not enable the iron to be hardened by rapid cooling. So far as I can see, it is only by employing the electrical method of Pepys that a decision can be arrived at as to the hardening properties of elements other than carbon.

A few words must be devoted to the consideration of the colours which, as I said (see *ante*, p. 11), direct the artist in tempering or reducing the hardness of steel to any determinate standard. The technical treatises usually give—not always accurately, as Reiser[2] has shown—a scale of temperature ranging from 220° to 330°, at which various tints appear, passing from very pale yellow to brown yellow, purples, and blues, to blue tinged with green, and finally to grey. Barus and Strouhal[3] point out that it is possible that the colour of the oxide film may afford an indication of the temper of steel of far greater critical sensitiveness than has hitherto been supposed. It is, however, at present uncertain how far time, temperature, and colour are correlated, but the question is being investigated by Mr. Turner, formerly one of my own students at the School of Mines.

That the colours produced are really due to oxidation was shown by Sir Humphry Davy in 1813,[4] but the nature of the film has been the subject of much controversy. Barus points out that "the oxygen molecule does not penetrate deeper than a few thousand times its own dimensions,[1] and that it probably passes through the film by a process allied to liquid diffusion. The permeable depth increases rapidly with the temperature, until at an incipient red heat the film is sufficiently thick to be brittle and liable to rupture, whereupon the present phenomenon ceases, or is repeated in irregular succession.

Looking back over all the facts we have dealt with, it will be evident that two sets of considerations are of special importance: (1) those which belong to the relations of carbon and iron, and (2) those which contemplate molecular change in the iron itself. The first of these has been deliberately subordinated to the second, although it would have been possible to have written much in support of the view that carburized iron is an alloy of carbon and iron, and to have traced with Guthrie the analogies which alloys, in cooling, present to cooling masses of igneous rocks, such as granite, which, as the temperature of the mass falls, throws off "atomically definite"[2] bodies, leaving behind a fluid mass of indefinite composition, from which the quartz and feldspar solidify before the mica. This view has been developed with much ability in relation to carburized iron by Prof. Howe, of Boston, who even suggests mineralogical names, such as "cementite," "perlite," and "ferrite," for the various associations of carbon and iron.

I am far from wishing to ignore the interest presented by such analogies, but I believe that the possibility of molecular change in the iron itself, which results in its passage into a distinctive form of iron, is at present the more important subject for consideration, not merely in relation to iron, but as regards the wider question of allotropy in metals generally.

Many facts noted in spectroscopic work will have, as Lockyer has shown, indicated the high probability that the molecular structure of a metal like iron is gradually simplified as higher temperatures are employed. These various simplifications may be regarded as allotropic modifications.

The question of molecular change in solid metals urgently demands continued and rigorous investigation. Every chemist knows how much his science has gained, and what important discoveries have been made in it, by the recognition of the fact that the elements act on each other in accordance with the great law of Mendeleeff which states that the properties of the elements are periodic functions of their atomic weights. I firmly believe that it will be shown that the relation between small quantities of elements and the masses in which they are hidden is not at variance with the same law. I have elsewhere tried to show[3] that this may be true, by examining the effect of small quantities of impurity on the tenacity of gold.

In the case of iron, it is difficult to say what property of the metal will be most affected by the added matter. Possibly the direct connection with the periodic law will be traced by the effect of a given element in retarding or promoting the passage of ordinary iron to an allotropic state; but "the future of steel" will depend on the care with which we investigate the nature of the influence exerted by various elements on iron, and on the thermal treatment to which it may most suitably be subjected.

Is it not strange that so many researches should have been devoted to the relations between carbon, hydrogen, and oxygen in organic compounds, so few to the relations of iron and carbon, and hardly any to iron in association with other elements? I think that the reason for the comparative neglect of metals as subjects of research arises

[1] *Stahl und Eisen*, ix. 1889, p. 728.
[2] "Das Härten des Stahles," p. 78 (Leipzig, 1881). See also Lœwenherz, *Zeitschrift für Instrumentenkunde*, ix., 1889, p. 322.
[3] *Bull. U.S. Geo. Survey*, No. 27, 1886, p. 51.
[4] Sir Humphry Davey, *Thomson's Ann. Phil.*, i., 1813, p. 131; quoted by Turner, Proc. Phil. Soc., Birmingham, vi., 1889, part 2.

[1] *Bull. U.S. Geo. Survey*, No. 35, 1886, p. 51.
[2] *Phil. Mag.*, June 1884, p. 462.
[3] Phil. Trans. Roy. Soc., clxxix., 1888, p. 339.

from the belief that methods which involve working at high temperatures are necessarily inaccurate; but the school of Ste. Claire-Deville has shown that they are not, and there are signs among us that our traditional love for the study of metals is reviving. Of course it cannot be that chemists and physicists are afraid "that science will be degraded by being applied to any purpose of vulgar utility," for I trust that I shall at least have shown that the empire over matter, and the true advancement of science, which I suppose is the object of all research, may be as certainly secured in the field of metallurgy as in any other.

PROF. WEISMANN'S "ESSAYS."

PROF. WEISMANN'S suggestions are, with reason, universally recognized as being most important and valuable; nevertheless certain questions treated of by him seem to me to require further solution, and at present to constitute difficulties which oppose themselves to an entire acceptance of his hypotheses.

Death in the Metazoa is, according to him, due (new translation, Clarendon Press, p. 21) to the cells of their tissues having ceased to be able to reproduce themselves—in "the limitation of their powers of reproduction." Such a cessation may be an inevitable result of an excessive amount of work or efficiency on their part, and "the advantages gained by the whole organism" might, as he says (p. 61), "more than compensate for the disadvantages which follow from the disappearance of single cells."

But granting all this, how did such a process begin? Some Metazoon must have been the first to die through this failure of reproduction in its component tissue-cells. Yet if the Protozoa were, and are (as Prof. Weismann represents), naturally immortal, the first Metazoa must have been entirely composed of immortal cells, and therefore themselves potentially immortal. Granted that cell-aggregations become every now and then accidentally dissolved, that would be "accidental death." Why should natural death arise, and, if it did, what advantage could ensue from the failure of cell-reproduction? It could not benefit the race, because as yet there was no race, but only individual clusters of naturally immortal cells which had happened to divide imperfectly. The Professor tells us (p. 29) it is "conceivable that all cells may possess the power of refusing to absorb nutriment, and therefore of ceasing to undergo further division." But how and why should a cell begin, for the very first time, to practice this abstinence? That it should do so, is, of course, like many other things "conceivable," but to my judgment it does not appear credible. Of course when once we have a race of mortal organisms propagating by germ cells, it is easy enough to understand how such a race would be benefited by the death of the "useless mouths" belonging to it, and therefore by the cessation of the tissue-reproduction which leads to such death. The difficulty lies in the natural death of the very first Metazoa which ever lived. Here, as in so many cases, it is "the first step" which tries us. How, from this perennial race of microscopic immortals, are we to obtain our first Metazoon *naturally* mortal?

By the hypothesis, each component cell consists of a form of protoplasm which has the power of growing and dividing. It is not easy to see how the mere coalescence of such cells can lead any one, or any set, of such cells to acquire an altogether new power—that of reproducing the whole complex organism of which it has come to be a part? The Professor tells us (p. 27) that probably "these units soon lost their primitive homogeneity. As the result of mere relative position, some of the cells were especially fitted to provide for the nutrition of the colony, while others undertook the work of reproduction." Referring to *Magosphæra planula*, he says (p. 75):—"Division of labour would produce a differentiation of the single cells in such a colony: thus certain cells would be set apart for obtaining food and for locomotion, while certain other cells would be exclusively reproductive." But how can the fact of a cell happening to fall into a position "especially fitted" for the performance of a certain function, lead to its performing this function? Supposing that the physical influences of the environment have modified the arrangement, or cohesion, size, or number of molecules in a cell, or modified their molecular motions, how can such influences give it a power, not of reproducing its thus "acquired" characters, or the characters of the cell before it becomes thus differentiated, but of reproducing the whole organism whereof it forms a part? Is it credible that any impacts and reactions thus occasioned should produce so marvellous a result? I do not know any phenomena in Nature which could warrant us in entertaining such a belief.

Of course, if we were dealing with races of creatures sexually reproduced, it is conceivable enough that, out of multitudinous, indefinite, minute accidental changes in the arrangements of the molecules of their germs, favourable arrangements might be selected in the struggle for life. But we are here concerned with nothing of the kind, but with the first appearance of the earliest Metazoa reproduced. If we meditate on the conditions affirmed by the Professor to have produced that origin, it will, I think, be clear that no hypothesis suggested by him will answer the question how any of the cells of the first coherent colonies came to reproduce, not such cells as their ancestors (or, rather, the earlier living portions of their very selves) had by countless processes of fission produced, but a whole "cell-colony," such as that whereof they had, by the hypothesis, for the first time come to form a part.

With respect to the immortality of Monoplastides and the question of death generally, he (the Professor) makes various remarks which do not appear to be satisfactory. The process of spontaneous fission, he says (p. 25), "cannot be truly called death. . . Nothing dies, the body of the animal only divides into two similar parts possessing the same constitution." Where such a perfect similarity exists we may say not only that there is no death, but also that there is no birth. In some of the Monoplastides, however, the relationship between parent and offspring does exist, but this, of course, need not necessarily involve death; as we see in higher species and in our own. But the fact that death does not take place during, or soon after, fission, does not prove that death never naturally occurs at all, and that the cell can balance its metabolism indefinitely. Very likely it may be able so to do, but this can hardly be affirmed to be an absolute certainty. What may be certainly affirmed is that reproduction by fission does not entail death to the degree that sexual reproduction entails it. But reproduction by gemmation may equally fail to entail death; as we see in the parthenogenetic Aphis and many Hydrozoa.

In *Euglypha* we can, as Prof. Weismann admits (p. 64), recognize the daughter cell (which is for a time without a nucleus, and we also find a very marked distinction between the segments of transversely dividing Infusorians; where one has to form a new mouth and the other a new anus.

After all that can be urged, then, in contrasting the multiplication by fission of Monoplastides with reproduction in the life-cycle of Polyplastides, there seems to me to be more of a true reproductive process in the former than the Professor is disposed to allow. In some *Heliozoa* and *Ciliata* we have all the complexity of indirect nucleus division by karyokinesis, while in *Euglypha* we have cell division without any antecedent separation of the nucleus into two parts. Of course it is easy enough to understand how a mere augmentation in bulk may overcome cohesion, how internal molecular arrangement may cause cleavage along definite lines, and, perhaps, even how such cleavage may be insured through an increase of mass in proportion to a relatively diminishing surface nutrition. But such a division would be much simpler than a process of karyokinesis, and certainly than the formation of a new mouth and a new anus. Here there is no question of a part (p. 73) growing "to resemble the whole," comparable to the regrowth, by crystallization, to replace a fragment broken from a crystal. We have a whole which divides itself in such a way as to initiate and carry out a progressively increasing *difference*—a difference between the two parts dividing, and a difference (but a different kind of difference) between each such part and the previously existing whole.

Passing from the consideration of the immortality of Monoplastides to the mortality of Polyplastides, I cannot see my way to accept the Professor's definition (p. 114) of death: "An arrest of life, from which no lengthened revival, either of the whole or any of its parts, can take place," nor can I agree to his assertion (*loc. cit.*) that death "depends upon the fact that the death of the cells and tissues follows upon the cessation of the vital functions as a whole." If we cut up a *Begonia* plant or a *Hydra* into small parts, such an individual *Hydra* or *Begonia* cannot surely be considered as still alive, because fresh *Hydræ* or *Begoniæ* may spring from such fragments. Similarly with higher organisms, it would be preposterous to say that a man was not dead because a *post-mortem*, inferior kind of life—such as can alone be manifested in very lowly structures—was still persisting in the cells of his tissues!

No doubt, as the Professor says, we cannot have death without a corpse, but the tissues and cells of the corpse may still retain a certain sort of life without the corpse being any the less a corpse on account of that circumstance.

But if life of some sort may be, as we agree, affirmed of such cells, can we deny it absolutely (since no one comprehends it) even to the molecules of the cells? But body-tissues of lower Vertebrates may retain such life for a very long time. If, then, such a Vertebrate be devoured by another animal, who would venture to affirm that it is impossible that some of the micellæ or tagmata, or at least the molecules of some of the cells of the creature devoured may not pass, while still retaining a sort of life, into the tissues of the devourer? Even tagmata must be small enough to traverse the tissues, and can the possibility that they may enter into their composition while still living be dogmatically denied? May we not affirm the certainty of the death of the animal devoured till we are sure of the impossibility of the survival of any of the molecules of its cells?

No doubt the Professor would refer us to *Magosphæra* as presenting phenomena (so far as regards its cells) which support his view. He says (p. 126):—"The dissolution of a cell colony, with its component living elements, can only be death in the most figurative sense, and can have nothing to do with the real death of the individuals; it only consists of a change from a higher to a lower stage of individuality. . . . Nothing concrete dies in the dissolution of *Magosphæra*; there is no death of a cell colony, but only of a conception." But surely it cannot be the same thing "to exist in a coherent interrelated mass bound together by a common jelly," and "to exist in separate parts, living independently without interrelations, and not bound together by a common jelly." If there is here "death of a conception," there must be an external objective death corresponding therewith. *Magosphæra* is a very lowly organism, and its life can be very little better than that of a Monoplastid, because its structure is very little more complex. It is not wonderful, then, that there is very little difference between its existence and the existence of its *post-mortem* surviving cells. Yet the difference must be allowed to be, however diverse in degree, like that in the higher animals. Let us suppose that half a dozen higher animals could be so divided that no two cells remained in contiguity, yet that every cell could retain a *post-mortem* life such that by reuniting they could build up other individuals. Would it be reasonable to affirm that the higher animals thus segmented had not been *killed*, or that when their cells had reunited—possibly in very different combinations—the individual animals were the same ones as before? An extreme illustration often best seems to bring out the force and significance of a principle.

The *Orthonectides*, referred to (p. 126) by the Professor in controversy with Götte, hardly illustrate the question here discussed, but we note with much interest and satisfaction that he is inclined to regard them as arrested larvæ, Leuckart having found them[1] greatly to resemble the new-born young of *Distoma*, as Gegenbaur has found that the Dicyemids are like a stage in the development of the Platyhelminthes. If this interpretation is, as it probably is, correct, we have here an interesting example of what we find in such *Batrachians* as *Axolotl* and *Triton alpestris*. I am inclined to look at *Menobranchus*, *Proteus*, and *Siren* as larval forms which have now altogether ceased to assume what was once the adult stage of their existence.[2]

Prof. Weismann's hypothesis concerning heredity is certainly the best which has yet been proposed, but I have not met with any reference to that proposed by Sir Richard Owen forty years ago.[3] It is now out of date, and his references are not of course expressly to "germ-plasm," but to the contents of germ-cells. Nevertheless, there is an undeniable resemblance between the two hypotheses, and any interested in Prof. Weismann's would do well to read over Owen's small volume on the same problem.

But the complexity of Prof. Weismann's hypothesis is such as to approach, if it does not even exceed, that of pangenesis itself.

He tells us (p. 191): "Every detail of the whole organism must be represented in the germ-plasm by its own special and peculiar arrangement of the groups of molecules," and (p. 146) that "the number of generations of somatic cells which can succeed one another in the course of a single life, is predetermined in the germ." Moreover none of these circumstances can be explained by any difference of quality,[4] but must be exclusively due to the size, number, and arrangement of the component parts. Now, if we consider what must be the complexity of conditions requisite to determine once for all in the germ the precise number of all the succeeding cells of epithelial tissue, including every one of the rapidly succeeding cells of glandular epithelium, and every blood corpuscle of the whole of life; to necessitate also every modification of structure which may successively appear in polymorphic organisms, which change again and again profoundly between the egg and the imago; to arrange, at starting, the successive very complex changes of arrangement which must be necessary to build up reflex mechanisms

1 "Zur Entwicklungsgeschichte des Leberegels," *Zool. Anzeiger*, 1881, p. 99.

2 In this connection may be noted a passage which occurs on p. 266 of Prof. A. C. Haddon's excellent introduction to the study of embryology. Sollas is there quoted as saying that a longer mature life is possessed by those forms which are "saved from the drudgery of a larval existence." It would be interesting to know whether *Rana opisthodon* is longer lived than its congeners, since it has no tadpole stage of life.

3 See his work "On Parthenogenesis" (Van Voorst, 1849). There we read:—"Not all the progeny of the primary impregnated germ-cell are required for the formation of the body in all animals. Certain of its derivative germ-cells may remain unchanged and become included in the body which has been composed of their metamorphosed and diversely combined or confluent brethren; so included, any derivative germ-cell *or the nucleus* of such may commence and repeat the same processes," &c. (p. 5). At p. 68 he speaks of "the retention of *some* of the primary germ-vesicles." Finally, on p. 72, he says:—"How the retained spermatic force operates in the formation of a new germ-process from a secondary, tertiary, or quaternary derivative germ-cell or *nucleus*, I do not profess to explain; neither is it known how it operates in developing the primary germ-mass from the impregnated germ-vesicle of the ovum. In both we witness centres of repulsion and of attraction antagonizing to produce a definite result."

4 P. 101, where the existence of "quality" is denied.

capable, not only of compelling complex instinctive actions occurring at one time of life, but of so successively changing as to be able successively to make necessary the successively occurring very different instinctive actions of different periods of life, as *e.g.* in *Sitaris.* But this is by no means all. The arrangement of the molecules must be such as not only to effect all this, but also all the constitutional pathological inherited modifications which are to arise at different periods of life, and all the capabilities of reaction upon stimuli of every cell, of every tissue, and every predisposition an organism may possess—"predisposition" and "capacity" being nothing more than names for a certain collocation of particles so built up as inevitably to fall down into other collocations—upon shock and impact—the original collocation again being such as to insure not only that the first ensuing collocation from impact shall be of an appropriately definite kind, but that its definiteness shall be such as to insure that all the succeeding varied collocations from successive impacts shall also be appropriately definite. I confess I do not believe that such a collocation of particles is possible.[1]

This, however, is, after all, only a portion of the difficulty from complication, necessarily involved in Prof. Weismann's hypothesis of germ-plasm. For we have to consider the modifying effect on the germ-plasm produced by its effecting those developmental changes which it is its own business to effect. After speaking of the great complexity of the germ-plasm in higher animals, he goes on (p. 191) to say:—"This complexity must gradually diminish during ontogeny, as the structures still to be formed from any cell, and therefore represented in the molecular constitution of the nucleoplasm, become less in numbers; . . . the complexity of the molecular structure decreases as the potentiality for further development also decreases, such potentiality being represented in the molecular structure of the nucleus."

According to the hypothesis, the whole organism at every stage of its existence is but a collocation of molecules of different sizes most complexly arranged. Amongst them, during development, are the portions of germ-plasm, everywhere building up the increasingly complex structures of the developing body, while they themselves are simultaneously decreasing in complexity of composition. Now, it seems somewhat difficult to conceive of such a mass, which may thus be said to both decrease and increase simultaneously in complexity, both centripetally and centrifugally, and yet to preserve its complexity both centrally and sporadically, as must be the case in order to effect sexual reproduction and such repair of tissues after injury, as the organism may be capable of. Prof. Weismann continues:—"The development of the nucleoplasm during ontogeny may be, to some extent, compared to an army composed of corps which are made up of divisions, and these of brigades, and so on. The whole army may be taken to represent the nuceloplasm of the germ-cell: the earliest cell-division (as into the first cells of the ectoderm and endoderm) may be represented by the separation of the two corps, similarly formed, but with different duties: and the following cell-divisions by the successive detachment of divisions, brigades, regiments, battalions, companies, &c.; and as the groups become simpler so does their sphere of action become limited. It must be admitted that this metaphor is imperfect in two respects: first, because the quantity of the nucleoplasm is not diminished, but only its complexity; and, secondly, because the strength of an army chiefly depends upon its numbers, not on the complexity of its constitution." A better illustration of the Professor's conception would seem to be that of an army very complexly organized sending off successively regiments of different kinds, but always retaining in the centre a few men of all arms, and always being recruited by rustics (the food of the germ-plasm), who become organized by the central reserve of all arms retained for that purpose.

[1] Prof. Weismann sees clearly enough the fatal complexity of the parallel hypothesis of Nägeli, who would explain all this by "conditions of tension and movement." "How many different conditions of tension," our author remarks (p. 182), "ought to be possessed by one and the same idioplasm, in order to correspond to the thousand different structures and differentiations of cells in one of the higher organisms? In fact, it would be hardly possible to form even an approximate conception of an explanation based upon mere conditions of tension and movement."

But how, according to this or any other conceivable illustration, are we to understand the germ-plasm becoming simplified by forming tissues and organs, and then regaining its complexity so as to be able to effect the various reparative growths which constantly take place after non-fatal injuries? Or if we are to deem that the germ-plasm only regains a portion of its complexity—one portion in one place, another in another—how can we conceive of the germ-plasm being so divided that each part of the body has just that portion of germ-plasm which is needed for its reproduction, in spite of that being the very portion which we might expect to have been exhausted, since it is it which has built up that part of the body.

Moreover, all these processes of succession, progression, simplification, and possible recomplication, of the germ-plasm itself, must, according to the hypothesis, have been laid down and necessitated in the first original collocation of the molecules of the germ. This seems to me to exceed the bounds of credibility.[1]

But if the hypothesis of germ-plasm be deemed one involving too much complexity for belief—that is, if the conditions supposed by it are deemed inadequate to explain the results of sexual ontogeny—the hypothesis seems yet more unsatisfactory with respect to processes of reparative growth and reproduction by gemmation. This is a subject the Professor has not yet expressly treated, and therefore some suggestions with respect to its difficulties may be welcome to him, as showing what elucidations some minds seem to require. He, however, tells us (pp. 197, 211, and 322) that such processes of growth are due to the presence of germ-plasm, and of course not so to hold would be to abandon his hypothesis. It is, however, difficult to understand how we can thus account for the reproduction of a human elbow with a joint structurally and functionally much as the old one (see "On Truth," pp. 170-171). Are we to understand that germ-plasm in all its complexity was there? If so, is it universally diffused through the organism as well as present in the sexual glands, and why does it not produce rather an embryo than an elbow-joint? If *not*, how comes it that the germ-plasm present happened to have the complexity needed to effect that which was, anatomically and physiologically, effected? With respect to germination generally, the Professor says (p. 322):—"The germ-plasm which passes on into a budding individual, consists, not only of the unchanged idioplasm of the first ontogenetic stage (germ-plasm), but of this substance altered so far as to correspond with the altered structure of the individual which arises from it, viz. the rootless shoot which springs from the stem or branches. The alteration must be very slight, and perhaps quite insignificant, for it is possible that the difference between the secondary shoots and the primary plant may chiefly depend upon the changed conditions of development,[2] which takes place beneath the earth in the latter case and in the tissues of the plant in the former."

[1] The term "Zielstrebig," as one used to denote a practically teleological process which is not really teleological, is a remarkable example of the mode in which we are led to regard the invention of a new name as an *explanation*.

[2] The remarkable readiness with which the fertile mind of Prof. Weismann excogitates hypotheses on hypotheses to explain away difficulties is rather remarkably shown by the way in which he tries to obviate the objection to his view as to parthenogenesis, which arises from the fact that in the bee the same egg will develop into a drone or not, according as it has or has not been fertilized. This would seem to emphatically contradict his doctrine, that the one cause of parthenogenesis is the greater amount of germ-plasm which exists in parthenogenetic eggs than in ordinary ones. He meets this by suggesting (p. 237) that if the spermatozoon reaches the egg it may, under the stimulus of *internal causes, grow to double its size*, thus obtaining the dimensions of the segmentation nucleus." What may *not* be thus explained?

Surely this is a very inadequate and even misleading statement of the matter. It is surely inconceivable that a portion of protoplasm should be affected in these diverse but most definitely diverse ways by the environment of earth and plant-tissues respectively. The radicle and plumule are formed (*e.g.* in the bean) while still surrounded by the tissues of the parent plant, but no radicle is formed in a growth by gemmation. Even if in all cases a radicle was formed, which radicle became largely developed under the stimulus of earth-environment, it would be difficult to understand why it should atrophy or metamorphose itself within those very plant-tissues under the influence of which it was itself first formed.

Again, as regards the Begonia leaf, if it is such germ-plasm as Prof. Weismann conceives of, which determines the development of such a leaf into a plant, what can be supposed to make it different from the germ-plasm of the seed? However complex may be the germ-plasm of Begonia, it must be a definite complexity. The germ-plasm cannot be simultaneously built up in two different ways. But a molecular arrangement which compels growth from a seed cannot possibly be the same as a molecular arrangement which compels growth from a leaf. The initial stages of the two processes are quite different.

Certainly the influence of the environment is sometimes very surprising; but these surprising results hardly, at least at first sight, seem to harmonize with Prof. Weismann's views. Thus the effect of the movements of the young of *Cynips*, newly hatched from an egg deposited in the tissues of a plant (p. 302), is to cause it to produce a gall—a result "advantageous to the larva but not to the plant." It causes "an active growth of cells" around the larva, much to that larva's advantage. Now surely it is too much to ask us to believe that the germ-plasm of the plant, in the first instance, before even, say, a single *Cynips* had visited it, had in the complex collocation of its molecules, an arrangement such as would compel the plant which was to grow from it, to grow these cells and form a gall as just mentioned.[1] However this may be, the production of the gall is certainly a curious effect of the action of the environment on an outgrowth from germ-plasm, conceived of as Prof. Weismann conceives of it.

But the question of the actual or possible influence of the environment suggests some further difficulties which can hardly fail to occur to any critical reader of what Prof. Weismann says concerning the inheritance of acquired characters. Although he absolutely denies that changes induced in the *soma* by the action of the environment, can be transmitted to a succeeding generation, he yet allows (p. 98) that the germ-plasm itself may be modified through the action of the environment on the *soma* increasing its nutrition, and such modifications, on his hypothesis, would be inherited. But if it is true, as stated, that oysters transported to the Mediterranean become rapidly modified, that the *Saturnia* imported to Switzerland from Texas become modified so as to transmit new characters in one generation, and that cats in Mombas, turkeys in India, and greyhounds in Mexico, have also been modified, their modifications being transmissible, it is very difficult to understand how such changed climatic conditions, or increased or diminished nutrition, could change the molecular structure of the germ-plasm in such a way as to compel the production in a second generation of modifications either so induced in the *soma* of the first, or of a nature appropriate to the conditions presented by a changed environment.

That the wild pansy does not change at once when planted in garden soil, and yet in the course of generations gains new characters which are propagated by seed, he explains (p. 433) by a modification of germ-plasm thus induced. But such an admission is enough to satisfy much of what is demanded by those who assert the inheritance of acquired characters. After all, such an inheritance must be due to the *soma*, since it is only through it that the germ-plasm can be modified.

If this effect on the germ-plasm itself is thus cumulative, may it not be partly due to a cumulative effect on the soma which transmits to the germ-plasm the actions which modify the latter? Can this be declared to be absolutely impossible? Anyhow, it is plain that effects of the environment on Polyplastides may be transmitted to succeeding generations. There are, however, still more striking phenomena amongst mammals which do not seem to accord with Prof. Weismann's theories. I refer to the production of offspring which resemble not their father, but the father of preceding offspring—as in the well-known case of Lord Zetland's brood mare, and the puppies of thoroughbred bitches which have once been coupled with a mongrel. How can the germ-plasm of the first father have been acquired by the offspring of a subsequent father? I have ventured to propose these questions, which must of course have occurred to many other naturalists, feeling sure that Prof. Weismann will be glad to have his attention drawn to a few points, a further explanation of which seems necessary for the acceptance of his most interesting hypotheses.

September 2. ST. GEORGE MIVART.

[1] It would be very interesting to know how "natural selection" (to the action of which, as everybody knows, Prof. Weismann constantly appeals) could have caused this plant to perform actions which, if not self-sacrificing (and there must be some expenditure of energy), are at least so disinterested. No doubt the Professor has an hypothesis to produce, though he only says (p. 302) here that "it would be out of place to discuss here the question."

NOTES.

THE Medals of the Royal Society have this year been awarded as follows:—The Copley Medal to the Rev. Dr. Salmon, F.R.S., for his various papers on subjects of pure mathematics, and for the valuable mathematical treatises of which he is the author; a Royal Medal to Dr. W. H. Gaskell, F.R.S., for his researches in cardiac physiology, and his important discoveries in the anatomy and physiology of the sympathetic nervous system; a Royal Medal to Prof. Thorpe, F.R.S., for his researches on fluorine compounds, and his determination of the atomic weights of titanium and gold; and the Davy Medal to Dr. W. H. Perkin, F.R.S., for his researches on magnetic rotation in relation to chemical constitution. Intimation has been received at the offices of the Royal Society that the Queen approves the award of the Royal Medals.

WE regret to learn that another officer of the Geological Survey of India has fallen a victim to the Indian climate. Mr. E. J. Jones, who joined the Survey in 1883, died of dysentery at Darjiling on October 15, at the age of thirty. Mr. Jones was an Associate of the Royal School of Mines, and having also studied chemistry at Zürich and Würzburg, he was a valuable member of the Survey, to the publications of which he contributed several geological and chemical papers.

TO add to the many obligations under which he has laid Cambridge University, Prof. Sidgwick has offered to give £1500 towards the completion of the new buildings urgently required for physiology, on condition that the work is undertaken forthwith. The Financial Board has accordingly recommended a scheme by which this can be effected. The alliance between mental science and physiology which this gift represents is a bright feature of Cambridge studies at present.

THE University of St. Andrews is to be congratulated on an extraordinary piece of good fortune. The sum of £100,000 has been bequeathed to it by Mr. David Berry, who died last September. Mr. Berry was a native of Cupar, Fife, and in 1836 went to Australia, where he ultimately inherited the estate of his brother, Dr. Alexander Berry. The latter had been a student of the St. Andrews University, and at the time of his death it was understood that he had left an unsigned will bequeathing a quarter of a million to his *alma mater*, but giving permission to his brother David to carry out the provisions as he might think proper. The legacy will not come into the possession of the University until 1894.

IN addition to the botanical appointments named last week, the following are announced from Russia:—Prof. Faraintzin having resigned his post of Professor of Botany in the University of St. Petersburg. Prof. Borodin has been appointed in his place. M. W. Palladin succeeds the late Prof. Pitra as Professor of Botanical Anatomy and Physiology in the University of Charkow; and is himself succeeded in the Botanical Chair in the Agricultural Academy at Nowo-Alexandria by M. Chmielewski. M. W. Rothert has been appointed Lecturer on Botanical Anatomy and Physiology at the University of Kasan.

IN the November number of the *Kew Bulletin* a curious correspondence is printed which illustrates very well the nature of some of the duties undertaken by the Kew officials. Towards the end of December 1876, Dr. Hooker received from the Colonial Office a letter inclosing a despatch in which the Governor of Labuan suggested that it might be well to promote in Labuan the cultivation of the African oil palm. A long correspondence followed, the result of which was that full and accurate information as to the palm oil industry was obtained from the Gold Coast, and transmitted to Labuan. Palm oil nuts were also obtained, and in due time planted in the fertile island of Daat, where no fewer than 700 healthy trees were soon raised. It recently occurred to Mr. Thiselton Dyer to make inquiry as to the later history of this interesting experiment. A despatch from the Acting Governor of Labuan to the Colonial Office, dated August 1, 1889, and forwarded to Kew, closes the correspondence. It is as follows:—"As reported in Mr. Treacher's despatch No. 72, of August, 26, 1878, it appears that 700 of these palms were raised in the island of Daat, and in due time produced nuts. No attempt, as far as I am aware, was ever made to manufacture any oil from the nuts, and last year the palms were all removed to make room for cocoa-nut trees. Daat, a dependency of this colony, is private property, and I venture to suggest that, should any further information be required by Mr. Thiselton Dyer, he should apply to the owner, Dr. Peter Leys, who is now in England, and who would no doubt be glad to supply it. The experiment, so far as I am in a position to judge, was a success."

THE authorities of the Royal Gardens, Kew, are always glad to aid any dependency of the Empire in introducing and establishing any new plant which promises to serve as the foundation of a new industry. The documents relating to the oil palm in Labuan show how much work may be involved in the carrying out even of a simple scheme of this nature, and how disappointing the results may be. "The enterprise," says the *Bulletin*, "is suggested; it is considered; a plan for carrying it out has to be matured; all the necessary incidental information has to be collected; and then the plan is carried into execution. Sometimes it fails the first time, and then a second attempt has to be made, and so on till success is secured. All that then remains is to wait for the result; and this, in any appreciable shape, will in most cases not be reached for years. But in the interval Governors and officials change. It may be, though it is not always so, that the ardour with which the experiment was launched evaporates with the individual whom it inspired. A new Colonial Government *régime* may regard with apathy and even hostility the work of its predecessor, and the whole enterprise may fall into oblivion till some chance inquiry on the same subject leads to the digging out of the file of papers containing its record from the Kew archives."

THE remaining contents of the *Kew Bulletin* relate to Phylloxera regulations at the Cape, Ramie or Rhea, and the collecting and preserving of fleshy Fungi.

THE Manchester Field Naturalists' Society has formed a special committee, with Mr. Leo Grindon, the President of the Society, as botanical referee, and Mr. C. J. Oglesby, as convener, for the purpose of determining which trees, shrubs, and flowers will succeed in the squares and streets of the city. The opinion prevails that, notwithstanding the unfavourable climatic conditions, several forest trees, climbers, and hardy plants would grow if special care were taken in planting and tending them. The planting of the quadrangle at Owens College, of the infirmary esplanade (in the centre of the town), and of several churchyards, has been attended with success.

THE following money-grants have been lately made by the Berlin Academy of Sciences:—£75 to Prof. Brieger, for continuation of his researches on the ptomaïnes; £60 to Dr. Krabbe, for investigation of the Cladoniaceæ of the Hartz; £30 to Dr. von Dankelmann, for utilization of meteorological observations at Finschhaven in New Guinea; £20 to Dr. Assmann, for measurements of air-temperature on the Säntis; £100 for publication of Prof. G. Finsch's work on Torpedineæ; £50 for publication of a memoir by Dr. Heiden, on the development of *Hydrophilus piceus*; £100 to Dr. Strehlmann, in Zanzibar, for prosecution of his faunistic researches in East Africa; £125 to Prof. Lepsius, of Darmstadt, for preparation of his geological map of Attica; £50 to Prof. Conwentz, for investigation of silicified wood in the island of Schonen; £75 to Dr. Fleischmann, of Erlangen, for researches in development; and the same to Dr. Zacharias (Silesia), for micro-faunistic studies.

THE first meeting of the one hundred and thirty-sixth session of the Society of Arts will be held on Wednesday, November 20, when the opening address will be delivered by the Duke of Abercorn, Chairman of the Council. Before Christmas there will be four ordinary meetings, in addition to the opening meeting. The following arrangements have been made:—November 27, Dr. J. Hall Gladstone, F.R.S., "Scientific and Technical Instruction in Elementary Schools"; December 4, Dr. Armand Ruffer, "Rabies and its Prevention"; December 11, Mr. H. Trueman Wood, "The Paris Exhibition"; December 18, Sir Robert Rawlinson, "London Sewage."

A NOVEL and interesting application of science to art may now be seen at the Arts and Crafts Exhibition, where Mrs. Watts Hughes shows specimens of what she calls "voice figures" (Catalogue, No. 723). These are practically Chladni's figures produced in a viscid medium. Semi-fluid paste is spread on an elastic membrane stretched over the mouth of a receiver. A single note "steadily and accurately sung" into the receiver throws the paste into waves and curves. The patterns formed are either photographed immediately after production, or are transferred as water-colour impressions while the membrane is still vibrating. Fanciful names, *e.g.* "wave, line, flower, tree, fern," are given to these; the effect, especially in transparencies, is very beautiful. Some of the forms would repay the study of physicists as well as of artists; the most interesting are perhaps the "daisy forms," in which we are told that "the number of petals increases as the pitch of the note which produces them rises." The apparatus employed is not exhibited, and the descriptive label is not very clear, but we understand that Mrs. Hughes would be most pleased to explain the matter to anyone scientifically interested in it: her address is 19 Barnsbury Park, N.

FOR determination of the air-temperature at great heights, the Berlin Society for Ballooning (we learn from *Humboldt*) is going to try a method of Herr Siegsfeld, who uses a thermometer, which, by closure of an electric circuit when certain temperatures are reached, gives a light-signal. Small balloons, each containing such a thermometer, will be sent up by night, and the light will affect photographically a so-called "photo-theodolite," while the height then attained will be indicated in a mechanical way. It is hoped that more exact formulæ for the decrease of temperature with height may thus be obtained.

THE rapid decrease in the number of kangaroos is beginning to attract the attention of scientific Societies in Australia. From the collective reports of the various stock inspectors it was estimated that in 1887 there were 1,881,510 kangaroos. In 1888 the number fell to 1,170,380, a decrease of 711,130. The chief obstacle to the adoption of measures for the effectual protection of the kangaroo is his vigorous appetite. One full-grown kangaroo eats as much grass as six sheep; and graziers—who as a class are not, it is to be feared, readily accessible to the influence of sentiment—find that the food eaten by this interesting animal might be more profitably utilized otherwise. In a communication on the subject, lately submitted to the Linnean Society of New South Wales, Mr. Trebeck suggested that the National Park might be used for the preservation not only of kangaroos but of very many members of the Australian fauna and flora.

AT the monthly meeting of the Royal Society of Tasmania on September 9, the President (His Excellency Sir Robert G. C. Hamilton) said he desired to bring before the Society a matter relating to the young salmon at the Salmon Ponds. These were the undoubted product of the ova brought out by Sir Thomas Brady, which had been stripped from the male and female fish and artificially fertilized, and the utmost care had been taken to keep them apart from any other fish bred in the ponds. He recently visited the ponds, accompanied by the Chairman of the Fisheries Board, the Secretary, and two of the members, when they carefully examined a number of the young salmon, among which they were surprised to find marked differences existing, not only in size, but in their characteristics. It has often been held that the *Salmonidæ* caught in Tasmanian waters cannot be true *Salmo salar* because so many of them have spots on the dorsal fin, and a tinge of yellow or orange on the adipose fin, but nearly half of the young salmon they examined, which had never left the ponds, had these characteristics. Again, many of them were almost "bull-headed" in appearance—another characteristic which is not supposed to distinguish the true *Salmo salar*. He would suggest to the Chairman of the Fisheries Board, whom he saw present, that the Secretary should be asked to make a formal report of the result of this visit, and to obtain some specimens of the young fish, which could be preserved in spirits, and perhaps sent to Sir Thomas Brady to be submitted for the consideration and opinion of naturalists at home.

AT the same meeting of the Tasmanian Royal Society, Mr. James Barnard read a remarkably interesting paper on the last living aboriginal of Tasmania. It has hitherto been generally believed that the aboriginal Tasmanians are extinct. Mr. Barnard, however, contends that there is still one survivor—Fanny Cochrane Smith, of Port Cygnet, the mother of six sons and five daughters, all of whom are living. She is now about fifty-five years of age. Fanny's claims to the honour of being a pure representative of the ancient race have been disputed, but Mr. Barnard makes out a good case in her favour. He himself remembers her as she was forty years ago, when there were still about thirty or forty natives at Oyster Cave; "and certainly at that time," he says, "I never heard a doubt expressed of her not being a true aboriginal."

THE Caucasus is a region of great interest in the study of prehistoric times, and a fresh impulse was lately given to its exploration, by Beyern's discovery of an extensive burial-ground south of Kura (in the district of the Anticaucasus). At the recent annual meeting of the German Anthropological Society, Dr. Virchow gave some account of this bed (which Beyern has named after General Repkin). The region is rich in ores, but bronze articles are absent; for, while copper is plentiful, there is no tin. On the other hand, various ornaments of pure antimony have been met with; also antimony buttons (or knobs), like those of Beni-Hassan in Egypt. The ground is largely of volcanic nature, and many articles of obsidian (chiefly knives and arrow-heads) have been found in the graves. One curious find was that of a skeleton having an arrow-head of obsidian in one of the leg-bones, partly overgrown by a callus. The metallic girdles in this burial-ground have figures of animals engraved on them; in the Koban ground, such figures are confined to the clasp, but this, in the Repkin ground, is wanting.

PROF. EDWIN J. HOUSTON contributes to the November number of the Journal of the Franklin Institute a short paper on a hail-storm at Philadelphia, October 1, 1889. After noting various points common to most hailstones, he refers to a characteristic which he had never before observed. "On some of the hailstones," he says, "though not in the majority of them, well-marked crystals of clear transparent ice projected from their outer surfaces for distances ranging from an eighth to a quarter of an inch. These crystals, as well as I could observe from the evanescent nature of the material, were hexagonal prisms with clearly-cut terminal facets. They resembled the projecting crystals that form so common a lining in geodic masses, in which they have formed by gradual crystallization from the mother-liquor. They differed, however, of course, in being on the outer surface of the spherules."

IN *Das Wetter* for October, Dr. W. J. van Bebber discusses a paper, by the late Prof. Loomis, on the rainfall of the earth. The following are noted as some of the conditions favourable to rain: (1) an unsettled state of the atmosphere, caused by unusually high temperature, with great humidity, a condition which occurs when the pressure is below the average value; (2) cold northerly or westerly winds on the west side of a depression, by which the winds on the east side receive a stronger impulse; (3) proximity to mountains, the ocean or large lakes; (4) deep depressions of small area and steep gradients. With regard to the rainfall which accompanies barometric depressions, it is found that in the United States, south of latitude 36° N., a rainfall of 2·5 inches occurs oftener on the east side than on the west side of a depression in the ratio of 2·6 : 1; on the eastern side of the Rocky Mountains, a rainfall of 9 inches occurs more frequently on the east than on the west of a barometric minimum, in the ratio of 6·2 : 1. In the North Atlantic Ocean, the ratios of large rain areas on the east and west sides of a depression are as 2·6 : 1; while in Europe a rainfall of 2·5 inches in twenty-four hours on the east and west sides of a depression occurs in the ratio of 2 : 1. The rainfall with a falling or rising barometer is also investigated.

WE have received the fifth and last part of vol. i. of M. Fabre's comprehensive "Traité Encyclopédique de Photographie" (Paris: Gauthier-Villars, 1889). The subject of lenses is considered in great detail, and the theory and use of diaphragms are fully gone into. The relation of the time of exposure to the subject and lens employed is also considered, and studios, dark rooms, and their various accessories are fully described and illustrated. From both the theoretical and practical point of view the work still bears out its original promise of becoming the most complete one on the subject.

A SECOND edition of Prof. Tait's "Light" (A. and C. Black) has been issued. The author says that in revising the work he has made use of various notes jotted down from time to time on his own copy, mainly as the result of questions asked, or of difficulties pointed out, by students who were reading the book with care. Suggestions of this kind he has found to be almost always of value, as they tend to make the book better suited to the wants of the class of readers for whom in particular it was designed.

PERSONS interested in ferneries and aquaria will find much to attract them in a little volume entitled "Ferneries and Aquaria: a Complete Guide to their Formation, Construction, and Management," by George Eggett, Sen. This is one of a series of "practical guide-books" issued by Messrs. Dean and Son.

THE third volume (new series) of the *Reliquary* (Bemrose and Sons) has been issued. It opens with an interesting illustrated article on two Assyro-Phœnician shields from Crete, by the Rev. Joseph Hirst. Mr. John Ward contributes three illustrated papers of scientific value—on Rains Cave, Longcliffe, Derbyshire; on relics of the Roman occupation, Little Chester, Derby; and on recent diggings at Harborough Rocks, Derbyshire.

MESSRS. DULAU AND CO. have sent us a "Catalogue of Zoological and Palæontological Works." It includes works on Reptilia and Amphibia, and on Pisces.

THE atomic weight of palladium has been redetermined by Dr. E. H. Keiser (*Amer. Chem. Journ.*). Among all the atomic weights at present adopted by chemists, that of palladium has been one of the most imperfectly determined, for the discrepancy between the results of the various previous investigations is most unsatisfactory. In 1826, Berzelius obtained the value 113·63 from a consideration of the proportion in which palladium combines with sulphur. Two years later, the same distinguished chemist derived a much lower value from analyses of potassium palladious chloride, $2KCl . PdCl_2$; known quantities of this salt were heated in a current of hydrogen, and the residuary potassium chloride and reduced palladium weighed. Recalculated by Profs. Meyer and Seubert, utilizing all the refined corrections of the present day, these analyses yield the value 106·2—a number which is almost identical with the atomic weight obtained by Dr. Keiser. In 1847, however, Quintus Icilius also investigated the subject, and, from determinations of the loss in weight which potassium palladious chloride undergoes when heated in a current of hydrogen, obtained the value 111·88. No other determinations having since been attempted, and the number 112 or 113 being certainly too high from considerations of the position of palladium among the metals, the number 106·2 obtained from Berzelius's second analysis recalculated by Meyer and Seubert has been universally adopted. To place the subject out of all doubt, Dr. Keiser has re-examined it from a totally different standpoint. The double chlorides of palladium and the alkalies, such as $2KCl . PdCl_2$ and $2NH_4Cl . PdCl_2$, are found to be unsuitable for atomic weight determinations; they retain water of decrepitation with great tenacity, and, after drying, are too hygroscopic for accurate weighing. On the other hand, the yellow crystalline salt, palladammonium chloride, $Pd(NH_3)_2Cl_2$, is a much more suitable substance. It is eminently stable, can be obtained in a state of practically perfect purity, contains no water of crystallization, does not retain water after drying in a desiccator, and the dried salt is not hygroscopic. Weighed quantities of it contained in a platinum boat were introduced into a combustion tube and heated in a stream of pure hydrogen. The hydrogen was rapidly absorbed, changing the bright yellow colour into black, metallic palladium and ammonium chloride being formed. The absorption of hydrogen occurred so readily that it was only necessary to warm one end of the boat when the heat of the reaction was found sufficient to complete the reduction of the whole. $Pd(NH_3)_2Cl_2 + H_2 = Pd + 2NH_4Cl$. Af er raising the temperature so as to volatilize the ammonium chloride, the finely divided palladium adhered together in the form of a porous bar having the shape of the boat. It was allowed to cool before weighing until just below a red heat in the current of hydrogen so as to prevent oxidation, and afterwards the hydrogen was displaced by dry air to prevent its occlusion. Two series of determinations were made, the salt for the second series being prepared from the reduced palladium of the first. The mean of eleven experiments in the first series gave the number 106·352, and of eight in the second series 106·350. The maximum value obtained was 106·459, and the minimum 106·286. The mean result 106·35 practically confirms that obtained by recalculating the results of Berzelius's second analyses.

IN our note in these columns three weeks ago (vol. xl. p. 655), upon pinol, the new isomer of camphor, it was pointed out that the nitrosochloride of pinol forms with β-naphthylamine an interesting base, $C_{20}H_{24}N_2O_2$, isomeric with quinine. This base, however, is not the first isomer of quinine which has been prepared, for an artificially prepared base of the same empirical formula was described by Dr. Kohn, of University College, Liverpool, in the Journal of the Chemical Society for 1886, p. 500.

THE additions to the Zoological Society's Gardens during the past week include three Rhesus Monkeys (*Macacus rhesus* ♂ ♂ ♂) from India, presented respectively by Colonel Cuthbert Larking, Mr. James T. Wilson, and Mrs. Charles Sainsbury; a Hairy-rumped Agouti (*Dasyprocta prymnolopha*) from Guiana, presented by Mr. Henry E. Blandford; a Common Polecat (*Mustela putorius*) from Norfolk, presented by the Earl of Romney; a Northern Mocking Bird (*Mimus polyglottis*) from North America, presented by Miss E. Breton; two White Pelicans (*Pelecanus onocrotalus*), a Crested Pelican (*Pelecanus crispus*) from Roumania, a Common Boa (*Boa constrictor*), a Neck-marked Snake (*Geoptyas collaris*) from Panama, a Mocassin Snake (*Tropidonotus fasciatus*) from North America, deposited; two Common Siskins (*Chrysomitris spinus*), two Twites (*Linota flavirostris*), two Lesser Redpoles (*Linota rufescens*), four Snow Buntings (*Plectrophanes nivalis*), two Knots (*Tringa canutus*), a Bar-tailed Godwit (*Limosa lapponica*), British, a Rosy-billed Duck (*Metopiana peposaca* ♂) from South America, purchased.

OUR ASTRONOMICAL COLUMN.

OBJECTS FOR THE SPECTROSCOPE.

Sidereal Time at 10 p.m. at Greenwich, November 14 = 1h. 36m. 45s.

Name.	Mag.	Colour.	R.A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) { G. C. 385	—	—	1 35 30	+50 50
{ G. C. 386	—	—	1 36 29	+50 51·5
(2) 57 Ceti	6	Yellowish-red.	1 54 36	−21 16
(3) ζ Ceti	3	Yellow.	1 45 32	−10 55
(4) δ Cassiopeiæ ...	3	Bluish-white.	1 18 36	+59 40
(5) 7 Schj....	7·0	Reddish-yellow.	1 10 5	+25 11
(6) R Pegasi	Var.	Red.	23 1 7	+ 9 57
(7) V Tauri	Var.	Reddish.	4 45 50	+17 21

Remarks.

(1) This is one of Herschel's double nebulæ. Dr. Huggins notes that both components give a gaseous spectrum, but could only be certain of the presence of the chief nebula line near 500, although 495 was strongly suspected. He notes, also, that there

is a faint continuous spectrum at the preceding edge of No. 386. The point chiefly requiring attention at present is the character of the line near 500. Many recorded observations describe this line as having a fringe of light on the more refrangible side, whilst others state that it is perfectly sharp on both edges. Low dispersion only should be employed in making this observation. The observation of continuous spectrum in a special part of the nebula 386 is also worthy of attention; the spectrum should be examined for maxima of brightness, as in the case of the nebula in Andromeda.

(2) Dunér records this as a star of Group II. (see below), but states that the spectrum is very feebly developed. The star is probably, therefore, either just condensing into a fully-developed star of Group II., or is just passing into Group III. If the former, there will practically be nothing but very narrow bands, and if the latter, absorption *lines* will accompany the bands. In the earlier stages of this group, the bands in the blue are strongest, whilst in the later stages red bands are strongest, and this point should also receive attention. As a check, the colour of the star should be noted at the time of observation.

(3) This star belongs to either Group III. or to Group V., and the criteria (see p. 20) should be observed in order to determine which.

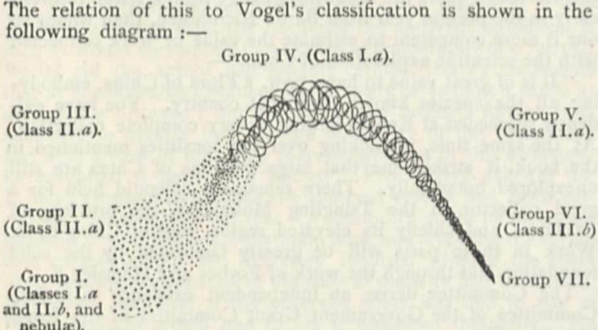
(4) According to Vogel, the spectrum of this star is of the same type as α Lyræ, *i.e.* Group IV. The relative intensities of the metallic lines and those of hydrogen, which vary from star to star, should be noted for future classification of the stars of this group according to temperature.

(5) This is a star of Group VI. Dunér describes the spectrum as consisting of four zones, the zones being the bright spaces between the dark carbon flutings. The presence of slight traces of carbon absorption in the solar spectrum indicates that stars of this group only differ in temperature from stars like the sun. The passage from one group to the other will probably be found to be very gradual, and the widths of the carbon flutings and the presence or absence of other absorptions should therefore be noted.

(6) Period given by Gore as 382 days, and magnitude at maximum (November 13) as 6·9–7·7. The spectrum has not yet been recorded, and the present maximum may, therefore, conveniently be taken advantage of.

(7) Period given by Gore as 168 days, and magnitude at maximum (November 15) as 8·3–9. Spectrum not yet recorded.

Note.—Lockyer's classification will, in future, be exclusively used, so that there will be no necessity for a double reference. The relation of this to Vogel's classification is shown in the following diagram:—



Lockyer's Temperature Curve.

The temperature increases from Group I. to Group IV., and then decreases to Group V. On the ascending side of the "temperature curve" we have probably to deal with condensing meteoritic swarms; and, on the descending side, with gradually condensing masses of meteoritic vapours.

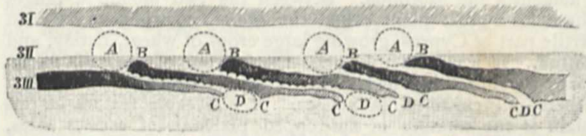
A. FOWLER.

LARGE-SCALE CHARTS OF THE CONSTELLATIONS.—Mr. Arthur Cottam has projected a series of thirty-six most excellent charts of the constellations from the North Pole to between 35° and 40° of south declination, and showing stars in half magnitudes down to 6½ by disks of various sizes. Although the primary object in constructing these charts was to make them companions to Webb's "Celestial Objects for Common Telescopes" and Smyth's "Cycle of Celestial Objects," their scope has been considerably enlarged, and a number of double, multiple, and variable stars have been laid down which are not included in either of the above-mentioned works. The Earl of Crawford's (Dun Echt) summary of F. G. W. Struve's Dorpat Catalogue included 2248 double and multiple stars, and of them, 2130 are shown upon these charts. In addition to this, 275 of the double stars discovered by Mr. S. W. Burnham have been mapped, this being the whole of those included in his first four catalogues, and a selection from his other catalogues. The maps have been drawn to a scale of one-third of an inch to a degree, which is a much larger scale than any hitherto published, and as each map includes but a small portion of the heavens, there is practically no distortion, whilst the epoch being 1890, the positions will hold good, without any serious errors, for fifteen or twenty years beyond that date. The projection is conical, or, in those charts which extend any distance both north and south of the equator, cylindrical. Hence it will be easy to lay down any additional objects that may be required. There is no doubt that these charts will be eminently useful, one of their great advantages being that they will enable possessors of telescopes mounted on altazimuth stands or without circles to find with ease a large number of interesting objects, and thus will help to extend the knowledge of the heavenly bodies and to popularize the most fascinating of sciences. We may say that the publisher of these charts is Edward Stanford, Cockspur Street, S.W., and that the first issue is limited to 200 sets, many of which have been already subscribed for.

BARNARD'S COMET, II. 1889, MARCH 31.—The following ephemeris is given in *Astronomische Nachrichten*, No. 2931:—

1889.	R.A. h. m. s.	Decl. ° ′	1889.	R.A. h. m. s.	Decl. ° ′
Nov. 6	1 8 54	−16 30·2	Nov. 22	0 28 2	−17 25·4
7	5 49	−16 37·2	23	26 3	−17 25·7
8	2 49	−16 43·6	24	24 8	−17 25·6
9	0 59 53	−16 49·5	25	22 17	−17 25·2
10	57 1	−16 54·9	26	20 29	−17 24·7
11	54 13	−16 59·8	27	18 45	−17 23·9
12	51 29	−17 4·1	28	17 5	−17 22·8
13	48 50	−17 8·1	29	15 28	−17 21·6
14	46 15	−17 11·6	30	13 55	−17 20·0
15	43 44	−17 14·8	Dec. 1	12 25	−17 18·3
16	41 17	−17 17·4	2	10 58	−17 16·3
17	38 55	−17 19·7	3	9 34	−17 14·3
18	36 36	−17 21·5	4	8 13	−17 12·0
19	34 21	−17 22·9	5	6 56	−17 9·7
20	32 11	−17 24·0	6	5 41	−17 7·1
21	30 5	−17 24·9	7	4 29	−17 4·4
22	28 2	−17 25·4	8	3 20	−17 1·5

THE STRUCTURE OF JUPITER'S BELT 3, III.—This dark band appears under ordinary conditions to be made up of two parallel bands, but Dr. Terby (*Astronomische Nachrichten*, No. 2928) says this appearance of parallelism is the result of the special structure represented in the accompanying figure, and



Structure of Jupiter.

that, therefore, the band 3, III., is composed of a lot of dark bands inclined in the same direction. The circular parts A are distinguished by Dr. Terby as emitting a sort of diffused light of an entirely different character from the white equatorial spots, properly so called; these luminous balls seem always to occur at the interval between two of the inclined bands, and touching what is generally their darkest part, B. The brilliant white spots D also appear at the dissolution of two successive bands, and occupy by preference their northern extremities. When the definition was very good, Dr. Terby observed that the interval between two of these fragmentary bands had the appearance of a series of globules, as shown in the figure. The structure appears so general and regular that it may be the means of adding considerably to our knowledge of the physical constitution of this planet.

GEOGRAPHICAL NOTES.

AT the first meeting of the session of the Royal Geographical Society, the paper was on Cyprus, by Lieut.-General Sir Robert Biddulph, G.C.M.G., C.B. The island of Cyprus is the third largest in the Mediterranean, being inferior in size only to Sicily and Sardinia. Its area is 3584 square miles. Its principal features are two mountain ranges, running pretty well parallel to each other from east to west. The northernmost of these two ranges extends almost the whole length of the island from Cape Kormakiti on the north-west to Cape St. Andrea at the end of the horn-like promontory which stretches for 40 miles from the north-east of the island. This promontory is called the Carpas, and the low mountain chain running through it is called the Carpas range. The westernmost and higher portion of the northern range is called the Kyrenia range, and rises to an altitude of 3340 feet. This range is of a remarkably picturesque outline, in some parts extremely rugged. It is mostly a single ridge without any remarkable spurs, and its summit is about two miles from the northern coast. It can be crossed in many places. The chief mountain peaks of this range are Kornos, 3105 feet; Buffavento, 3140; and Pentedaktylos, 2400. The last named is a remarkably shaped rock in the centre of the Kyrenian range, owing its name to its shape, the word Pentedaktylos signifying in Greek "five-fingered." Beneath this rock there rushes out southward from the mountain side, at an altitude of 870 feet, a torrent of water, which never ceases to flow summer or winter, and which, descending into the great plain in the centre of the island, carries its fertilizing streams to the lands of several villages, its course marked by mills, gardens, and trees, until its water is exhausted by various irrigating channels. A similar stream of water gushes from the northern side, about 12 miles west of the Kyrenia Pass. Smaller streams descend on either side of the range at various places; their waters are used for irrigation in the valleys. The southern range of mountains is of a much more extensive nature than the northern range. The easternmost point of this range is the mountain of Santa Croce, so called from the church of the Holy Cross which stands on its summit. This mountain, which is 2260 feet in height, is of a peculiar shape. Beginning then from this point the southern range rapidly rises to considerable altitudes, finally culminating in Mount Troodos, the highest point in Cyprus, being 6406 feet above the sea-level. The other chief peaks in the southern range, are Adelphe, 5305 feet; and Machera, 4674 feet. But it is not only in altitude that the Troodos range is distinguished; numerous spurs run down to the north and south, and as we proceed further west these radiate out to greater distances, so that half way between Troodos and the sea, the mountain range is not less than 20 miles wide. Here there are very considerable forests, many miles in extent, rarely visited save by wandering flocks and by wood-cutters, and affording shelter to the moufflon, or wild sheep of Europe, some 200 or 300 of which still roam over these hills. On the map it will be seen that numerous rivers descend from both sides of the southern range. These are mostly dry in summer, but after rain their waters descend with violence, filling up the river-beds in the plains, carryiny away trees and cultivated patches, and often rushing in a turbid stream into the bays of Famagusta and Morphou. Between the two mountain ranges there lies a great plain called the Mesaorea, which is the most fertile part of Cyprus, growing large crops of wheat, barley, and cotton. It was evidently once the bottom of the sea, for in many parts are large beds of marine shells—gigantic oysters and others—all clustered in masses. A noticeable feature of this plain is the number of flat-topped plateaux of various sizes, where the rock seems to have resisted the action of the water. The tops of these plateaux are clothed with short herbage, affording a scanty provision for flocks, and are usually from 100 to 200 feet above the plain. The rivers which descend from the hills carry down large quantities of alluvial soil, and this forms in the eastern part of the Mesaorea a rich deposit, something similar to the Delta of the Nile. The two rivers which mainly contribute to this plain are the Pediæus and the Idalia, the former taking its rise from the northern slopes of Mount Machera, and the latter from the eastern slopes of the same mountain. The beds of these rivers have, however, become so choked up with alluvial deposit towards the end of their course, that their waters overflow the plain and mingle together, so that their separate mouths can with difficulty be distinguished. The normal condition of these rivers is to be without water, but whenever there is a heavy rainfall in the mountains, the river "comes down," as it is called, and runs for one, two, or more days. It occasionally happens that the water descends with great suddenness and violence, causing disastrous floods. Considerable supplies of water for irrigation purposes are obtained by sinking wells. A long chain of wells are sunk at distances of five or six yards apart, and being connected by underground galleries, a channel is thus formed which conveys the water to a reservoir constructed at the foot of the last well, and it is thence raised to the surface by a water-wheel; or in some cases the level of the ground admits of the channel being brought out on the surface. In this way the town of Nicosia is supplied with excellent water, which is brought in two aqueducts from a distance of some miles. Larnaca and Famagusta and other towns have similar aqueducts. Closely connected with the water supply is the forest question. Sir Robert Biddulph then entered into detail with reference to the denudation of Cyprus of its forests, and the grêat locust-plagues which have been so successfully treated since the British occupation.

THE FLORA OF CHINA.[1]

SINCE the last meeting of the British Association, two additional parts of the "Index Floræ Sinensis" have been published, bringing the enumeration of known, and the description of new, species as far as the *Loganiaceæ*. The Committee now, therefore, look forward with some confidence to the completion of their labours at no distant date.

Further extensive and valuable collections have been received from China in aid of the work, more especially from Dr. Augustine Henry, late of Ichang. The novelty and richness of the material obtained by this indefatigable botanist far exceeds any expectations the Committee could have formed. It is to be regretted that his duties as an officer of the Chinese Imperial Maritime Customs have necessitated his removal to Hainan. It is probable, however, that he had practically exhausted the immediate neighbourhood of Ichang, and that without opportunities of travelling over a wider radius, which the Committtee regret they were unable to procure for him, he would not have been able to add much of material novelty to the large collections already transmitted by him to Kew.

The Committee have met with the kindest sympathy and assistance in their labours from Dr. C. J. de Maximowicz, of the Académie Impériale of St. Petersburg, who has long been engaged on the elaboration of the collections made by Russian travellers in China, and from M. Franchet, of the Muséum d'Histoire Naturelle at Paris, who is describing and publishing the extremely rich collections made by the French missionaries in Yunnan.

The Committee have received striking proofs of the appreciation of their labours by botanists of all countries. They permit themselves to quote the following passage from a letter received early in the present year from Baron Richthofen, than whom no one is more competent to estimate the value of work connected with the scientific exploration of China:—

"It is of great value to have, now, a Flora of China, embodying all the species known from that country. You have evidently succeeded at Kew in getting a very complete collection. At the same time, in looking over the localities mentioned in the book, it strikes me that large portions of China are still unexplored botanically. There remains a splendid field for a good collector in the Tsingling Mountains, the province of Sz'chuen, and chiefly its elevated region west of Ching-tu-fu. Work in those parts will be greatly facilitated by the solid foundation laid through the work of Forbes and Hemsley."

The Committee derive an independent existence as a Sub-Committee of the Government Grant Committee of the Royal Society. They are at present in possession of sufficient funds to enable them to carry on the work. They do not therefore ask for their reappointment at the hands of the British Association.

SCIENTIFIC SERIALS.

American Journal of Science, October.—Assuming that the earth's crust rests on a layer of liquid as a floating body, Mr. Le Conte here offers an explanation of normal faults. The crust is supposed to be raised into an arch, by intumescence of the liquid, caused by steam or hydrostatic pressure; it is thus broken by long more or less parallel fissures into oblong prismatic

[1] Third Report of the Committee, consisting of Mr. Thiselton-Dyer (Secretary), Mr. Carruthers, Mr. Ball, Prof. Oliver and Mr. Forbes, appointed for the purpose of continuing the preparation of a Report on our present knowledge of the Flora of China.

blocks, which, on relief of the tension by escape of lava or vapour, are readjusted by gravity, in new positions. The blocks may be rectangular in section, but are more likely to be rhomboidal or wedge-shaped; giving level tables with fault cliffs (as in the plateau region) in the one case, and tilted blocks with normal faults (as in the basin region) in the other. The author considers the Sierra and Wahsatch to have been formed by lateral crushing and folding; and the region between to have been arched, broken, and readjusted, as described, in the end of the Tertiary.—Two determinations of the ratio of the electromagnetic to the electrostatic unit are furnished from the Johns Hopkins University; one made this year, by Mr. Rosa, by Maxwell's method of measuring a resistance, the other ten years ago, by Messrs. Rowland, Hall, and Fletcher, by measuring a quantity of electricity electrostatically, and then measuring it electromagnetically with a galvanometer. The former gives $v = 2{\cdot}9993 \times 10^{10}$ centimetres per second; the latter, $2{\cdot}9815 \times 10^{10}$ centimetres. It seems certain, according to Mr. Rosa, that v is within a tenth per cent. of 300 million metres per second.—Mr Long continues his account of the circular polarization of certain tartrate solutions; and his experiments point to a law that the rotation of a double tartrate may be made to approach that of a neutral tartrate of either of the metals present, by addition of a salt of that metal (the effects being apparently explained by substitution).—Mr. Eldridge proposes a new grouping and nomenclature for the middle Cretaceous in America.—There are also papers on the gustatory organs of the American hare (Mr. Tuckerman); on the output of the non-condensing engine, as a function of speed and pressure (Mr. Nipher); and on some Florida Miocene (Mr. Langdon).

SOCIETIES AND ACADEMIES.

London.

Physical Society, November 1.—Prof. Reinold, F.R.S., President, in the chair.—The following communications were read:—On a new electric-radiation meter, by Mr. W. G. Gregory. The meter consists of a long fine platinum wire attached to a delicate magnifying spring of the Aryton and Perry type, and stretched within a compound tube of glass and brass. At the junction between the wire and spring a small mirror is fixed. When the tube is placed parallel to a Hertz's oscillator in action, the mirror is turned in a direction indicating an extension of the wire. The arrangement is so sensitive that an elongation of $\frac{1}{200000}$ of a mm. can be detected, and when placed at a distance of 4 metres from the oscillator the apparent extension is such as would correspond to a change of temperature of $0^{\circ}{\cdot}003$ C. By its aid the author has roughly verified Hertz's statements that at considerable distances the intensity of radiation varies as the inverse distance; but before he can proceed further it is necessary to greatly increase the sensibility of the apparatus; and with a view of obtaining some suggestions in this direction, he exhibited it before the Society. Prof. Perry asked if the E.M.F. required to produce the observed results had been calculated; he also believed that the sensibility might be increased by using copper instead of platinum wire, and replacing the spring by a twisted strip. Mr. Blakesley inquired whether the effect of increasing the capacity of the ends of the wire had been tried. Mr. Boys said that if the observed effect was due to rise of temperature he would like to see it measured thermally. He also thought the effect might be due to extension caused by rapid electric oscillations in some such way as the elongation of an iron bar caused by magnetization. In answer to this, Prof. S. P. Thompson said the matter had been investigated experimentally, but with negative results. Prof. Herschel suggested the use of a compound spring such as is used in Breguet's metallic thermometers. In reply, Mr. Gregory said he had estimated the E.M.F. by observing that a Leclanché cell through 50 ohms produced about the same result. No improvement in sensitiveness was obtained by using copper wire or by increasing its capacity, and attempts to measure the rise of temperature by an air thermometer had been given up as hopeless. The President, in thanking the author for his paper, congratulated him on the ingenuity and courage displayed in producing an apparatus to measure such microscopic quantities as are here involved.—On a method of driving tuning-forks electrically, by Mr. Gregory. In order to give the impulses about the middle of the stroke, the fork is arranged to make and break the primary circuit of a small transformer, the secondary circuit of which is completed through the electromagnet actuating the fork. The prongs of the fork are magnetized and receive two impulses in each period. Another device was suggested, where the prongs respectively operate contacts which successively charge and discharge a condenser through the coils of the actuating magnet. Prof. S. P. Thompson said the methods, if perfect, would be of great service, and suggested that a fork so driven be tested optically by comparison with a freely vibrating one. He regarded the mercury contacts used as objectionable, for their capillarity and adhesion would probably cause the impulses to lag behind the appointed epochs. Prof. McLeod remarked that Lissajous' figures gave a satisfactory method of testing the constancy of period, and could be readily observed without using lenses, and in reference to liquid condensers suggested by the author for his second device, said that platinum plates in sulphuric acid were found to disintegrate when used for this purpose. He thought lead plates would prove suitable. Prof. Jones, who read a paper on a similar subject in March last, said he now used bowed forks, with which to synchronize the speed of the disk there described, and the frequency is determined by causing the disk to complete the circuit of his Morse receiver once each revolution.—On a physical basis for the theory of errors, by Mr. C. V. Burton. After pointing out that the law of error for any particular measurement depends on the nature of the conditions governing such measurement, the author considers several simple cases, and deduces their curves of error. A kinematic method of combining two or more independent errors, each following known laws, is then described and applied, and the general formula obtained leads to Laplace's law of error in the case of an infinite number of similar errors. Referring to Most Advantageous Combinations of measures, it is shown that the method of least squares is only a particular solution of the general equation, and is derived by assuming the individual errors to conform to Laplace's law. Subjective errors are next considered, and in conclusion the author says that "the law of error in a set of observations depends on the nature of each special case, and what may be called the probable law of error is determined by our knowledge of the conditions. The combination of three or more sources of error of comparable importance gives in general a law not seriously differing from that of Laplace, so that the method of least squares will be practically the most advantageous, except where a single source of error with a very different law is predominant above all the rest."—A note on the behaviour of twisted strips, by Prof. J. Perry, F.R.S., had been prematurely announced by mistake, and he accordingly gave only a brief outline of the paper. In a previous communication, Prof. Ayrton and the author enunciated a working hypothesis in which the strips were imagined to be split up into pairs of filaments, each pair acting as a bifilar suspension. The resulting formula for the rotation produced by a given load did not agree with experiment, and quite recently the author had recognized why the formula was incorrect. The bifilar law they had assumed was only true for small twists, but he now saw another method of treatment by which he hoped to verify the formula derived from experiment before the next meeting. Prof. Fitzgerald reminded Prof. Perry of a method of attacking the problem suggested by the speaker some time ago, in which each filament was supposed to be wrapped round a smooth cylinder; and said that on working it out the formula was found to be very complicated. Mr. Trotter thought the pairs of strips might be regarded as twisted ladders, and Mr. Gregory said this suggestion reduced the problem to a series of bifilar suspensions which had already been worked out.—On electrifications due to contact of gases and liquids, by Mr. J. Enright. For some time past the author has been studying the electrical phenomena attending solution, by connecting an insulated vessel in which the solution takes place with an electrometer. As a general rule, no effect is observed if nothing leaves the vessel, but when gases are produced and allowed to escape the vessel becomes charged with + or − electricity, depending on the nature of the liquid from which the gas passes into the air. As an example, when zinc is placed in hydrochloric acid, the deflection of the electrometer is in one direction whilst the liquid is chiefly acid, but decreases and reverses as more and more zinc chloride is produced. From such observations the author hopes to obtain some information relating to atomic charges. Owing to the lateness of the hour, the latter portion of the paper and the discussion on it were postponed until next meeting.

PARIS.

Academy of Sciences, Nov. 4.—M. Des Cloizeaux, President, in the chair.—Instrument for measuring the coefficient of elasticity of metals, by Mr. Phillips. This is a large spiral spring and balance wheel, the former made of the metal to be examined. —*Rôle* and mechanism of the local lesion in infectious diseases, by M. Ch. Bouchard. Whereas in absolute immunity, there is, after inoculation, neither general infection nor local lesion, and in total absence of immunity, general infection, often without local lesion, in relative, normal, immunity there is local lesion mostly without general infection; in the last case, as experiment shows, it is not the local lesion that causes the immunity, but *vice versâ*. Inoculating vaccinated and unvaccinated rabbits with pyocyanic Bacillus, the author found, in the former, rapid appearance of leucocytes, all having many Bacteria, which were soon resolved into granulations, and in sixteen hours were quite gone; while the free Bacteria soon decreased in number. In the other animals, few leucocytes, no Bacilli in them, and free Bacteria multiplying.—Statistics of preventive treatment of rabies, from February 9, 1888, to September 15, 1889, at the Pasteur Institute of Rio de Janeiro (Dr. Ferreira dos Santos), by the Emperor of Brazil. Of 156 who underwent full treatment, only one died, and not certainly from rabies; this gives a mortality of 0·64 per cent.—On the velocity of wind at the top of the Eiffel Tower, by M. A. Angot. Three months' observations give a mean of 7·05 m. as compared with 2·24 m. at the Central Meteorological Office (21 m. from the ground). While at low stations there is a minimum at sunrise and a maximum at 1 p.m., the Eiffel (like mountains) showed a minimum about 10 a.m. and a maximum at 11 p.m. (while at midday there was but a slight upward bend of the curve).—On phenyl-thiophene, by M. A. Renard. This is prepared by passing through an iron tube, heated to dark redness, vapours of toluene and of sulphur, and distilling the condensed product. Analysis gave the formula $C_6H_5—C_4H_3S$. With bromine, nitric acid, and sulphuric acid, substitution products are obtained.—Researches on digitaline and tanghinine, by M. Arnaud. By heating digitaline with baryta-water to 180° for several hours, it combines with water yielding the compound $C_{31}H_{52}O_{11}$, from which the formula $C_{31}H_{50}O_{10}$ is deduced for digitaline. The formula of tanghinine, similarly deduced, is $C_{27}H_{40}O_8$. This formula differs from that of Schmiedeberg for digitaline, viz. $C_{21}H_{32}O_7$.—Studies on the embryology of the axolotl, by M. F. Houssay. He describes the mechanics of segmentation, the origin and development of the peripheral nervous system, and the morphology of the head.—On the cytoplasm and the nucleus in Noctiluci, by M. G. Pouchet. Flemming's chromatine seems to be formed of two substances, chromatoplasm and hyaloplasm; and the proportion of the former increases as gemmation proceeds; hence the more and more lively colour of the segmented nuclei.—On the parasitic castration of *Typhlocyba* by a Hymenopterous larva (*Aphelopus melaleucus*, Dalm.), and a Dipterous larva (*Atelenevra spuria*, Meig.), by M. A. Giard. In *T. hippocastani*, the eight terminal branches of the penis are reduced to six, four, or three. A pair of curious invaginations on the ventral surface of the body are also shortened.—Action of serum of diseased or vaccinated animals on pathogenic microbes, by MM. Charrin and Roger. Operating with the pyocyanic Bacillus and rabbits, they found the serum of vaccinated animals more adverse to growth of the Bacillus than normal serum, but somewhat less than that of the diseased animals.—Contribution to the semeiological and pathogenic study of rabies, by M. G. Ferré. Inoculating by trepanation, and with stronger virus than before, they found that the respiratory acceleration appeared on the fourth instead of the fifth day; the respiratory centres being invaded correspondingly sooner. The symptoms could not be attributed to thermal elevation, the maximum of this occurring later.—Statistics of preventive inoculations against yellow fever, by Dr. Domingos Freire. From 1883 to 1889, there were 10,524 persons inoculated in Brazil; and the mortality was 0·4 per cent. The deaths of non-vaccinated during the four epidemics were over 6500.—On the modifications in normal gaseous exchanges of plants by the presence of organic acids, by M. L. Mangin. He injected malic, citric, and tartaric acids into leaves of Japanese prick-wood, bay rose, and lilac, and found these leaves to behave like Cacteæ and Crassulaceæ. In the dark, the volume of carbonic acid liberated is greater than that of oxygen absorbed; and in the light, there is emission of oxygen without correlative absorption of carbonic acid.—On the existence of numerous zeoliths in the gneissic rocks of Upper Ariège, by M. A. Lacroix.

DIARY OF SOCIETIES.

LONDON.

THURSDAY, NOVEMBER 14.

MATHEMATICAL SOCIETY, at 8.—Isoscelian Hexagrams: R. Tucker.—On Euler's φ-Function: H. F. Baker.—On the Extension and Flexure of a Thin Elastic Plate: A. B. Basset, F.R.S.

INSTITUTION OF ELECTRICAL ENGINEERS, at 8.—On the Lighting of the Melbourne Centennial International Exhibition: K. L. Murray.

FRIDAY, NOVEMBER 15.

PHYSICAL SOCIETY, at 5.—On the Electrification due to the Contact of Gases and Liquids: J. Enright.—On the Effect of Repeated Heating and Cooling on the Electrical Resistance and Temperature Coefficient of Annealed Iron: H. Tomlinson, F.R.S.—Notes on Geometrical Optics, Part II.: Prof. S. P. Thompson.

INSTITUTION OF CIVIL ENGINEERS, at 7.30.—The New Harbour and Breakwater at Boulogne-sur-Mer: S. C. Bailey.

MONDAY, NOVEMBER 18.

ARISTOTELIAN SOCIETY, at 8.—Scepticism: S. Alexander.

TUESDAY, NOVEMBER 19.

INSTITUTION OF CIVIL ENGINEERS, at 8.—Water-Tube Steam-Boilers for Marine Engines: John I. Thornycroft.

ROYAL STATISTICAL SOCIETY, at 7.45.—Opening Address by the President, Dr. T. Graham Balfour, F.R.S.

WEDNESDAY, NOVEMBER 20.

GEOLOGICAL SOCIETY, at 8.—On the Occurrence of the Striped Hyæna in the Tertiary of the Val d'Arno: R. Lydekker.—The Catastrophe of Kantzorik, Armenia: M. F. M. Corpi. Communicated by W. H. Hudleston, F.R.S.—On a New Genus of Siliceous Sponges from the Lower Calcareous Grit of Yorkshire: Dr. J. G. Hinde.

ROYAL METEOROLOGICAL SOCIETY, at 7.—Second Report of the Thunderstorm Committee—Distribution of Thunderstorms over England and Wales, 1871-87: William Marriott.—On the Change of Temperature which accompanies Thunderstorms in Southern England: G. M. Whipple.—Note on the Appearance of St. Elmo's Fire at Walton-on-the-Naze, September 3, 1889: W. H. Dines.—Notes on Cirrus Formation: H. Helm Clayton.—A Comparison between the Jordan and the Campbell-Stokes Sunshine Recorders: F. C. Bayard.—Sunshine: A. B. MacDowall.—On Climatological Observations at Ballyboley, Co. Antrim: Prof. S. A. Hill.

SOCIETY OF ARTS, at 8.—Opening Address by the Chairman, the Duke of Abercorn, C.B.

UNIVERSITY COLLEGE CHEMICAL AND PHYSICAL SOCIETY, at 4.30.—Pyridine and the Alkaloids: Dr. N. Collie.

CONTENTS.

PAGE
Science and the Future Indian Civil Service Examinations 25
The Lund Museum in the University of Copenhagen 26
Hydraulic Motors. By **A. G. G.** 27
Physiology of Education. By **J. H. G.** 28
Our Book Shelf:—
Whitham: "Steam-Engine Design."—**N. J. L.** 29
Hake: "Coloured Analytical Tables" 29
Tidy: "The Story of a Tinder Box" 30
Jamieson: "Magnetism and Electricity" 30
Ball: "Time and Tide; a Romance of the Moon" . . 30
Letters to the Editor:—
Specific Inductive Capacity.—**Prof. Oliver J. Lodge, F.R.S.** 30
Who discovered the Teeth in Ornithorhynchus?—**Prof. W. H. Flower, F.R.S.; Oswald H. Latter** . . 30
"La Pietra Papale."—**Dr. P. L. Sclater, F.R.S.** . 31
On a Mite of the Genus *Tetranychus* found infesting Lime-trees in Leicester Museum Grounds.—**F. R. Rowley** 31
Retarded Germination.—**E. A.** 31
The Relation of the Soil to Tropical Diseases.—**Surgeon A. Ernest Roberts** 31
The Earthquake of Tokio, April 18, 1889.—**Prof. Cargill G. Knott** 32
A Brilliant Meteor.—**Paul A. Cobbold** 32
On the Hardening and Tempering of Steel. II. (*Illustrated.*) By **Prof. W. C. Roberts-Austen, F.R.S.** 32
Prof. Weismann's "Essays." By **Dr. St. George Mivart, F.R.S.** 38
Notes 41
Our Astronomical Column:—
Objects for the Spectroscope. (*With Diagram.*)—**A. Fowler** 44
Large-Scale Charts of the Constellations 45
Barnard's Comet, II. 1889, March 31 45
The Structure of Jupiter's Belt 3, III. (*Illustrated.*) . 45
Geographical Notes 45
The Flora of China 46
Scientific Serials 46
Societies and Academies 47
Diary of Societies 48