THURSDAY, APRIL 11, 1895.

PHYSIOLOGY OF THE EXCITABLE TISSUES.

Electrophysiologie. Von W. Biedermann, Professor der Physiologie in Jena. (Jena: Fischer, 1895.)

PROF. BIEDERMANN, who was not many years ago promoted to the chair of Physiology at Jena, is well known as having co-operated with Prof. Hering in a very extended series of researches on the "general physiology of muscle and nerve," of which the results have been communicated to the Vienna Academy from time to time during the last twenty years. The present work will be welcomed by physiologists in the hope that it will not only place at their disposal the rich harvest of the author's persevering labours in this field of inquiry, but that it will afford to him the opportunity of dealing more completely than has hitherto been possible with one of the most fundamental characteristics of the animal organism—its power of responding in a specific way to stimulation. This being, as we learn from the introduction, the purpose of the book, the title "Electrophysiologie" may seem scarcely suitable, for however intimate and essential may be the relation between vital processes and the electrical phenomena which accompany them, these, after all, are only concomitants, and must be thought of apart from the process itself. However this may be, the author makes it quite clear that his scope is not electrical, but purely physiological. Referring to the "Muskel-physik" and the "Nerven-physiologie" of Prof. Hermann, published in 1880, he declares it to be his purpose to bring the subjects therein treated up to date, and in doing so to follow the true physiological method.

"In morphology it is obvious that we must proceed from the simple to the more complicated, but in physiology . . . the opposite is in a certain sense the case. Under the apparent simplicity of an amœba manifold physiological functions are latent." "The most various duties are performed by the same protoplasm, whereas in the higher animals each particular cell is devoted to a specific function, and consequently affords a better subject of study than the protozoon, when the nature of that function is to be determined." It is for this reason that "our knowledge of contraction and of the processes which are associated with it have been advanced infinitely more by muscle physiology than it ever could have been by the microscopical examination of the lower organisms." (p. 1.)

The volume before us is the first part of a work which is to include the whole of the physiology of the "excitable tissues." Its 440 pages relate directly to muscle, but also comprise the electromotive properties of epithelial and gland cells. In accordance with the general principle that the study of structure must precede that of function, the book opens with a very useful chapter of anatomical prolegomena, in which, beginning with a section on the contractile fibres of the stem of vorticella, the author proceeds to the epithelial nerve-muscle cells of the cœlenterates, and so on to those of the worms. Then come in order the muscular elements of Mollusca (Lamellibranchs and Gasteropods), of interest as showing the striking relation of structure to physiological endowment. Next we have before us the simplest forms of muscle-cells in vertebrates, and finally the structure of striped muscle, with reference to which we again find that the relations between anatomical characteristics and functions receive more attention than the endless questions of histological detail which, in the minds of most students, are associated with this subject.

Of the 340 pages given to the physiology of muscle, scarcely a third relate to its electrical properties. These comprise four sections. The first and second deal with the electromotive phenomena of muscle at rest and in action; the third relates to the so-called "positive variation"; the fourth to secondary electromotive actions. Throughout it is felt that Biedermann writes as a physiologist, not as a physicist, availing himself of the best physical methods to investigate phenomena, but regarding them not as the thing itself, but merely as indications, by the aid of which the time and place relations of vital processes become known to us. In this tendency to take things as they are, postponing theoretical speculations, until the results of the more exact methods of observation which we are now only beginning to know how to use, have been systematised, Biedermann is happily at one with all the leaders of thought and work in this field of inquiry.

Among the most interesting of the observations by which Prof. Biedermann has contributed to the advance of what may, in the truest sense, be called *elementary* physiology, are those by which, in co-operation with his master, and, it may be added, in language not unfamiliar to English students, he has sought to work out that notion of *ana* and *cata*, or of A and D, as Hering puts it, in accordance with which the influences exercised by one part of the organism on another through the nervous system, are regarded as manifestations of two antagonistic tendencies, the one quelling, the other exciting in its nature. This principle, which Prof. Hering first invoked to explain certain antagonisms relating to the perception of colour, has served Biedermann as a guide in the study of similar antagonisms in the behaviour of muscles, and he has furnished us with several instances showing that in the responses of muscles to the stimulation of their nerves, we may have to do not with spur only, but with a mixture of spur and bridle, a co-operation of quelling and exciting influences conveyed to the responding muscle by the same channel—its motor nerve.

In Biedermann's very interesting third section, the reader will find this subject fully discussed. The gist of what he has observed is as follows:—The change of form by which a muscle responds to stimulation of its nerve is not always, as is ordinarily the case, of the nature of shortening or contraction. Under certain well-ascertained conditions the "normal" effect is reversed: the muscle relaxes. By comparison of the instances in which this happens, we learn that the anomaly (if we are to call it so) depends on the terminal organ, not on the nature of the stimulus, or on the channel by which it is conducted. For it is found that muscles which normally exhibit no tendency to respond by relaxation, acquire that tendency when they have undergone "modification," either by the persistent action of certain external causes or under exceptional physiological conditions; and, moreover, that in certain invertebrates the constant condition of the muscles corresponds rather to the modified than to the ordinary state of the muscles of the higher animals. If we proceeded to inquire in what this modification consists, Biedermann's answer would be that it may be distinguished in all cases by its electrical concomitants—all "modified" parts being relatively negative to similar parts which are unmodified. Thus when the adductor muscle of the nipper of the crayfish, which is normally in a state which resembles the "modified" state of an ordinary muscle, passes into a condition corresponding to that which is normally present in the muscles of the frog, a change takes place in the electrical relations of the structure of such a character that the sign of the electrical response to stimulation is reversed; it becomes negative instead of positive. Comparing these facts with others relating to vision, with the processes of secretion, and with other processes under the immediate control of the nervous system, one is encouraged in the hope that the relations between *ana* and *cata*, between rest and activity, between restoration and exhaustion, between integrity and hurt, will eventually be linked together by their electrical concomitants, and that by these characters it may be possible to ascertain with exactitude and certainty in how far these various antagonisms may be referred to a common principle.

Space will not allow us to give an account of the last chapter in Prof. Biedermann's book, which contains much that is new and important relating to the electromotive phenomena of secreting cells. Enough has been said to satisfy the physiological reader that the book is one which will have permanent value. It is, moreover, readable. The author tells us in his preface that he has done his best to get rid of "lehrbuchmässige Trockenheit." We think that he has fairly succeeded.

J. Burdon Sanderson.

THE PHYSIOLOGY OF PLANTS.

A Popular Treatise on the Physiology of Plants, for the use of Gardeners, or for Students of Horticulture and of Agriculture. By Dr. Paul Sorauer, Director of the Experimental Station at the Royal Pomological Institute in Proskau (Silesia). Translated by F. E. Weiss, B.Sc., F.L.S., Professor of Botany at the Owens College, Manchester. (London: Longmans, 1895.)

Of all the sections into which the teacher of botany must divide his subject, perhaps the most difficult for him to deal with is that of vegetable physiology. No adequate elementary text-book has hitherto existed in English, the classical works of Sachs and Vines being, by their very completeness, too bulky and too full of detail for the student who is beginning the study of the subject. The work of Dr. Sorauer is intended to supply this deficiency. It is written especially for those whose interest in the matter is a practical one, and it deals, consequently, mainly with those questions which are of interest to the horticulturist as bearing on the problems of cultivation. Approaching the matter, however, from this side only, the book, as a work on physiology, to a certain extent comes short of what is needed, as many important sections of the subject are left untouched.

The author deals almost entirely with the problems of nutrition and their bearings on the details of horticultural practice. At the outset he strikes a rather heavy blow at what has hitherto been, unfortunately, the prominent idea of most agriculturists and horticulturists as to what constitutes the scientific side of agriculture. The latter has been held to embrace little or nothing beyond the chemistry of the soil, and everything not directly connected with this has been pushed into a secondary position, the metabolism of the plant and the chemical changes of its constituents being generally very superficially treated and held of not very great interest. It certainly seems strange that those who have been engaged especially in the cultivation of sensitive organisms under very varied and rapidly changing conditions should have given so little attention to the physiological peculiarities which these organisms present, and to their power of availing themselves of the various advantages that their environment offers to them. Dr. Sorauer, at the outset, strikes the right line when he points out that the chemistry of the soil, important as it is, plays only a secondary part in the development of the various plants which that soil supports, and directs his readers' thoughts chiefly to the problems of nutrition which the plant itself offers for consideration, showing that it is the business of the cultivator to endeavour to guide the natural development of the plant towards the special ends which are the needs of horticulture.

This idea, put prominently forward in the opening chapters, runs throughout the whole of the book. In developing it, the interactions between the soil and the plant, or between the plant and the atmosphere, are kept in the foreground, so that the gardener may realise that he is face to face with an actual struggle that is going on continuously between the organism and its environment, and that he is working to secure that in this contest those conditions of the latter may be secured which are most favourable to the success of the organism, and hence to its most complete development.

In the earlier chapters of the book, the author treats successively of the several members of the plant, the root, stem, leaf, flower, and fruit. Without going very deeply into their anatomical structure, he explains it sufficiently for the student to get an adequate comprehension of the duties or functions which are especially discharged by each. Again his mode of procedure is sound, as it is only function that gives the clue to the right interpretation of structure. The method, as carried out by Dr. Sorauer, is, however, open to a little objection, as it has led him rather to push too far the idea of the several members of the plant living for themselves, and to relegate a little to the background that of the plant being an organic whole, differentiated in different directions for the carrying out of different functions. Thus the leaf is spoken of as passing off into the vascular bundles of the axis only such materials as it cannot at once employ for its own growth, leading the student probably to the conception that the latter problem is for the leaf the primary one, and that the nutrition of the whole organism is only subsidiary when the relation of leaves to the rest of the plant is considered.

The chapters on the general processes of horticulture; the correlation of stems and roots, and their separate treatment; the principles of manuring; the theories of the proper supply of water to the plant, the aëration of its roots, &c., will be read with much attention, and will be found very useful in actual practice. The deposition of nutritive material in various parts of the plant, and the ways in which this can be influenced by different modes of treatment of the shoot at different ages, deserve careful study. The relation of such questions to the operations of pruning, grafting, &c., is discussed at some length, and the development of vegetative or fruiting branches respectively, is shown to be capable of great modification in the hands of the skilled horticulturist.

The book, however, though a valuable one for practical gardeners, leaves something to be desired as a contribution to the literature of vegetable physiology. Indeed some of the fundamental facts of the metabolism of the cell are, if not inaccurately, at any rate not clearly stated. The absorption of water is certainly one of the most important processes which it carries out. As described on p. 127, the early changes in the young cell connected with this process are stated as follows:—

"Very soon there arise within the protoplasmic mass which fills up the cells, very small quantities of liquid substance (cell sap) which collect together to form small vesicles (vacuoles), and these give to the protoplasm a foam-like structure. Now, as the protoplasm becomes used up during the elongation of the cells, the vacuoles increase in number, run together, and force the remainder of the protoplasm outwards towards the cell wall."

Put in this way, the student would get the idea that the elongation of the cells takes place apart from the entry of water, and that the formation of the vacuole, that is, the increase of the cell-sap, is rather the result than the cause of the extension of the cell. No statement is made as to the cause of the entry of the water, the osmotic activity of the cell-contents, which really underlies all cell-growth.

Another strange statement is that the main cause of the growth in length of a young stem is the longitudinal tension in the pith, which "drags the young ring of wood with it, as long as the latter is still soft and thin-walled." This will be a new view to most physiologists.

Dealing with the absorption of nitrogen by certain leguminous plants, and the recently established fact that the atmosphere is a source of supply of this constituent, the author commits himself to the view that this absorption is carried out by means of their leaves.

In section 23, which treats of the substances formed by the leaves, we find several statements to which exception must be taken. The classification of the vegetable proteids is inaccurate, the latter being said to consist of albumins, caseins, and fibrins. Albumins are extremely rare in plants, and fibrin resembling animal fibrin is unknown. Globulins, which are numerous and varied, and albumoses, which have comparatively recently been investigated, are not even mentioned. The formation of starch grains in the leaves is thus described—

"When the light shines on the leaf its transpiration is exactly increased, and its cells lose some of their water; then we may imagine the starchy substance to be forced out of the thickened (concentrated) cell-sap, and the latter will become more liquid."

The nutrition of the cell is summed up in the remarkable sentence:

"The nutrition consists in this, that the raw materials which enter the cell are attracted, now by one, now by another of the constituents of the cell, and adopt its specific movements, and are thus transformed with the same substance (assimilated)."

These drawbacks to an otherwise valuable work make one regret that Prof. Weiss has confined himself to translating Dr. Sorauer's text. Had he rather edited than translated it, no doubt we should have found the English edition free from these defects.

MINES AND MINERALS.

Traité des Gîtes Minéraux et Métallifères. Cours de Géologie appliquée de l'École Supérieure des Mines. By E. Fuchs and L. de Launay. (Paris: Baudry, 1893.)

Étude industrielle des Gîtes Métallifères. By George Moreau. (Paris: Baudry, 1894.)

IN these days, when memoirs and papers follow in rapid succession, the geologist and the miner are justly grateful to any careful compiler who will aid them to keep pace with the ever-widening flood of technical literature. The debt of gratitude is increased when the task is undertaken by a writer like Prof. de Launay, who brings together into one colossal work the results of the labours of his master, the late Prof. Fuchs, and of his own personal researches.

The book is a treatise upon applied geology, which Fuchs defined as the application of geological knowledge to seeking and working mineral deposits. Very rightly Fuchs took a wider basis for his course of lectures at the Paris School of Mines than mere ore deposits. He recognised the fact that the mining engineer of to-day must be a man of wide attainments, capable of giving an opinion upon all sorts of mineral workings, reporting upon gold mines in one journey, upon gem-diggings in the next, and a little later being asked to determine the value of deposits of phosphate of lime or other earthy minerals. For work of this description Prof. Fuchs' lectures formed an admirable kind of training, which has too often been neglected in the past; in fact, until this treatise appeared, many geologists may have failed to realise the vast importance of the subject.

While admitting that various systems of classification may be adopted, Prof. de Launay says that he felt bound to arrange his minerals according to the metalloid or the metal, and he chooses the so-called chemical order. In a huge book of this description, alphabetical order would have been more convenient, and would have prevented some of the anomalies which cannot help striking the reader. The chloride, carbonate, and sulphate of sodium appear under the head of that metal, whilst the borate and nitrate are classed respectively under boron and nitrogen; again, chalk and gypsum are brought under calcium, whilst apatite is placed under phosphorus, no doubt for the reason that the non-metallic element is the one to which the mineral more especially owes its commercial importance. While slate is sandwiched in between the gems, under the heading "Silica and Silicates," clay is relegated to "Aluminium."

In a general treatise upon mineral deposits, exception may fairly be taken to the absence of any mention of coal; the author excuses himself on the ground that fuel forms a special subject too vast to be included within the compass of his book. The overwhelming importance of coal seems, on the other hand, an additional reason for finding it a place in a work which is practically an encyclopædia of applied geology; besides, if fuel is to be omitted, why are natural gas and petroleum inserted?

In a gigantic work of this description, it is impossible for an author to avoid some errors. Five mistakes in spelling Welsh words in the first three lines of p. lxxviii. will not be looked upon as a heinous crime by the average Englishman, who himself feels that he is skating upon thin ice when he is dealing with the orthography of names of places in the Principality; however, these are not the only cases of misprints which might be noticed.

The geographical tables, occupying forty-five pages, form a useful feature in the book; an alphabetical list is given of the principal minerals worked in each country, together with references to the pages where they are described. These tables and the index of localities take the place of a general index, which is not supplied. Small maps inserted in the text are of much assistance in enabling the reader to follow the author's descriptions; the total number of woodcuts, 390, may seem large, but, scattered as they are through more than 1800 pages, they are not as numerous as one would like. Great pains have been taken with the bibliography, and Prof. de Launay's lists will often bring into notice original papers which would otherwise have passed unnoticed.

Though too expensive for the pockets of ordinary students, the book will find a place in the libraries of all mining schools and geological and engineering societies, and it is sure to be frequently consulted with profit by geologists and mining engineers.

Moreau's work has a totally different object to that of de Launay; the latter is a record of observed facts, whilst the former, dealing solely with metallic ores, is mainly devoted to general principles. After describing briefly the classifications of ore deposits proposed by von Groddeck, de Lapparent, Phillips, Whitney and Raymond, the author suggests one of his own, viz.:—

(A) Stratified deposits.
(B) Eruptive deposits.
(C) Deposits in pre-existing cavities.
(D) Substitution deposits.

Mineral veins are classed under the head (C), and apparently Moreau does not admit that they may sometimes be substitution deposits.

Two chapters are devoted to disquisitions upon the origin of mineral veins, and are based upon the works of Daubrée, Lyell, Elie de Beaumont, De la Beche, Henwood, Moissenet, Geikie, and others; Vogt's recent researches and theories remain unnoticed.

Chapter v., which treats of the chemical and physical characteristics of ores, and the earthy minerals which accompany them, should have been omitted; and the same remark applies to the chapters upon qualitative testing, quantitative assaying, dressing and smelting. These subjects cannot be taught in a few pages, and are better studied in the ordinary text-books.

Chapter vi., largely borrowed from von Groddeck, Phillips, Fuchs and de Launay, contains descriptions of well-known characteristic ore deposits. The important iron ores of the Secondary rocks are dismissed in a very cavalier fashion, for, strange to say, no mention is made of the great iron-field of the Department of Meurthe and Moselle, Lorraine and Luxemburg.

The most valuable part of the book for mining men will be found in two of the concluding chapters, which deal with the study of ore deposits from a purely commercial point of view. They are full of useful hints and warnings, not only to the engineer who is examining mining properties offered for sale, but also to the capitalist who is meditating their purchase. M. Moreau is evidently imbued with many of the ideas which were running through the brain of Mr. J. H. Collins when he delivered his recent presidential address to the Institute of Mining and Metallurgy. The more deeply counsel of this kind is taken to heart, the better will it be for investors in mining enterprises.

A few errors in the names of well-known geologists are inexcusable. It is strange that any one should write "Sir *Richard* Lyell"; "Sir Logan Foster, Whitney," is evidently meant for "Sir William Logan, Foster and Whitney"; even Elie de Beaumont's name is not always spelled correctly. "Gossan" is a Cornish and not a Welsh term, as supposed by the author.

OUR BOOK SHELF.

Elementary Text-book of Metallurgy. By A. Humboldt Sexton, F.I.C., &c. (London: Griffin and Co., 1895.)

This book is intended, for those who are commencing the study of metallurgy, as a kind of preparatory course to that of Prof. Roberts-Austen, as given in his "Introduction to the Study of Metallurgy," which the student is advised to read after digesting the contents of Prof. Sexton's manual. The work is got up in good style by the publishers, and printed in a clear and distinct manner. With the general arrangement there is very little to complain of; but the same by no means can be said of the subject-matter, which contains many errors, and the definitions are often expressed in such crude language that a student might be easily misled in taking his first lessons in metallurgy from its pages. The following examples may be cited:—P. 3: Malleability. This is the property of being expanded into sheets. Ductility: This is the property of being drawn into wire. A small quantity of antimony in lead is said to make it quite brittle. P. 4: Lead may be drawn into wire if means be taken to prevent the metal being subject to stress. P. 5: With regard to tensile testing, the author says that elongation takes place mostly near the point of fracture. P. 46: There are but two neutral substances in general use, graphite and chrome iron ore. P. 89: Fluor spar is said to be used to increase the quantity of slag. On pp. 92 and 121: C below ·5 is termed wrought iron, and steel when C is between ·5 and 1·5; while on

p. 110, it is stated that, in puddling, the carbon is reduced to ·1 per cent.; and also that during the melting-down stage there is little chemical action. P. 111: Puddled bloom is chemically wrought iron with intermingled slag. P. 123: Steel is made by *carbonising* malleable iron. P. 128: Mild steel, not more than ·5 per cent. carbon, does not harden when heated and quenched in water. P. 134: The slags from the acid Bessemer process are *very basic silicates* of iron and manganese. P. 138: In Open Hearth process it is stated that the iron ore should be as free as possible from silica, whereas the Spanish hematite usually employed is very siliceous. P. 143: The strength increases as the diameter of the wire decreases. Also cake or tough copper may contain any amount of impurities. P. 223: An alloy of 80 per cent. copper and 20 per cent. zinc is called *red brass*. P. 229: Electro refining of copper; the anode is a thin sheet of copper; the cathode is a bar of blister copper. P. 230: The passage of the electric arc through the carbons produces a very high temperature. It is a great pity that a work which has been so judiciously compiled as the present one should be marred by so many mistakes, when by a more careful supervision of the proof sheets they might have been easily detected and corrected.

Annals of British Geology, 1893. A Digest of the Books and Papers published during the Year. By J. F. Blake, M.A., F.G.S. (London: Dulau and Co., 1895.)

ONCE more has Prof. Blake overcome all the obstacles of prolonged research among publications that are many of them difficult of access. Once more has he braved the disappointment of inadequate support, and following his own independent course in the selection and arrangement of his material, he has now given us the fourth volume of his "Annals of British Geology." Every addition to the series renders the whole of greater value, and we sincerely hope that the present volume will be self-supporting, as he ventures to anticipate. To all geologists, and to the provincial worker especially, these "Annals" must be of the greatest service, for the author contrives to give so much of the substance of each paper, that the reader will gain a very fair notion of the additions made to our knowledge during each year. Except in the student's special department of work, there will be no occasion to consult the originals.

Altogether 730 papers and books are noticed, being an increase of 180 over those recorded in the previous volume. The author's introductory review, occupying twenty-four pages, gives a summary of the chief geological news of the year. Although not essential to the Annals, this review acts as a safety-valve for the escape of some few of the critical remarks which arose while the author was perusing the 730 works. New forms of Ammonites and Corals come in for critical observations, so also do the "hemeræ" of the Inferior Oolite, and various glacial theories.

In Palæontology the place of honour is rightly given to the Elgin Reptiles described by Mr. E. T. Newton, and an excellent illustration of *Elginia mirabilis* forms the frontispiece of the book.

The Origins of Invention. By Otis T. Mason, A.M. Ph.D. Pp. 419. (London: Walter Scott, Limited, 1895.)

TO trace our modern industries to their origins, to show how they have evolved, and to point out the changes from naturalism to artificialism that mark the course of civilisation, is a difficult task, but an attractive one; and few ethnologists are better equipped with facts relating to this development than the Curator of the Department of Ethnology in the United States National Museum, who is the author of this book.

Dr. Mason lays down the following as the order in which kinetic energy has been commanded: (1) man-power in every pursuit; (2) fire as an agent in cooking, pottery, metallurgy, &c.; (3) the power of a spring, as in a bow or trap; (4) beast-power, for burden and traction; (5) wind-power, on sails and mills, and in draught; (6) water-power, as a conveyance and a motor, and gravity or weight generally; (7) steam-power, utilisation of an expanding gas; (8) chemical power, in the arts of the civilised; (9) electric power, motors, message-bearers, in mechanics and illumination; (10) light as a mechanical servant, only beginning to be domesticated.

Prominent among inventions are tools and mechanical devices—objects employed as means to ends. Many of these have come down from remote antiquity. Following M. Adrien de Mortillet's classification, Dr. Mason describes the tools and appliances used by primitive peoples for cutting; abrasion and smoothing; fracturing, crushing, pounding; perforating, grasping, and jointing. At the basis of tool-using, lie the systems of counting and weighing and measuring, all of which receive attention.

The invention and uses of fire, forms the subject of a very interesting chapter. Other matters treated in separate sections are stone-working, pottery, primitive uses of plants, the textile industry, inventions belonging to the chase, methods used for the capture and domestication of animals, means of travel and transportation, and instruments of warfare.

The work is readable throughout; it is a valuable history of the development of the inventive faculty, and has, therefore, an important relation to the history of humanity. The ethnologist will find in the volume much that is interesting in regard to the relationship between man's activities in different regions.

Short Studies in Nature Knowledge. By William Gee. Pp. 313. (London: Macmillan and Co., 1895.)

FOR boys in the upper standards of our elementary schools, this forms an ideal reading-book. It is simply worded, is not too full of details, contains numerous illustrations, and is likely to create and foster a love of natural knowledge. The book is intended to be used as an introduction to physiography, and it covers the ground usually understood to belong to that science. Copious extracts from the poetical and prose writings of standard authors are introduced into the text wherever possible, and serve to lighten it. The author appears to have spent a deal of care upon the work, and we think he has succeeded in producing a volume which will be welcome to teachers, as well as readable to all who find pleasure in the study of inanimate nature.

Organic Chemistry: The Fatty Compounds. By R. Lloyd Whiteley, F.I.C., F.C.S. Pp. 291. (London: Longmans, Green, and Co., 1895).

THIS is another elementary science manual "written specially to meet the requirements of the elementary stage of science subjects as laid down in the syllabus of the Directory of the Science and Art Department." It is hardly a book that we could recommend to followers of departmental organic chemistry, and certainly not one to be adopted by other students of the science. It is most unequal in structure, and very deficient in parts: Chapter iv., on percentage composition and empirical formulæ, consists of less than one and a half pages. As well-known standard works have been "freely employed" in the preparation of the volume, it is difficult to recognise the sections for which the author is responsible, and therefore undesirable to impeach the accuracy of some of the information.

LETTERS TO THE EDITOR.

The Age of the Earth.

I AM surprised to observe, in the article which Prof. Sollas has written on this subject in your issue of the 4th inst., p. 533, that he speaks with approval of Dr. A. R. Wallace's method of calculating the earth's age. About two years ago (I have only this week's number of NATURE at hand) I wrote to you on this subject, and was under the impression that I had proved the complete fallacy of Dr. Wallace's method of calculation.

To put Dr. Wallace's view briefly, he assumes that deposition within a limited area of, if I remember rightly, 3,000,000 square miles, goes on 19 times as fast as denudation over the whole land area, which is 19 times as great, and then argues that the whole maximum thickness of the stratified rocks (and hence the earth's age) could be deposited in 1/19 of the time required to carry away from an equal area of land an equal bulk of material.

The fallacy consists in assuming that a great rapidity of deposit over a limited area can in some way allow of the deposit or formation of sedimentary rocks at a greater rate than that of denudation.

It is obvious that, in a given time, no greater volume of deposits can be formed than the volume of material denuded in the same time. If, therefore, as Prof. Sollas assumes, 1/2400 of a foot of sediment per annum is denuded from the land area, by no arrangement can a land area of equal extent, consisting of sedimentary rocks of the same composition and thickness as those which actually constitute the land area, have been formed as a whole more rapidly than 1 foot thickness over 57,000,000 square miles area in 2400 years. Taking the estimate of Prof. Sollas, viz. 164,000 feet, as the maximum thickness of the sedimentary rocks, and taking the existing land area to be accounted for as 57,000,000 square miles, the time required to form an area of 57,000,000 square miles of rock 164,000 feet thick, at 1/2400 of a foot per annum, is 393,600,000 years, unless the area undergoing denudation was greater or less than it is at present (and it could not be four times as great as at present). No concentration of the deposit over a small area would shorten the time required by a single moment. BERNARD HOBSON.

IF, in the compass of a short article, I did not allude to the controversy which followed the attack made by Dr. Hobson (NATURE, vol. xlvii. p. 175, 226) on Dr. Wallace's method of estimating the age of the stratified series, it was because I thought, as I do still, that the honours of that controversy rested entirely on the side of Dr. Wallace.

There is no fallacy in Dr. Wallace's argument, but a strange misconception on the part of Dr. Hobson, which arises from his consistent disregard of the word *maximum* as prefixed to the estimated total thickness of stratified rocks. It is obvious that stratified systems cannot have a *maximum* thickness everywhere over the whole 57 million square miles of the land surface. As a matter of observation, a system attains its maximum thickness over a very limited area, and over a large part of the 57 millions of square miles of land surface it has no thickness at all, or, in other words, is entirely absent. If "maximum" could be made to mean the same as "average," no doubt Dr. Hobson's contention would hold, but those who have made use of a maximum in estimating the age of the stratified series have observed a strict distinction in the application of the two terms.

Rathgar, April 9. W. J. SOLLAS.

Polyembryony.

IN connection with the note in the last number of NATURE on the above, I think it should be known that the phenomenon was incidentally observed some two years ago in the red beet (*Beta rubra*) by the late Mr. Romanes and myself. We found that a single seed might produce as many as four distinct plants, and as far as our observations went, polyembryony was quite the normal condition. It seems to be more characteristic of the Gymnosperms than the Angiosperms, and has of course been investigated in the former, and in the latter among the Monocotyledons (Tretjakow) and Dicotyledons (*e.g. Citrus*-Strasburger). The fact of its occurring in such a common type as *B. rubra* should, I think, be taken advantage of by some botanist, as the results could not fail to be both interesting and important. Tretjakow's discovery that the supernumerary embryos in Monocotyledons may be produced by the antipodal cells, certainly suggests his comparison between such embryos and those produced by [parthenogenetic?] apogamy on the prothallia of the lower plants. FRANK J. COLE.

IMPROVEMENTS IN PHOTOMETRY.

NEARLY sixty years have passed since it first occurred to the philosophic mind of Sir John Herschel to attempt an arrangement of the relative brilliancy of the stars, upon a method that should be more secure than the eye estimations that had done duty for many centuries. It is not necessary to enter into any description of his method, which may be regarded now as entirely superseded. Doubtless, had he been surrounded by skilled workmen, furnished with better tools, the cumbrous method employed would have been simplified, but the establishment of an observatory remote from the assistance and contrivances of the workshop is not without drawbacks, as he and others since have discovered and regretted. About the same time, Seidel, in Germany, was at work on the same problem, and the fact that two astronomers, independently of each other, undertook the solution of the same problem, is a proof that it was ripe for mature consideration, while the series of astronomers who have laboured in the same path confirms the suspicion that this kind of investigation too long neglected offered a field having a rich prospect of reward.

But a photometer at once convenient and capable of general application to the stars remained to be invented, and this want was effectually supplied by Zöllner, who proposed a form of construction which has certainly obtained the most general use of any of the suggestions that have been from time to time put forward by astronomers, who have recognised its deficiencies and tried to remedy them. The distinguishing characteristics of the Zöllner photometer are the introduction of an artificial star formed from a lamp shining through a small aperture, and the controlling of the light of that star by means of polarisation. This principle is now of such general use that no lengthened description is necessary. But to explain the reason for the introduction of other forms of photometer, it is necessary to point out what are, or what were, considered to be its defects by those who first used the instrument, defects which it is believed care and experience have since done much to diminish, if not entirely to remove. A source of error might be anticipated in the varying brilliancy of the lamp employed to form the artificial star, and in the early days of the instrument this was a fruitful source of annoyance. Next, the light of the lamp had to strike no less than twenty-eight surfaces, and apart from the difficulty of getting so many surfaces true, and ensuring the parallelism of the Nicol prisms by which the diminution of the artificial star is effected, there is also to be considered the inevitable loss of light at so many surfaces. One consequence of this is that the brightest stars of the heavens are apt to be brighter than the artificial star, and since the observation is made by reducing this light to match that of the real star, it is necessary to have recourse to some such expedient as reducing the aperture of the telescope. And then a difficulty is encountered which has not yet met with a complete explanation. The light deducted from the star, as seen with a reduced aperture, does not coincide with that which would be predicted from theory. In some of the recent series of observations the differences between observation and theory are as great as they are perplexing. "There can be no doubt," wrote Mr. C. S. Peirce, of Harvard, twenty years ago, "that the errors introduced by the use of these diaphragms are by far the most serious of those by which my observations are effected." Dr. Wolff met with similar difficulties, and doubtless anomalies such as these have encouraged the production of other photometers which should be free from the suspicion of error. Having regard to the photometric work actually accomplished, we may confine attention to two forms of apparatus known as the Pickering Meridian Photometer and the Pritchard Wedge

Photometer. In the first of these the principle of polarisation is still used, but the artificial star is discarded. This is apparently an advantage, but it is a question if it does not introduce an error as large as that which it seeks to eliminate. An image of a star, as Polaris, is used as the constant of comparison, and this image can be reduced by polarisation till it equals that of the star of which the magnitude is sought. A lack of resemblance between the stars under consideration is removed, but the removal is effected by the introduction of a second object-glass with evidently different optical capacity, requiring a fresh constant to be determined. Prof. Pickering's photometer consists practically of two telescopes, placed at right angles to the meridian, and over each of the object-glasses is placed a right-angled prism. By means of the northern prism the image of Polaris is reflected, and by suitable adjustment can be made to occupy any convenient position in the field of view, while the prism on the other object-glass can be set to any declination so as to bring the image of any other star, when on or near the meridian, into juxtaposition with that of Polaris. Ingenious arrangements are introduced to ensure the coincidence of the pencils forming the images to be compared, and a control over the accuracy and efficiency of the whole is secured by contrasting the brilliancy of Polaris with itself—that is, by comparing the images formed by either object-glass. This is effected in all cases by rotating a Nicol prism in the eye-piece of the telescope through a measurable angle, and thus equalising the lights of the stars by means of varying the planes of polarisation. How effective this instrument has proved itself in the hands of Prof. Pickering, we shall presently see.

But either of these forms of photometer is necessarily a special production, and therefore the object-glasses are small and the light-gathering powers limited. In Prof. Pickering's first photometer, the aperture was only 4 c.m., with a magnifying power of only fifteen diameters. Prof. Pritchard, considering this limitation a defect, directed his attention to the construction of a photometer which should be readily available on any instrument, and be applicable to stars of very varying degrees of brightness. For this purpose he had recourse to the principle of extinction of the light of a star, by means of a wedge of neutral-tinted glass, which could be moved over the image of a star till its rays were lost by the gradual increasing thickness of the medium through which they had to penetrate. This principle had been used by the late Mr. Dawes, and also by Capt. Abney, but the long-continued use of such an apparatus by the late Savilian Professor is likely to connect his name with this form of photometer. The main defects in its construction arise from the difficulty of obtaining an absorbing medium equally operative throughout the entire length of the spectrum, and also that of determining with certainty the coefficient of absorption—in other words, how much light is lost by the difference of thickness corresponding to one inch in length of the wedge. Recent and more exact methods than those employed by Prof. Pritchard seem to show that the constant used in his work is in error, and that a correction to his magnitudes so obtained is necessary. But it is a peculiarity of the form of construction and method of observation adopted that such a correction can be very easily applied.

These forms of photometers, the Zöllner, the Pickering, and the Wedge, are the three instruments which have been most generally in use, and with which the modern work has been accomplished. The rapidity and the progress of this class of observation can easily be shown by a few statistics. Previous to their introduction, exact photometry was limited practically to two catalogues. Exact photometry is, of course, a relative term; it is meant to include observations other than eye estimations, and therefore Herschel and Seidel, the one with 69 stars, the other with 208, are the only two observers to whom it is necessary to refer. Since the introduction of the more rapid methods, and possibly from a better appreciation of the importance of the inquiry, we have had many extensive catalogues. Leaving out Zöllner himself, who did not attempt to condense his observations into catalogue form, we have—

Peirce's Harvard Catalogue of	494	stars.
Wolff's First Bonn Catalogue of	475	,,
Wolff's Second Catalogue of	923	,,
Potsdam Photometric Catalogue of	3522	,,

All these catalogues have been made by means of a Zöllner photometer, but the list in no way exhausts the photometric work that has been done by this instrument. For instance, Lindemann, at Pulkova, has carried out a long series of observations with the view of determining the scale that has been unintentionally adopted in the record of eye estimations in various catalogues. Ceraski and others have been at work on variable stars, while interesting inquiries into the extinction of light by the atmosphere and other physical investigations have been made by its aid. A debt of gratitude, therefore, of no common kind is due to the ingenuity of Dr. Zöllner. Confining our attention, however, solely to the compilation of catalogues, we have with the Pickering meridian photometer a collection of the relative magnitudes of 4260 stars, followed by a photometric revision of the Durchmusterung of Argelander, in which are given the magnitudes of some 17,000 stars. This leaves out of the summary a quantity of miscellaneous work on the Pleiades, on the Asteroids, on double stars, standard stars, &c. In fact, Prof. Pickering has placed it on record, that the number of measures made with the Nicol prism was up to 1890 slightly under half a million. Finally we have the more modest catalogue of Prof. Pritchard, embracing 2647 stars, and, to complete the record with this particular instrument, we must add a small item of some 45,000 extinctions made at Harvard under Prof. Pickering's direction. Of course, many stars are common to all the catalogues, but the record shows that in the last few years instrumental photometry has been applied to something like 30,000 stars. It is not easy to form an adequate conception of so much activity.

But if the numbers have increased in such welcome proportions, it may be asked is there an equally gratifying advance in the accuracy of the observations? This question is not so easily answered. Doubtless there is a much greater accord among the observations found in the same catalogue, and made by the same observer, employing the same instrumental means. But when these catalogues are compared with one another, large and provoking differences are sometimes encountered, and not a small portion of time has been given by various astronomers to the investigation of these differences, and the attempt to systematise the various recorded values of lustre. But when all has been done, there still remain individual differences which baffle explanation. They seem to point either to irregular variations of brilliancy in the stars themselves, or to baffling meteorological influences, which it is impossible entirely to eliminate. The suggestion has been made by others, and it is intended here to give it the fullest support, that a far larger number of stars exhibit variations of lustre than are included in our variable star catalogues. It must be remembered that these catalogues have been formed, and the variations detected, by the eye alone—that is to say, without the advantages of a photometer. Consequently it is only the larger variations that have attracted attention. It is not easy to establish the fact of an alteration in brilliancy, if it be small, either with or without a photometer; but it seems not unlikely that as star magnitudes gain in trustworthiness, a larger addition will be made to the stars recognised as variable. To come, however, to actual facts, it is recorded in the latest published catalogue of magnitudes, that of Potsdam, that the probable error of the concluded magnitude is 0·04 mag. This amounts to the same thing, practically, as deciding between the illuminating power of 25 and 26-candle gas. It is not known whether such a problem would offer any difficulty to gas experts, but even they sometimes fail to gain full credence from the public.

But the record of photometric research is by no means exhausted by this catalogue of work, limited to the application of specially devised photometers to the stars directly. Another and entirely different method of investigation has been actively prosecuted in the last few years, and apparently with the greatest success. This method avails itself of the refinements and the results of photography. Every one knows the appearance of a photographic plate on developing it after it has been exposed in the focal plane of a telescope for a longer or shorter time. It is seen that the circular images of the stars impressed differ greatly in size, and it may be in depth of deposit, according to the magnitude of the stars impinging on the plate. Consequently, by appropriate means of discussion we are able to determine the relative magnitudes of the stars themselves. And since we have here to contemplate the measurement of a sensible area, it may not be unwise to recall the fact that the term "magnitude" of a star is strictly limited to its brilliancy. Magnitude, therefore, in its accurate astronomical sense, is not easy of definition; difference of magnitude, involving as it does difference of brilliancy, is, however, easily apprehended, and it is a difference of magnitude that is sought to be determined by measurement of the blackened area corresponding to the star images on the sensitised film.

The problem here offered for solution is not precisely the same as that in the direct application of a photometer to the light of the stars. The eye ceases to be the actual photometer employed. For the impression on the retina we have substituted the impression recorded on the photographic film. This film may be more or less sensitive to some of the rays that go to make up the whole of the light of a star than is the ordinary retina, and consequently the record will differ in individual cases from that obtained by photometric means in the more ordinary sense of the word. Leaving out of the question orthochromatic plates, which are not usually employed in recording the positions of stars, the films are prepared so as to be most sensitive for the violet light of a star, whereas the eye is generally most sensitive to the yellow. If object-glasses are employed, this difference is usually aggravated again, for the optician seeks to make this coloured light most operative, according to the direction in which the telescope is to be employed. In the case of a photographic telescope, the rays about G in the spectrum are most important; in the visual telescope, those rays about D. In whatever way the photographic observations are discussed, with the view of ensuring a general agreement with photometric results, it must be anticipated that exceptional cases will differ, especially when the star light is rich in violet rays. Speaking generally, while a photometer, as usually employed, seeks to arrange the stars according to their appearance to a normal eye, a photographic determination of relative brilliancy exhibits the stars as they would appear to an eye most keenly sensitive to chemical rays.

The method of deriving the photographic magnitude will differ according to the manner in which the observations have been made. In the first place the ordinary plate, whether it be taken with the view of producing a general chart of the heavens, or the accurate representation of any small selected area on the sky, will contain implicitly the magnitudes of the stars impressed. Consequently, if we measure the diameter of the circular images produced by the stars of known magnitude, we have a relation between diameter and stellar magnitude. Such attempts end in the derivation of a convenient formula of interpolation. We may find that an expression of the form $m = a - bd$ or $m = a - b \log d$ (where m and d are respectively magnitude and diameter and a and b constants applicable only to that plate, and available only through a small range of magnitudes) is serviceable practically, but has no physical meaning. The determination of the constants a and b is troublesome, and demands a previous knowledge of the photometric magnitudes of some of the stars on the plate—information not always at hand. For these reasons attempts, more or less successful, have been made to assign the magnitude of a star from a knowledge of the diameter of the image and the duration of exposure. To be completely successful such an inquiry demands an acquaintance with the manner in which the image grows on the sensitised film, and this inquiry has progressed but slowly, and is still incomplete. In the early days of photography, it was supposed that the diameter varied as the square root of the time of exposure; later, with the modern dry plate, the fourth root of the time was thought by some to more nearly express the rate of growth; but Prof. Turner and the Astronomer Royal have both shown that neither of the suggestions is satisfactory. The character of the plate, the steadiness of the image, and the accuracy of "driving" (that is, the successful removal of the effects of the earth rotation), all enter as perplexing variables in a research of this character. The Astronomer Royal has suggested that the square root of the diameter of the photographic image increases as the logarithm of the time of exposure. This may be applicable to a particular telescope and through a definite range of magnitudes, but is scarcely likely to express a physical law. But, accepting such a result as a working hypothesis, we cannot pass directly to the magnitude of stars without making another assumption with regard to the diameters. This is usually summed up in the expression that for equal diameters--

$$\text{Exposure} \times \text{brightness} = \text{constant.}$$

That is to say, in order to get equal diameters of the images of two stars, one of which has four times the light of the other, we must expose the plate to the fainter star four times as long as to the brighter. This sounds almost axiomatic, and was for a long time accepted as a demonstrated fact. So much so, that at the Paris Conference in 1889 it was decided that the proper time of exposure to photograph eleventh magnitude stars was six and a quarter times that required for a ninth magnitude star. This decision of six and a quarter was adopted because this number expresses the ratio of the light in a ninth magnitude star to that in the eleventh. Probably no great harm will come from the adoption of such a resolution, but Captain Abney has given good reasons for doubting the assumption that length of exposure and intrinsic brightness are equally operative in producing the same photographic effect. All this goes to show that the determination of magnitude from an examination of the small circular dots on a "star plate" is not at all a simple problem. There is, too, another fact which should be borne in mind. All the discs are small, and yet in a range of five magnitudes, one hundred times more light has gone to make up the larger than the smaller of the two discs. This means that the scale is much contracted, and will probably interfere with final accuracy, quite as much as a want of definiteness at the edge of the disc, or distortion from a circular shape by being photographed at a distance from the optical axis, or other causes which make the measurement of the exact size of the blackened area, uncertain.

It is a question if the problem be materially simplified when the plates are photographed with the direct purpose of determining magnitude. We should then probably adopt a plan which has been extensively employed by Prof. Pickering, but so far has had few imitators. This consists in photographing the trail of a star. If we leave a phototelescope at rest with a plate exposed, the stars describe circular arcs on the plate having the pole as a centre, and having a length of 15° for each hour of exposure. The linear length will vary according as the star is near or remote from the equator, and since the energy is distributed over this varying length, polar stars will produce more intense trails than those stars of equal brightness near the equator. Effectively if two stars are found giving trails of equal density, the brightness of the two stars varies as the cosine of the declination. But if it be found that the stars near the equator travel, by reason of the earth's rotation, so rapidly across the plate that the fainter among them leave no trail, it is possible to give such a rate to the driving clock that the trail may be of any definite length, and the energy concentrated for a longer or shorter time over this space.

The method of deriving the stellar magnitude from an examination or measurement of these trails will be best understood by considering the case of the polar stars. A plate was exposed to the pole for ten minutes, and the telescope left stationary. The aperture was then reduced by successive amounts, so that theoretically any star would appear one magnitude fainter. In the case of a selected star, therefore, we have the thickness of the trail corresponding to known magnitudes, which could at once be compared with the trails formed on other plates. Actually these trails, corresponding, it is presumed, to stars of known magnitudes, were brought into juxtaposition with the trails of stars whose magnitude was sought, and the brilliancy was decided by equality of appearance. Of course similar practices could be and have been pursued when the stars are represented on the plate by means of circular discs. By varying the length of exposure in the photograph of a star of known magnitude, we can approximate to the appearance that stars of any magnitude would present for known durations of exposure. But here difficulties connected with the sensitiveness of the plate, and the meteorological circumstances of the night affecting the transparency of the atmosphere, have to be taken into the account, and the effects eliminated from the observation as carefully as possible, so that it is doubtful if a higher degree of precision results than in the case of photometric observation. There is, however, the obvious advantage that the photographs remain, and greater leisure and further experiment may suggest improved methods of observation and reduction, that shall ultimately give us all the accuracy needed in investigations of this character. The process as at present employed by Prof. Pickering appears to be fairly rapid. Three or four years ago he could report that he had applied his method to over 60,000 images, and the accuracy appears to be about as great as in the case of photometric observations. The chances of systematic error are probably greater.

NOTES.

AT Marlborough House, on Tuesday, in the presence of the Council of the Society of Arts, the Prince of Wales presented to Sir Joseph Lister, Bart., the Albert Medal accorded to him by the Society for "The discovery and establishment of the antiseptic method of treating wounds and injuries, by which not only has the art of surgery been greatly promoted and human life saved in all parts of the world, but extensive industries have also been created for the supply of materials required for carrying the treatment into effect."

PROF. CHRISTOPHER HEATH has been elected President of the Royal College of Surgeons, in the place of the late Mr. J. Whitaker Hulke, F.R.S.

DR. G. S. BUCHANAN has been appointed to the office of Medical Inspector of the Local Government Board.

THE Croonian Lectures at the Royal College of Physicians will be delivered by Dr. W. Marcet, F.R.S., on June 18, 20, 25, and 27, the subject being the "Respiration of Man."

THE grants lately made by the United States Congress, for the Geological Survey during the fiscal year 1895-96, amount to 515,000 dollars, or £103,000. This sum includes all field and office expenses and salaries.

LAST week, the colleagues and former pupils of Sir William Turner, Professor of Anatomy in the University of Edinburgh, presented him with his portrait, as a mark of appreciation of his services in the cause of science and to the University.

IN connection with the Goldsmiths' Company's grant for researches on the anti-toxin treatment, a Committee of the Royal College of Surgeons of England have recommended a grant of one hundred pounds to Dr. Sidney Martin, for the purpose of working out the action of the anti-toxic serum, when used to counteract the effects of various poisons separated by him from the membrane, and from the spleen, in cases of diphtheria.

WE have already noted that the London Chamber of Commerce are promoting a Bill for legalising the use of metric weights and measures for export trade purposes. In connection with this, the London County Council has just resolved to do all in its power to secure the passing of the Bill during the present Session. In the meantime the Council's inspectors will not interfere with the use of metric weights and measures in the execution of foreign orders.

AMONG the men of science who have died during the past week is Theodor Brorsen, whose name is so well known to astronomers. He was born in 1819, and was the discoverer of five comets, as well as the author of a number of writings on astronomical subjects. Since 1879, he lived in retirement at Norburg, his native place. Mr. J. H. Greener, the constructor of several early lines for telegraphic communication, and one of the most able of practical telegraph engineers, died on Sunday, in his sixty-sixth year.

THE President of the German Meteorological Society has issued invitations for a general meeting of the Society, to be held at Bremen on the 17th, 18th, and 19th inst., when various matters of interest to meteorologists will be discussed. The time of meeting has been fixed so as to fall in with the geographical conference, which will be held at the same place during Easter week, and in which oceanography and maritime meteorology form a prominent part. The subject of south polar investigation is also included in the geographical programme, so that it is anticipated that a large number of scientific men will take part in the proceedings.

THE spring meeting of the Iron and Steel Institute will be held in London on Thursday and Friday, May 9 and 10 next, in the rooms of the Society of Arts. The presidential address will be delivered by Mr. David Dale, and the Bessemer gold medal will be awarded to Mr. H. M. Howe, who will contribute a paper on "The Hardening of Steel." Mr. Stead, of Middlesbrough, will contribute a paper on "The Effect of Arsenic on Steel"; Mr. Sergius Kern, Metallurgist to the Russian Admiralty, will discuss the manufacture of armourpiercing projectiles in that country; Mr. Herbert Scott will communicate a paper on the iron ore mines of Elba; and Mr. W. J. Keep, of Detroit, will contribute a paper on "Tests of Cast Iron."

PROF. DR. G. SCHWEINFURTH, the distinguished African traveller and botanist, has passed the winter at Hélouan, near Cairo, where he is engaged in mapping and describing the adjacent mountains and "wadis," for a memoir to be communicated to the Institut Egyptien. Dr. Schweinfurth has just published the text of an interesting lecture on Erythræa—the Italian colony on the Red Sea—delivered before the Geographical Society of Berlin in July last year. This paper gives an account of a four-months' expedition which he made to Erythræa in the early spring of 1894, in company with Dr. Max Schoeller. It gives a most favourable report of the progress of the new Italian colony and of its future prospects.

A CORRESPONDENT has sent us particulars of an explosion which occurred at Providence, Rhode Island, U.S.A., on March 8, in connection with the electric light mains of that city. The explosion resembles those recently investigated by Major Cardew (see p. 539). Through a leak in the gas mains, the conduits became full of an explosive mixture, which suddenly exploded with great violence. The street was blown up for some thirty feet, all the glass in neighbouring houses wrecked, and missiles of wood and iron thrown to some distance. The noise and shock are said to have been felt all over the city.

THE circumstances connected with the formation of the dangerous alkaline deposits found by Major Cardew on the mains of the St. Pancras electric light supply, and the presence in this deposit in some cases of the alkaline metals in an unoxidised condition, have been inquired into by a Committee of the Royal Society and the Institution of Electrical Engineers, and the results have been issued in a report, which states that "the Committee are of opinion that the explosions which have occurred were caused by the firing of an explosive mixture of coal-gas and air by sparks caused by means of the above-mentioned incrustation. It has been proved that sparks may be caused either by the incrustation itself acting as an imperfect electrical conductor, or by moisture coming into contact with metallic sodium or potassium, both of which metals have been found to exist within the incrustation. These metals have been produced by the electrolytic decomposition of alkaline salts, chiefly derived from the soil and conveyed by moisture along the fibres of the wooden bearers towards the negative conductor. To avoid a repetition of these accidents, the bearers of the insulators at present in use should be replaced by other devices through which moisture is prevented from travelling, and it is recommended that the pattern of insulator in use should be changed, and a pattern adopted in which a longer insulating surface is interposed between the conductors and the bearer. The Committee are also of opinion that it is desirable that means should be provided by which the conduits can be inspected throughout their length, so far as is necessary to detect incrustations on the insulators. The Committee have not thought it within their province to investigate the causes of the presence of coal-gas within the electric lighting conduits, but it is obvious that this is the primary source of danger."

DR. DUPRÉ's report with reference to the recent explosion of a cylinder filled with compressed oxygen, at Fenchurch Street Station, was read at the coroner's inquiry last Thursday. An examination showed that the inner surface of the cylinder was fairly clean, but at the end on which the valve was, the surface was incrusted with magnetic oxide of iron, which was easily removable, and which under the microscope showed in many places the globular form assumed by the magnetic oxide produced by the burning of iron or steel. The lower end of the brass screw, by means of which the valve fittings were screwed into the bottle, was also incrusted with magnetic oxide of iron, much of which was in the form of small globules produced by fusion at very high temperature. They were evidently fused to the material of the screw, and it in some cases even slightly pitted the metal. These facts lead Dr. Dupré to conclude, first, that the bottle at the time of the accident contained an explosive gaseous mixture; and that, secondly, this mixture was fired by some portions of finely-divided iron, or perhaps grease, igniting in the compressed gas. That some iron had actually been on fire in the cylinder the condition of the screw sufficiently proved. The coroner, in summing up, spoke strongly in favour of a Government test being imposed with regard to the cylinders; and the jury included in their verdict the recommendation that all compressed gases of an explosive nature should be scheduled under the Explosives Act, that all cylinders should be tested by the Government periodically, that no cylinder should be allowed to be used or conveyed about unless bearing the Government stamp, that all manufacturers should be licensed by the Board of Trade in the future, and that separate hydraulic pumps should be used in the apparatus in filling the cylinders, and also recommended a Board of Trade and railway inquiry.

AT the Institution of Civil Engineers, on April 2, Mr. J. I. Thornycroft, F.R.S., and Mr. S. W. Barnaby, in a joint paper, expressed the belief that the speed of vessels had now approached within measurable distance of that at which propulsion by screws became inefficient. For a given pitch-ratio and slip, the thrust per unit of area varies as the square of the speed. Certain conditions of pressure on the screw-blades cause the formation of cavities, filled with air and vapour driven off from the water, behind the blades. Experiments carried out by the authors, with screw-blades of elliptical form, show that this "cavitation" does not commence suddenly, but appears to become detrimental when the mean negative pressure on the forward side of the blades exceeds about 6¾ lbs. per square inch, or when the whole thrust exceeds 11¼ lbs. per square inch. Cavitation can only be avoided at very high speed by increasing either the immersion of the screw or its blade-area. Immersion is limited by considerations of draught. Increased area, the authors remarked, can be obtained in three ways: (1) by increasing the ratio of surface to disc-area, (2) by employing a larger diameter than that theoretically best for the given conditions, (3) by increasing the pitch-ratio, which involves a larger diameter with a reduced rate of revolution. Either tends to a waste of power if pursued beyond somewhat narrow limits, and it appears inevitable that reduced efficiency must be submitted to as the speed of vessels is increased.

A FEW noteworthy points were brought out last week in evidence before the Select Committee of the House of Commons appointed to consider the advisability of adopting the metrical system of weights and measures. Captain H. R. Sankey, R.E., director of the engineering firm of Willans and Robinson, Thames Ditton, said that the metric system of measurement had been adopted by his firm for the last two years. The reason of the change was the advantage of working interchangeably with manufacturing engineers on the continent. His firm's trade was mainly British, but no objection had been made to metrical measurements by British customers. The workmen were agreed that the metric system was much more easy to work with than the English measures, and was much less liable to error. The inconvenience of the change to the metric system was only felt by the workmen for a few days. In his evidence, Dr. Gladstone estimated that in the elementary schools about 350 hours were occupied in teaching our cumbrous system of weights and measures. As the children had to learn decimals, very little more time would be needed by them for learning the metric system. A chart explaining the metric system was now to be found in nearly all the London schools, and lately it had been decided to supply actual examples of the metre and litre to such schools as asked for them. The metric system was but partially taught in the schools, for the teaching involved additional labour, and was not insisted on by the inspectors. The introduction of the metric system would be much facilitated if the system were compulsory in the elementary schools.

In a paper read before a recent meeting of the Académie des Sciences (*Comptes rendus*, cxx. p. 611), M. Lucien Poincaré described some experiments on secondary batteries he has been performing. With a view of reducing the losses that take place in the ordinary lead accumulator, the author has tried to obtain a battery with *liquid* metallic electrodes. For this purpose mercury has been employed, and as it would not do to employ an acid as the electrolyte, since the hydrogen would be liberated and thus cause a waste of energy, a solution of a salt is used. Under these circumstances an amalgam is formed at the cathode, which, together with the mercury of the anode, forms a secondary cell. Of all the different salts tried, the most interesting results have been obtained with the haloid salts of the alkaline metals. With these salts the electromotive force of the secondary battery is about two volts, but in the case of the chlorides and bromides, the chlorine or bromine combines with the positive mercury, forming a badly-conducting layer, so that the output of the cells is not satisfactory. With a solution of iodide of sodium, however, very satisfactory results are obtained, for as long as the solution is kept sufficiently concentrated, the current density during the charging is not too great, and the positive electrode has a larger surface than the negative electrode, no deposit is formed on the anode. Hence by this arrangement a secondary cell is obtained in which the two electrodes remain, after the charge, entirely metallic, so that the internal resistance does not increase as the cell is charged. The efficiency of these cells amounts to very nearly 90 per cent., the electromotive force, when fully charged, being 1·85 volts. The capacity per kilogram does not differ much from that obtained with ordinary lead accumulators, being about 10 ampere-hours. A very important point about this form of cell is that the density of the *discharge* current is immaterial, and they may be completely discharged without in any way deteriorating.

We have received from Prof. G. Hellmann an interesting pamphlet entitled "Contribution to the Bibliography of Meteorology and Terrestrial Magnetism in the Fifteenth, Sixteenth, and Seventeenth Centuries." The work was prepared for the Report of the Chicago Meteorological Congress, and contains a list and brief bibliographical notes of 272 old books in Dr. Hellmann's library, arranged under the authors' names, with cross references under subjects and dates. This pamphlet is of considerable value to meteorologists, as the early literature of this subject is as yet little known, and no meteorological institute is as rich in the older literature as the library in question. Arranging the books according to the language in which they were written, Dr. Hellmann shows that the authors comparatively seldom employed their native language, as 157 of the works are in Latin. There is also a preponderance of Italian works, as in the seventeenth century Italy probably contributed more to meteorology than all the other nations of Europe. Many of the works described are exceedingly scarce.

Since the great Japanese earthquake of 1855, the strongest shock felt in Tokyo was that of last June 24. No house was absolutely destroyed, but in the lower parts of the city many brick buildings were damaged and chimneys thrown down. The total land-area disturbed was 42,000 square miles. The diagram of the earthquake, taken by a large-motion seismograph at Tokyo, is given by Messrs. Sekiya and Omori in a short but valuable paper (*Journ. of the Coll. of Science*, Imp. Univ., Tokyo, Japan, vol. vii. part v. 1894). This diagram being on the natural scale, the preliminary tremors are not shown, and during the first two seconds of the record the motion was already strong. It then became suddenly violent, the ground moving 37 mm., followed by a counter movement of 73 mm., the maximum horizontal displacement during the earthquake. At about the same time, the maximum vertical motion of 10 mm. occurred. The movement, which lasted altogether 4½ minutes, soon became comparatively weak, and it is no doubt to the small number of violent oscillations that the slight amount of damage is due. As usual, the direction of motion changed during the earthquake, but the maximum horizontal motion was directed towards S. 70° W., and this is identical with the mean direction of overturning of 245 stone lanterns in different parts of Tokyo.

The Natural History Society of Queensland, established in the beginning of 1892, has progressed so well that it is able to issue a volume of *Transactions*. The volume should prove to be a useful contribution to the literature on the natural history of the colony.

The fifteenth volume of observations of "Rainfall in the East Indian Archipelago," published by the Government of Netherlands, India, has been received. The volume is edited by Mr. Van der Stok, the director of the observatory at Batavia, and it comprises observations made at 194 stations, of which 104 are in Java and Madoera, and ninety in Sumatra and the different islands of the Eastern Archipelago.

In the *Geological Magazine* for April appears a translation of a Swedish paper by Prof. Lindström, on the discovery of *Cyathaspis* in the Silurian formation of Gotland. In the cutting of canals for the drainage of marshy tracts of the island, many good exposures have been made of the fossiliferous marl-shales that underlie the famous limestones with the operculate coral *Rhizophyllum*. Among the fossils obtained from these shales were a pair of dorsal shields of a Pteraspidian fish. Hitherto the oldest known indubitable fish-remains date from the Ludlow epoch, but the Gotland beds are of Wenlock age. It is pointed out that since the vertebrate character of the Cambrian conodonts has been disproved, and the supposed Ordovician fish-remains of North America are very doubtfully of that age, these Gotland fossils are, for the present, the oldest known Vertebrates.

First among the new editions received within the past few days is Prof. James Dana's "Manual of Geology," published by the American Book Company. The appearance of this fourth edition of Prof. Dana's classical work, makes us marvel at the energy of the eminent author who, though now eighty-three years of age, could rewrite the whole of the matter in such a ponderous volume as the "Manual," and add one hundred and fifty pages to what was published in the third edition. It is hardly necessary to say that a multitude of new principles, new theories, and new facts, in all branches of geology, have been included in the new edition. "The Partition of Africa" (Stanford), by Mr. J. Scott Keltie, has reached a second edition, after two years of life. The many events that have taken place in Africa during this time are all given consideration. Another volume which has attained the eminence of a second edition is "Outlines of Zoology," by Mr. J. A. Thomson, published by Mr. Y. J. Pentland. The book has evidently been appreciated by students of zoology as a manual for use in the lecture-room, museum, and laboratory. Messrs. W. H. Allen and Co. have published a third and revised edition of "Practical Microscopy," by Mr. G. E. Davis.

THE lower sulphide of carbon, CS, appears to have been obtained by Dr. Deninger, of the Dresden Laboratory, in considerable quantities. Anhydrous sodium sulphide and excess of chloroform were heated in sealed tubes, from which the air had previously been removed, to a temperature about 180°. Upon opening the tubes after cooling a large volume of gas was discharged, which consisted of sulphuretted hydrogen, a small quantity of hydrogen chloride, and a new gas, which was practically unaffected by passage through a solution of caustic soda. This gas was combustible, burning with production of sulphur dioxide, and would appear to be carbon monosulphide, produced in accordance with the following equation :

$$3Na_2S + 2CHCl_3 = H_2S + 6NaCl + 2CS.$$

The same gas is obtained, less admixed with impurity, by heating in sealed tubes a mixture of silver sulphide and iodoform, and subsequently allowing the product, consisting of carbon monosulphide and sulphuretted hydrogen, to bubble through caustic soda solution, whereby the latter gas is absorbed.

$$3Ag_2S + 2CHI_3 = H_2S + 6AgI + 2CS.$$

An analysis was made of the gas thus obtained, and the result agreed with the formula CS. The gas explodes violently in the eudiometer, so that analysis is not readily carried out volumetrically ; the explosion pipette was destroyed in a second attempt. The action of sodium upon carbon disulphide was next studied. When the metal is placed in the liquid disulphide it becomes coated with a greyish substance, which prevents further action. It was found, however, that aniline dissolves this coating, leaving the metal clean ; when carbon disulphide is then added, a continuous evolution of gas occurs, but the greater portion of the gas does not escape, being energetically absorbed by aniline, unless the sodium is maintained at the surface of the liquid. After purification of the gas by passage through caustic soda, and through triethyl phosphine to remove carbon disulphide, it is found to burn with a blue flame, with production of sulphur dioxide and water. The latter is owing to admixture of free hydrogen, which can readily be isolated by absorption of the carbon monosulphide in aniline in a Hempel burette. Carbon monosulphide is also rapidly absorbed by alcohol. Upon allowing the mixture of hydrogen and carbon monosulphide to stream through tubes immersed in a freezing mixture, the latter gas condensed to a clear colourless liquid, which rapidly evaporated upon removal from the freezing mixture. It would thus appear that carbon monosulphide is a gaseous substance at the ordinary temperature, but which readily condenses to a liquid in an ordinary freezing mixture, is combustible, and is energetically absorbed by alcohol and aniline.

THE additions to the Zoological Society's Gardens during the past week include a Feline Douracouli (*Nyctipithecus vociferans*) from South Brazil, a Squirrel Monkey (*Chrysothrix sciurea*) from Guiana, presented by Mr. Augusto Lewy ; a Ring-necked Parrakeet (*Palæornis torquatus*) from India, presented by Lady Aitchison ; two Hybrid Widgeon (between *Mareca penelope*, ♂, and *M. chilœnsis*, ♀), bred in England, presented by Mr. J. Charlton Parr ; seven Common Skinks (*Scincus officinalis*) from the Sahara Desert, presented by Major Sullivan ; a Haast's Apteryx (*Apteryx haasti*), an Auckland Island Duck (*Nesonetta aucklandica*) from New Zealand, nine Hamadryads (*Ophiophagus elaps*) from India, deposited.

OUR ASTRONOMICAL COLUMN.

THE LYRID METEORS.—Mr. Denning draws attention to the fact that this year the Lyrid shower may be observed practically in the absence of the moon. Though rarely forming a striking shower, the stream is astronomically important for the reason that it is probably the only one, besides the Perseids, which has its radiant displaced on successive nights of observation. The duration of the shower is from April 16 to 23, with a maximum on the 20th, and the displacement of the radiant is comparatively small. On April 20, the radiant is situated in R.A. 270°, Decl. + 33°, that is, about 1° N. of 104 Herculis, a star of the 5th magnitude. Like the Perseids of August, the Lyrids never fail to show themselves, though they are rarely to be compared with the Perseids in point of richness (*Observatory*, April). The orbit of the swarm is probably identical with that of Comet I, 1861.

A NEW FORM OF ZENITH TELESCOPE.—The determination of the zenith distances of stars near the zenith appears likely to be greatly simplified by an arrangement recently devised by M. Louis Fabry (*Bulletin Astronomique*, April). The main idea is to materialise the zenith point so that micrometric measurements can be made directly in the field of view. A horizontal telescope is fixed in the meridian, and in front of it is a thin sheet of glass inclined at 45° ; beneath this is a basin of mercury. When adjusted so that the reflected image of the cross-wires is coincident with the wires themselves, the wires mark the zenith point, and stars of sufficient brightness will also be seen in the field of view after transmission through the glass and subsequent reflection from the mercury and glass. The great objection to this simple plan is the reduction of brightness of the star images, and there is also an objection to observing stars after reflection from mercury. To overcome these difficulties, a second telescope is directed towards the first, so that the cross-wires can be seen at the centre of the field, and the sheet of glass is coated with silver to a thickness that will just allow the strongly illuminated cross-wires of the first telescope to be seen in the second. The first telescope thus serves the purpose of marking the zenith point, and this is utilised in the second telescope for the measurement of distances.

As compared with the zenith telescopes in common use, the new instrument has the advantage of greater simplicity and rigidity, and it is unnecessary to make differential measures of stars north and south of the zenith. It should especially be of use in such an investigation as that of the variation of latitude.

THE ORION NEBULA.—An exhaustive discussion of the photographs of this nebula, and of the stars in its vicinity, taken at Harvard and elsewhere, has been recently completed by Prof. W. H. Pickering (*Annals of the Harvard College Observatory*, vol. xxxii. part 1.) 146 stars have been catalogued, in addition to those given by Bond for the same area, and a comparison of magnitudes suggests that a few of the stars are either variable or have increased in brightness. No clear indications of change in the nebula, either of shape, position, or brilliancy, have been detected in the photographs taken during the last ten years. By the ingenious arrangement of photographing the nebula through a thin perforated sheet of brass placed in contact with the sensitive film, Prof. Pickering has constructed a chart of the nebula showing isophotal contours, or lines of equal photographic intensity, which will be very valuable in subsequent searching for evidence of change. A photograph of the spectrum of the nebula taken with the objective prism shows at a glance which regions shine with light of any particular wave length. The image corresponding to the hydrogen line $H\gamma$ is seen to resemble most closely the ordinary photographs, while the ultra-violet line at λ 372 is found particularly strong along the south-east border of the Huyghenian region. Among the photographs reproduced, that showing the vast nebulosity which nearly surrounds the whole constellation of Orion is, perhaps, of the greatest interest. This was taken with a portrait lens of about 2½ inches aperture, and shows the nebulous stream, about 15 degrees in diameter, which has since been photographed by Prof. Barnard (NATURE, vol. li. p. 253). Prof. Pickering gives reasons for supposing that the parallax of this remarkable nebula is not greater than 0″·003, corresponding to a distance of 1000 light-years. A useful account of the fundamental principles and processes of astronomical photography, including the determination of photographic stellar magnitudes, is given in the same volume of the *Annals*.

THE SUN'S PLACE IN NATURE.[1]

III.

THE next question that we have now to consider has to do with the connection between nebulæ and stars, and I shall show that the more the facts are studied the closer does this connection prove to be. You remember that that was the idea which lay at the bottom of the hypotheses both of Kant and of Laplace. In the last lecture I referred to some of the earliest observations which had been made of the nebulæ by means of the spectroscope, and it so happened that Dr. Huggins, to whom we owe this work, came to the conclusion that the result of his inquiries was rather to show that this connection, which had been asserted both by Kant and Laplace, and which had been accepted by everybody up to then, really did not exist. In a paper which detailed these spectroscopic observations, published in 1865, Dr. Huggins stated his conclusion that the nebulæ, instead of having anything whatever to do with any evolutionary line along which both nebulæ and stars might be traced, possessed a structure and a purpose in relation to the universe altogether distinct and of another order. So that you see the first apparent teaching which we got from the spectroscope practically put us in a very considerable difficulty; if it had to be accepted, of course the views of Kant and Laplace would have to be rejected.

When I commenced my general survey in 1887, this view held the field, and further, it was imagined that the observations of Dr. Huggins justified the idea that the nebulæ were masses of gas; what particular gas will concern us a little later on, but for the moment I need only say that these statements announcing the nebulæ to consist of one or two gases, at once led to several most remarkable views of the general constitution of the heavens.

Near the end of this nineteenth century chemists claim to know something of the materials which have built up the planet on which we dwell, and if you consult any of the books which have been written about spectrum analysis, giving the results of the work during the last thirty years or so, you will find it stated over and over again that the spectroscope has put it for ever beyond doubt that the chemistry of the skies, *i.e.* the chemistry of the various bodies which people space, and which are at a sufficiently high temperature to enable us to examine them spectroscopically, exactly resembles the chemistry of the earth. So that, if this were true, we should have a common chemistry of the earth, of the stars, and among the stars of course our own sun. On the other hand, we should have, according to Dr. Huggins, absolutely and completely distinct from these bodies another class, the nebulæ, in which the chemistry is absolutely and completely unique. This was so clearly the idea suggested to philosophical students of these questions, that Dr. Wolf, a famous French astronomer, who has written an all-important book for those who are interested in these inquiries, "Les hypotheses cosmogoniques," published in 1886, writes: "If we admit the data of spectrum analysis as to the gaseous state of these singular bodies, the nebulæ, and the simplicity of their composition, one is led to see in them only the residuum of the primitive matter after condensation into suns and into planets has extracted the greater part of the simple elements which we find on the earth and chemically in some of the stars."

It was perfectly clear then to Dr. Wolf that, if the constitution of the nebulæ was anything like what was supposed to have been revealed by early spectroscopic observation, we were dealing with a residuum. There was one kind of action in space, bringing together one class of elements with which we are familiar here, and forming them into stars, suns, and planets; but there was another kind of matter which declined to form part of these aggregations, which remained by itself, and finally put on the appearance of the so called nebulæ.

The first thing I have to say concerning this view is, that the discussion of the spectroscopic observations which I told you, in the last lecture, had been undertaken with a view of seeing what really could be determined in relation to this question, showed, beyond all question, that there was no ground whatever for it; that there was no real ground for supposing that there was this enormous difference between the nebulæ and the stars. In the year 1887, the year following the course of lectures to which I have already referred, after testing the views on this question by an appeal to all the available observations, I stated that the facts taken in all their generality showed that the nebulæ simply represent early stages of evolution; that is to say, that we have a continuous and orderly progression from the nebulæ to the oldest star, and that the nebulæ represent the first stage, and the oldest star or planet represents the last. It seemed to be perfectly clear from the discussion that we were justified in stating that every nebula which is visible now will some time or other, owing to the condensation of its various parts, become a star of some order or another; and that it is equally true to say that every star which we see now in the heavens, knowing it to be a star, has really been a nebula at some time or another.

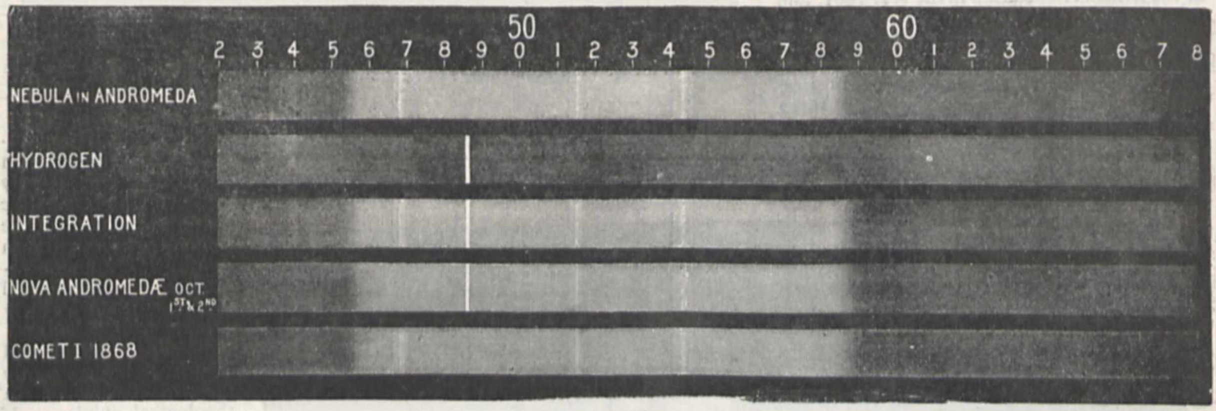

FIG. 12.—Spectrum of the nebula in Andromeda compared with Nova Andromedæ and comet. The flutings common to all are those of carbon.

I told you that the first suggestion of a possible condition which would enable an evolution to take place from nebulæ to stars had been made by Prof. Tait, when he thought that probably cool meteoritic particles might have something to do with it. The complete inquiry shows that these meteoritic particles might account equally well both for the luminosity of comets and of nebulæ. This association is important because it is generally conceded that comets are swarms of meteorites.

It seemed so obvious that there was this close connection that in 1888 I predicted that, if the nebulæ were carefully observed, we should find in them sooner or later indications of that same substance which makes the comet's spectrum so very distinct and special. In almost every comet which has been observed, the spectrum of carbon, or of some compound of carbon, is the strongest and most obvious feature which is presented to us. In 1889, *i.e.* only the next year, matters were made very much clearer by the discovery, by Mr. Fowler and Mr. Taylor, of the spectrum of carbon in the nebula of Andromeda (Fig. 12), so that there, you see, was a prediction verified, and such verification is always a very precious

[1] Revised from shorthand notes of a course of Lectures to Working Men at the Museum of Practical Geology during November and December, 1894. (Continued from page 393.)

test, since it helps us to know whether one is going right or wrong, and it seemed to strengthen very much the view under consideration. Further, not only do we find carbon both in comets and nebulæ but it is recognised by everybody that in some stars the same substance exists in enormous quantities. Here, then, we are in the presence of the fact that the statement that there is an enormous chemical difference in structure between nebulæ and stars is shown spectroscopically to be unfounded, while the evidence also goes to show that there is a close connection between nebulæ and comets. By this, of course, the argument is very much strengthened all round, because, as we have seen, nearly everybody agrees that comets most probably consist of meteoritic stones or particles.

I am glad to say that among the first to accept the new evidence proving that nebulæ are really early stages of evolution of stars was Dr. Huggins himself, the observer whose statement which I have quoted I had been fighting for years. That you see was a great victory. He says now not only that these bodies may represent early forms; places them in the line of evolution where I had placed them, but he even adduces the same evidence which I had brought forward in several of the arguments which I had employed. Dr. Huggins made a reference to this question as President of the British Association in the year 1891, and if any of you read that you will see that it is really an argument in favour of the views that I have been insisting upon since 1886, and his agreement seems all the more important since Dr. Huggins appears to have arrived at these conclusions quite independently. Not one word is said throughout the address of any arguments which I may have used, or of any line of thought or observation on which I had founded the various statements which I had made; and therefore it would be charitable to suppose that he was unacquainted with my work when that address was given to the world. Of course you will acknowledge that that was a very extraordinary change of opinion, so extraordinary indeed that it is clear that Dr. Huggins felt that it was of importance to himself that the change should be explained; and he confesses in the address, to which I refer, that the communication he made to the Royal Society in 1864 was not entirely founded on scientific evidence, but partly made under, to use his own words, "the undue influence of theological opinions then widely prevalent."

Fig. 13.—The nebula near 52 Cygni, from a photograph by Dr. Roberts.

So after all I had been fighting partly an expression of a theological opinion. If we had known that before, probably some trouble might have been saved.

It is a very important thing to know that now, from east to west, those who dwell upon this planet are all perfectly convinced that nebulæ are early forms in the evolution of the heavenly bodies. The more one knows of the history of human thought, especially during the last two centuries, the more important does it seem that that result should be acknowledged as one of the most important truths established during the present century.

Before I go further, let me refer to two or three typical examples of these strange bodies, as I can do by the kindness of Dr. Roberts, whose method of work I described in the last lecture.

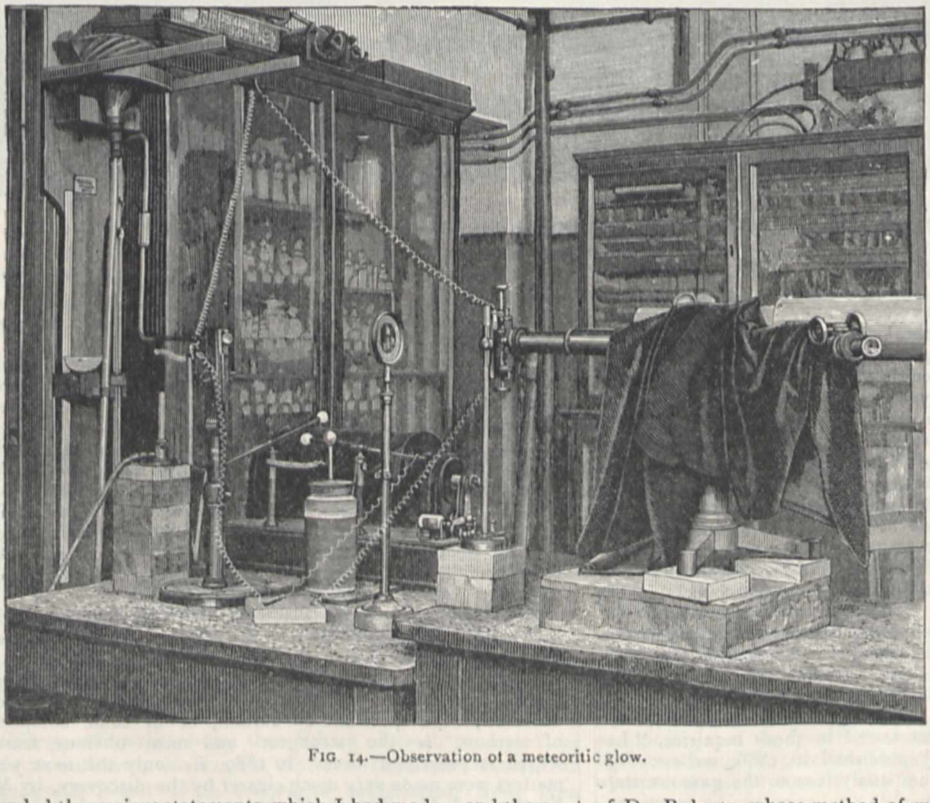

Fig. 14.—Observation of a meteoritic glow.

First of all we have a representation of a form of nebulæ which is not very common, but the study of which is all important for our present purpose. It is called a spiral nebula, it is one of the nebulæ in the constellation of the Great Bear. I wish to point out that in the centre we have a condensation, and from the centre of the condensation the luminosity gradually gets less and less until at last we have no luminosity greater than that of the surrounding sky. In the nebula itself we find exquisite spirals, starting apparently from different points, and gradually coming towards the centre, and if you look along these spirals you will see that the star-like masses, which may not be stars, are in many cases located on the spirals, representing apparently minor condensations, each spiral itself being probably brighter than the other parts because it is more disturbed.

Next we have an absolutely untouched photograph of the famous Dumb-bell Nebula. I am certain that many here have studied the drawings of this nebula given in encyclopædias and in books of astronomy during the last forty years, and that it is a great comfort to you to see, as it is to me to be able to show you, the autobiographical account that it gives of itself, because if you refer to those drawings it will be very difficult to find any two alike, even if it is distinctly stated that one has been copied from the other. In this again we find a central condensation, and associated with it arcs in which the luminosity is greater than in the adjacent regions.

The other nebula that I have to exhibit is one remarkable for its difference from the other two, inasmuch as no condensation is suggested. This nebula, which you see stretching across the screen like a sort of celestial river (Fig. 13), seems to be careering through space, and I call your attention specially to this because it is well to remember that, if we have meteoritic swarms in space, *i.e.* swarms which are condensing, it is quite possible that we may have meteoritic streams. I think you will consider that it is not any misuse of words to say that we have there a possibility of a meteoritic *stream*.

So much then for some typical representations of some of the different forms of nebulæ.

While, however, Dr. Huggins in his presidential address, apparently from quite independent inquiry, announced my main contention, viz. that nebulæ and stars *do* belong to the same order of celestial bodies, and withdrew his unfortunate statement as having been made on theological grounds, I am compelled to say, but wish to say it with the utmost courtesy, that a complete study of the literature shows that he was quite familiar with my work all the time, and that while he thought fit to republish my main contention as his own on the one hand, on the other he was engaged in attempting to throw discredit on my work, and to conceal his retreat after the manner of the sepia by a

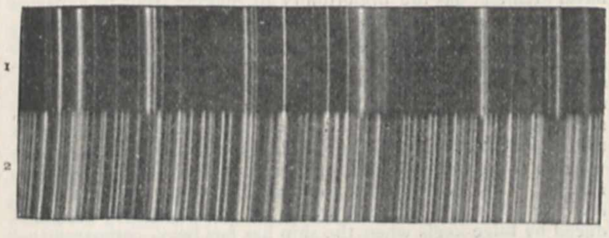

FIG. 15.—Line-spectra of barium and iron. 1, Barium. 2, Iron.

great cloud of ink—printer's ink, referring to a minor point. He endeavoured to suggest to anybody who was not really completely acquainted with my work, that the methods employed by my assistants and myself for something like three years were inaccurate, and that the conclusion reached, which must have been right because he had come to agree with it, had been got at in the wrong way. Although the charges of inaccuracy which Dr. Huggins thought fit to make were general, in his printed papers the chief stress has been laid upon a statement I had made with regard to a matter of secondary importance in the general discussion, I refer to the possible origin of the chief line in the nebular spectrum.

I propose to go into this matter in some detail, because it will enable me to indicate the closeness with which the skill of trained observers and the magnificent instruments of modern research that I have already referred to, enable us now to deal with facts, and to replace the imperfect observations of the past with others of which the accuracy may be relied upon.

Among the many lines of evidence which had been brought forward, it was stated by myself that in following up the suggestion of Prof. Tait and experimenting upon meteoritic dust, a line had been seen very near the position of the chief line which Dr. Huggins had discovered in the year 1864 in the spectrum of the nebulæ. In fact, after accumulating all the spectroscopic

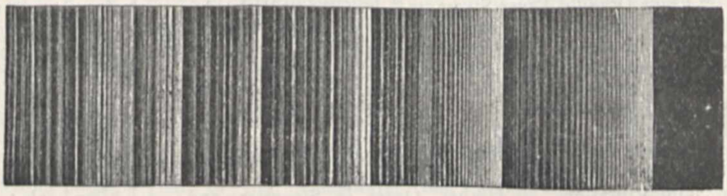

FIG. 16.—Flutings of carbon.

observations which I could lay my hands on, I went on to experiment myself, and I have here some apparatus which will give you an idea of the kind of experiment which was undertaken (Fig. 14.) Meteoritic dust is placed in a horizontal spectrum tube connected with a Sprengel air-pump, so that an electric spark can be passed through the dust; an image is formed on the slit of the spectroscope, and an arrangement for an ordinary electric spark in air serves for a comparison spectrum. The point was to see whether there was any probability that Prof. Tait's suggestion was right, by examining the spectrum of meteoritic dust. For this purpose some dust was placed in a tube resembling the one now on the table, and an electric current was sent through it. Now it had long been known that when one heats meteoritic dust, it gives out compounds of carbon, and also hydrogen gas; what I did was to observe the change in the spectrum of that tube under different conditions. For instance, if it were wished to expose the dust to a higher temperature, a Bunsen burner was placed underneath it.

You will be able to see that, in a little time, the heat will make a considerable difference in the phenomenon observed in the region which comes under its influence, and I think you will also see that in some parts we get a distinct indication of green colour. Now what I found was that in the spectrum of dust from several meteorites so examined, there was a line very near the position which had been stated by Dr. Huggins to represent the actual position of the chief line seen in the spectrum of the nebulæ. That line I was able to trace home by comparative work to olivine, a substance which occurs in almost all meteorites, even in iron meteorites. and not only to olivine, but to one of the constituents of it, which is magnesium. I have here a diagram of some of the results obtained in the green part of the spectrum, and it will be seen that we get in the nebula of Orion, and in the comets of 1866 and 1886, a bright line apparently in the same position. When I say line I should correct myself, this luminosity given out by magnesium does not take the form of a line as ordinarily so called. I will throw on the screen photographs of two spectra of the vapours of two metallic substances, barium and iron (Fig. 15), and you will then see what is meant by men of science when they talk about a line spectrum.

But besides what we call line spectra, there is another thoroughly well-recognised class, which we call fluted spectra, because it reminds one of the flutings of a column. Here, for instance, is a fluting of carbon (Fig. 16). In these flutings, instead of the lines being distributed irregularly, as in the case of iron and barium, we get a beautiful rhythm from one part where the light rapidly degrades to another where there is an enhancement of the light, followed by another degradation, and so on. Indeed, we not only get main flutings, but we get subsidiary flutings.

J. NORMAN LOCKYER.

(*To be continued.*)

THE INSTITUTION OF NAVAL ARCHITECTS.

THE annual spring meeting of the Institution of Naval Architects was held on Wednesday, Thursday, and Friday of last week, in the theatre of the Society of Arts. There was a long list of papers set down for reading and discussion as follows:—

"Notes on further Experience with first-class Battle-ships," by Sir William White, Director of Naval Construction.

"The Elements of Force in a Warship," by Vice-Admiral P. H. Colomb.

"On Steam Pipes," by J. T. Milton, Chief Engineer Surveyor, Lloyds' Registry of Shipping.

"Light Draught River Steamers," by George Rickard.

"On solid Stream Forms and the Depth of Water necessary to avoid abnormal Resistance of Ships," by D. W. Taylor, Senior Assistant to Chief Constructor of U.S. Navy.

"On the Method of initial Condensation and Heat Waste in Steam Engine Cylinders," by Prof. R. H. Thurston, Sibley College, Cornell University, New York.

"Description of an Aluminium Torpedo Boat built for the French Government," by A. F. Yarrow.

"On Vibrations of Higher Order in Steamers, and on Torsional Vibrations," by Otto Schlick.

"On the Vibrations of Ships and Engines," by W. Mallock.

"On a Method of Extinguishing Vibrations in Marine Engines," by Mark Robinson and H. Riall Sankey, Captain (retired) R.E.

"On the Transverse Stability of Floating Vessels containing Liquids, with special Reference to Ships carrying Oil in Bulk," by W. Hök.

"Induced Draught as a Means for developing the Power of Marine Boilers," by W. A. Martin.

After the usual formal proceedings had been transacted, and the President, Lord Brassey, had given a brief address, Sir William White's paper was read by his colleague at the Admiralty, Mr. W. E. Smith, the author being absent owing to serious illness; from which, every one will be glad to hear, he appears in a fair way of recovery. This contribution dealt chiefly, indeed almost wholly, with the question of steadiness in the large battle-ships of the *Royal Sovereign* class, and the effect of fitting them with bilge keels. This paper may be said to be the complement of another memoir by the same author, read before the Institution last year, in which the experience gained with the big line of battle-ships was recorded. When the *Royal Sovereign* was designed, it was thought that the period of oscillation would approximate to that of the *Inconstant*, *Hercules*, and *Sultan*, which were all remarkably steady ships. The *Hercules* and *Sultan* had only shallow side keels from 9 to 10 inches deep. It was thought that the great inertia due to the large dimensions of the new ships would render them so steady that these small bilge keels might be omitted. That the expectations formed in this respect have not been fulfilled, shows that even the best-informed naval architects, even when supported by the best scientific evidence at their command, may be wrong at times. The performance of the ships of the *Royal Sovereign* class at sea led to the fitting of bilge keels to one of them—the *Repulse*. These keels are each about 200 feet long and 3 feet deep. Four of the class were at sea off the west coast of Scotland in company. There was a long low swell, 300 to 400 feet from crest to crest, and with a period of about ten to twelve seconds. The *Resolution*, without bilge keels, rolled 23, and the *Repulse* 10. The sea was on the quarter, and the speed such as to produce heavy rolling. The records of angle of roll were obtained by the pendulum, an instrument notoriously untrustworthy, but in the present case the results quoted were probably fairly accurate. At any rate, there was no doubt that the behaviour of the ship was immensely improved by the bilge keels, and it was decided to fit all other ships of the class in a similar manner to the *Repulse*. The consensus of opinion of naval officers is that rolling has been greatly reduced. Sir William White very frankly admits that the steadying effect of bilge keels has greatly exceeded that which was anticipated; and, indeed, the opinion of naval architects at large will now have to be readjusted, and the data upon which ship designers work will require to be modified. For scientific purposes more exact information was required than could be obtained by trials at sea, on which, as is well known, it is generally impossible to obtain accurate records. It was therefore arranged that still-water rolling experiments should be made with the sister ships *Revenge* and *Resolution*. Mr. R. E. Froude, whose father's work is so well known in this field, had charge of these trials. One series of experiments was made before bilge keels were fitted, and another after the keels had been attached. Each series was divided into two sections, the ships having a metacentric height of $3\frac{1}{4}$ feet in one case, and a little under 4 feet in the other. The conditions approximate to those of maximum and minimum stability on service. The oscillation of the ships was produced by training the heavy guns and running men across the deck in the usual way. The results of these trials were shown by means of diagrams hung on the walls of the theatre. Curves for the older ships *Sultan* and *Inconstant* were also placed on the diagram. Starting from an angle of inclination of 12° from the vertical, it was seen that in order to reduce the corresponding inclination of 6°, the *Revenge*, without bilge keels, required to make 18 to 20 swings, and the *Sultan* about 17; there being a remarkable similarity between the curves of declining angles of the two ships. The *Sultan*, as already stated, has always been considered a satisfactory vessel in regard to steadiness. Starting from an angle of inclination of 6° from the vertical, it required 45 swings in the *Revenge* without bilge keels to reduce the corresponding angle of inclination to 2°. After bilge keels were fitted, an equal reduction was obtained by only 8 swings. For the *Sultan* and *Inconstant*, 32 to 20 swings respectively would be required to produce the same reduction. The *Revenge*, before the bilge keels were fitted, could be rolled up to an angle of inclination of 13° to the vertical by moving her barbette guns. After the keels were in place it was difficult to exceed an inclination to the vertical of 6° to 8°, even with 300 to 400 men running across the deck, and acting in conjunction with the movement of the guns.

The variation in the periods of swing (from out to out) brought to light by these trials are instructive. Without bilge keels, the author continues, the rolling was practically isochronous at large as well as small angles. The period for a single swing was 7·6 seconds for maximum stiffness, and 8 seconds for minimum stiffness, for large as well as small arcs of oscillation. With bilge keels, within the range of experiment up to a swing of about 12° (6° each side), the period of a single swing decreased as the angle of inclination became smaller; the reduction being about $2\frac{1}{2}$ per cent. in going from a mean inclination to the vertical of 5° to one of 1°. When the metacentric heights and radii of gyration of the ship were appreciably unchanged, there was an increase of about 5 per cent. in the period due to the action of the bilge keels.

A study of the action of bilge keels on a ship's performance not only involves questions of the greatest practical importance to the ship designer, but also problems of high scientific interest. From whichever point of view we consider Sir William White's paper, it is one of exceptional value. It is seldom that the naval architect has the opportunity of making trials or experiments on so grand a scale as that afforded by the big battle-ships of the *Royal Sovereign* class, and for this reason we are giving unusual space to this paper. Nevertheless, we are unable to follow the author in his comparison of the experiments under consideration with those previously made both by French and English engineers. For these we must refer our readers to the original memoir, confining ourselves to the main results, from which a comprehensive view of the scope of the paper may be formed. One of the most important of these results is the increased extinctive effect shown by the experiments to be produced by bilge keels when the ship has headway, compared to the effect when she is not progressing. The experiments under way were limited in number, and, as the author points out, are perhaps not so definite as those with the ship not having headway. "Still they are very suggestive," to once more quote the author's words, "and confirmatory of the conclusion from previous experience that the rate of extinction is sensibly increased by headway, the ship entering undisturbed water while oscillating, and the inertia of that undisturbed water having to be overcome." We can only quote the broad results. Starting at 5° from the vertical the angles of inclination reached, after a certain number of swings, were as follows:—After 4 swings, no headway 2·95°, at 10 knots 2·35°, at 12 knots 2·2°. After 16 swings, no headway 1·15°, 10 knots ·20°, 12 knots ·25°.

It will be remembered by students of this subject that the late Mr. Froude assigned a coefficient of resistance of 1·6 lbs.

per square foot for deadwood and keels moving at 1 foot per second, and this was confirmed by the behaviour of the *Sultan*. Accepting Mr. Froude's formula, the extinctive effect due to bilge keels, such as have been added to the *Revenge*, was calculated, with the result that, in an extreme case, supposing the ship to be rolling to 20° on each side of the vertical, the extinction value due to the bilge keels would appear to be not quite 2°. This, it will be seen, is far short of the observed results obtained from the vessels themselves. Working on the data that have been obtained in this way, new coefficients have been calculated for bilge keel resistance, it being assumed that the whole increase of work were credited to the bilge keel area. For an angle of swing of about 10° instead of the coefficient being 1·6 lbs., it would be about 11 lbs. for a swing of 4° the coefficient would reach as high a value as 15 or 16. It must be remembered that these coefficients are not put forward as truly representative, they only hold good if the assumptions stated are accurate.

In any case the difference is very great. Mr. R. E. Froude, who contributed a most interesting speech to the discussion, confessed that the results took him, when he first had them put before him, entirely by surprise, and, indeed, he did not credit the statements made as to the improved behaviour of the ships; or, rather, he could not attribute this improvement to the presence of the bilge keels. We judged, however, from his remarks that he now accepts the observed data and the truth of the recorded experimental conditions, but still considers the phenomenon one for which he can offer no adequate explanation. He himself had made tank experiments which agreed fairly well with the results obtained by his father, and was quite at a loss to account for the great difference between these experiments and the results of the trials now recorded. The only explanation he could suggest was that bilge keels, on a rolling ship, meet on the return roll with water set in motion by the previous roll; but this, he thought, was quite insufficient to account for an increase in resistance of as much as, say, ten times that which would be calculated on the 1·6 coefficient. Naval architects will be glad to hear that the whole question is to be madethe subject of exhaustive inquiry at Haslar. The principal reasons that bilge keels are not fitted—putting aside expense and difficulties as to docking—are that they add to the immersed surface, and are thus likely to decrease speed. It is, therefore, satisfactory to learn that "the practical tests of actual service prove there is no sensible reduction in speed for power." As it is also stated that the keels have not sensibly reduced diameter of circles made by the vessels, and, further, that additional steadiness in steering has been obtained, it is not hazarding much to say that in future ships of this class in the Royal Navy will all be fitted with bilge keels, unless exigencies of docking forbid their application.

The space which we have devoted to Sir William White's paper will compel us to dismiss most of the other contributions briefly. Mr. Milton's paper on steam-pipes was an excellent practical contribution, and was followed by a no less excellent discussion. The general conclusion arrived at appeared to be that, with high pressure, steel steam-pipes are likely to take the place of those of copper. Mr. Taylor's paper was read in brief abstract, and as it was not in the hands of members until a few minutes before the meeting, we must pass it by. The same thing may be said of Prof. Thurston's paper. It is very gratifying to the members of an English institution to receive papers from foreign members of such eminence as the two American gentlemen just mentioned. We regret we have not yet been able to devote the time to their contributions which their merits doubtless demand. Mr. Yarrow's paper on the aluminium torpedo boat he had built for the French Government, was a very interesting contribution. The boat appears to have been thoroughly successful, so much so that she is to be the prototype of a class. The discussion turned largely on the form of test pieces for copper alloys, it being generally conceded that there is a want of standard conditions for tests. The micro-sections of various alloys thrown on the screen were also very interesting.

The last day of the meeting (Friday) was devoted chiefly to the vibration question, the sitting proving one of the most instructive of the series. As will be seen, three papers were contributed on this important and interesting subject.

These three papers on vibration of steamers formed, with Sir William White's paper on steadiness, the two distinctive features of the meeting. It is hardly necessary to insist on the importance of these two features in steamship performance, both of which affect alike the comfort of the passenger in mercantile vessels, and fighting efficiency in a war vessel. Since engine power has increased so greatly and speeds have been raised, the vibration question has become one of extreme importance in passenger steamers. Two of the most recent largest and costliest of our ocean liners were almost unfitted for carrying passengers—at any rate, they were fast acquiring an uneviable reputation—on account of excessive vibration. By the application of scientific principles the cause of this defect was traced, and the evil cured; a circumstance, if measured by money value, now worth many thousands of pounds to the owners. The extremely interesting paper and series of experiments performed two or three years ago by Mr. Yarrow, at a meeting of this Institution, will be remembered by our readers; and since then Herr Schlick has read two papers on the subject. Records of these will be found in previous volumes of NATURE. The seriousness of vibration in steam vessels is largely dependent upon the period of the hull as a structure synchronising with the beats of the engines, and thus it is that a vessel may vibrate excessively at speeds less than the highest speed she can attain. That is the elementary fact upon which a study of the problem is based. Herr Schlick, in his previous papers, has already considered the case of vibrations of the first order—that is to say, such oscillations of the longitudinal axis of a ship in a vertical direction as have two nodular points. Vibrations of this order claim most attention because they are most common, and are more violent than those of higher orders. It is in vessels with engines running at high speeds that vibrations of a higher order are sometimes observed. It would, as the author of the paper points out, be very advantageous if the naval architect could ascertain beforehand the position of the nodular points of a ship in getting out the design; but this, he is of opinion, cannot be done directly in a satisfactory manner. Mr. Mallock also enters into this question, as will be seen when we deal with his paper later on. As the question cannot be treated directly in a satisfactory manner in the case of a ship, Herr Schlick has recourse to the mathematical investigations of the vibrations of an elastic, prismatical rod. Such investigations have been made by several authorities, and the author quotes at some length the formulæ that have been constructed for vibrations of the first and higher orders. These it is not necessary to repeat.

It is evident that in a complex structure like the hull of a vessel, the vibrations will be of a very different nature to those of a prismatical rod. Treating only of vibrations of the first order—for the author has not yet succeeded in correctly ascertaining coefficients for the second order—Herr Schlick finds that in a ship they are at a greater distance from the ends than in a vibrating prismatic rod; a circumstance which is explained by the fact that a ship is less weighted in the ends than in the middle. For ships of very fine lines, the only class investigated, for vibrations of the first order the distance of the after nodular point from the after perpendicular is 0·231 to 0·253 times the length of the ship. The distance of the fore modular point from the fore perpendicular varies from 0·310 to 0·365 times the length. The author had already shown that an ordinary engine with three cylinders cannot produce vibrations of the first order when the moving weights (pistons, &c.) of each cylinder are in such proportions to each other that the products obtained by multiplying these weights by the distance between the axis of the cylinder and the next nodular point are the same for all three cylinders. The same engine, therefore, will produce vibrations of the second order when the number of the revolutions increases accordingly. The new nodular point, moving away from the engine, causes the moments of the moving weights to be no longer equal to each other. The author considers, therefore, that as the nodular points can only be determined after the ship is completed, it is necessary to alter the moving weights of the engines in such a manner that their moments respecting the nodular point are made equal to each other. The vibrations will thus be considerably reduced, if not entirely avoided. The influence of the screw in producing vibration, owing to the impulses it imparts at the extreme end, is also discussed in this part of the paper; and the author then proceeds to deal with the so-called "horizontal vibrations," which he considers really consist of a twisting action on the ship's axis, due to the turning-moment of the engines, acting on a screw, in the case of a single propeller. An apparatus, constructed for the purpose, illustrated this fact by showing that the horizontal oscillations at the deck and bottom of the ship respectively are in opposite directions at the same instant of time. At a certain height above the keel there is no horizontal oscillation, this being therefore the *locus* of the axis of torsion. Maxima and minima of the turning moment at each revolution depend on the number of cranks, the amplitude of the oscillations being mostly dependent on steam distribution. These oscillations are periodic, and likewise have their nodular points. The author next proceeds to treat the points mentioned mathematically in the case of a prismatic rod. He shows that the number of vibrations is proportional to the speed of progress of vibration. Substituting a ship's body, he finds that this speed of progress remains constant for similar ships, and also that the number of torsional vibrations varies in an indirect proportion with the length of the ship. For a better understanding of these points, we must refer our readers to the original paper and the diagrams by which it is illustrated. That engines of special construction will cause no vibration if placed just above the nodular point, is also true for torsional vibrations.

Mr. Mallock, who, it may be stated, has done much excellent work for the Admiralty in connection with this subject, dealt in his paper with "the determination of the direction and magnitude of the forces and couples which arise from the unbalanced moving parts of marine engines." Something may be done, the author said, towards balancing an engine by the proper disposition of the pistons, connecting-rods, and cranks; but it does not seem practicable to produce a complete balance in any ordinary engine without having recourse to counterbalance weights. In order to determine the weights required, the author has produced a geometrical construction showing, by the aid of arithmetic only, the resultant force and couple due to the unbalanced moving parts of any engine. Without the aid of the diagrams shown at the meeting, it would be impossible to make the explanation clear, even if space permitted us to give the details in full. It will be sufficient to say that the engine is divided up into its component parts, to each of which a value is given, and in this way the resultant force is found and the resultant couple determined. Having got all the information necessary to assign the magnitude and direction of the force and couple which will completely balance the engine, if the force could be applied at the centre of gravity of the moving parts, it would merely remain to decide what weights should be used to produce the required effect. In general the construction of the engine makes this inconvenient, if not impossible, and other positions for the counterbalance weights must be found. This aspect of the problem is then considered in detail by aid of the figures.

The second part of the paper was devoted to showing how the frequency of vibration of any ship, loaded in any manner, can be found by models, and that all the data for shaping these models can be readily obtained from curves which would be in the hands of the ship designer. An example of the apparatus used was shown, the author giving a practical illustration of its working. The course pursued is to make an exact copy of the ship on a very small scale, exactly proportional in all dimensions and identical in material. It is known by theory that the frequency of vibration of the model and ship will be inversely proportional to their lengths. The model is replaced by an exact copy on the same scale, made of some other material—wood—the frequency of the new model differing from that of the former in the ratio

$$\sqrt{\frac{q_s \rho_m}{q_m \rho_s}},$$

where q_m, q_s, and ρ_s are the respective elasticities (Young's Modulus) and densities of the wood and the material of the ship. Next the wooden model is replaced by a plank of the same wood of uniform thickness but variable breadth, the breadth being such that the stiffness of the plank against bending at every cross section is proportional to the stiffness of the model at the corresponding position. Weights are fixed to the plank in such a manner that the weight at any cross section is proportional to the weight at the corresponding section of the model. Then the frequency of the plank, compared with that of the model, can be ascertained by a formula.

In the apparatus shown the plank was supported by two rollers slung from two similar rollers, the latter resting on an overhead railway. The plank was kept vibrating by a magnetic apparatus, and a recording device was added. The rollers supporting the plank gave the position of nodes. It is only when the rollers are at the positions where the nodes would be, if the plank was free from all constraint, that the frequency of the plank will be related to that of the ship as given by the author's formula. The natural nodes are found by varying the position of the rollers until the frequency is a maximum for the type of vibrations under consideration.

The method here introduced by Mr. Mallock is interesting and ingenious, but how far it is applicable to the needs of the naval architect, or whether the average ship builder, if he wish to reduce vibration, will prefer the former method of adjusting the engine to the known conditions of the ship after she is built, are questions which experience alone can decide.

The paper of Mr. Robinson and Captain Sankey dealt largely with the question of vibration in connection with electric light engines, the problem of vibration in the hull of a vessel being thus eliminated. Here again a number of diagrams were used which we cannot now reproduce, and our abstract of this paper must be therefore brief. Investigation showed that in the case of an electric light station the high speed vertical engines, each 200 indicated horse-power, with two cranks set opposite each other, and run at 350 revolutions per minute, were mounted on a large slab of concrete. The engines being vertical, the moving parts had to travel through a greater distance during the upper half of the revolution of the crank pin, than during the lower half. Calculation showed that each line of parts singly tended to lift the engine at up-stroke by about 3·5 tons, and it tended to depress it 2·3 tons. Therefore twice in a revolution a net lifting power of one ton acted upon the engine, and changed an equal number of times into a depressing power of about 1·2 tons. The result was a "pumping action" on the water-soaked soil beneath the concrete slab, and in this way vibration was conveyed to surrounding buildings. The action, it will be seen, was due to the angular movement of the connecting rods, a feature which Herr Schlick said might be neglected; a point in which the authors, naturally, and also Mr. Mallock, by no means agreed with him.

An arrangement of two engines with their framings rigidly connected, and having three cylinders each, was proposed, the object being to neutralise the endways rocking or tilting tendency, and also to give freedom from tendency to vary the downward pressure.

A discussion followed the reading of these papers.

Mr. Hök's paper described a new way of carrying out a known investigation. Whether the new way is better than the old way, is a point which may be decided by experience. The last paper of the meeting, that by Mr. Martin, was of a disappointing nature. Marine engineers have long been asking for an explanation of the hitherto unexplained fact—if fact it be—that "induced draught" is so much better for boilers than "forced draught." Mr. Martin's experiments were quite beside the mark.

The summer meeting of the Institution will be held in Paris, commencing on June 11.

QUESTIONS BEARING ON SPECIFIC STABILITY.[1]

AT the suggestion of your President, I beg to submit three questions to the notice of this Society. They bear on a theoretical problem of much importance, namely, the part played in evolution by "organic stability."

The questions are especially addressed to those who have had experience in breeding, but by no means to breeders only; nor are they addressed only to entomologists, being equally appropriate to the followers of every other branch of natural history. I should be grateful for replies relating to any species of animal or plant, whether based on personal observation or referring to such observations of others as are still scattered through the wide range of periodical literature, not having yet found a place in standard works. The questions are for information on:—

(1) Instances of such strongly marked peculiarities, whether in form, in colour, or in habit, as have occasionally appeared in a single or in a few individuals among a brood; but no record is wanted of monstrosities, or of such other characteristics as are clearly inconsistent with health and vigour.

[1] A paper read at the Entomological Society, April 3, 1895, by Francis Galton, F.R.S.

(2) Instances in which any one of the above peculiarities has appeared in the broods of different parents. In replying to this question, it will be hardly worth while to record the sudden appearance of either albinism or melanism, as both are well known to be of frequent occurrence.

Note.—The question is *not* asked now, whether such peculiarities, or "sports," may be accounted for by atavism or other hypothetical cause.

(3) Instances in which any of these peculiarly characterised individuals have transmitted their peculiarities, hereditarily, to one or more generations. Especial mention should be made, whether the peculiarity was in any case transmitted in all its original intensity, and numerical data would be particularly acceptable, that showed the frequency of its transmission (*a*) in an undiluted form, (*b*) in one that was more or less diluted, and (*c*) of its non-transmission in any perceptible degree.[1]

It is impossible to explain to a general meeting the precise way in which the desired facts would be utilised. An explanation that would be sufficiently brief for the purpose could not be rendered intelligible except to those few who are already familiar with the evidence, and the technical treatment of it by which the law of Regression is established, and with the consequences and requirements of that law. Regressiveness and stability are contrasted conditions, and neither of them can be fully understood apart from the other.

I may as well take this opportunity of appending a list of my various memoirs on these subjects. They appeared from time to time in various forms as the inquiry progressed and as suitable openings occurred for writing or speaking. The more important of these are Nos. 1, 3, part of 6, 7, and 8 in the following list. Nos. 1 to 5 refer to regression only.

LIST OF MEMOIRS, BY MR. F. GALTON, ON REGRESSION AND ORGANIC STABILITY.

(1) Typical Laws of Heredity. *Journal* of the Royal Institution, 1877. (This was the first statement of the law of Regression, as founded on a series of experiments with sweet peas.)

(2) Presidential Address, Anthropological Section of the British Association, 1885. (Here the law of Regression was confirmed by anthropological observations.)

(3) Regression towards Mediocrity in Family Stature. *Journal* of the Anthropological Institute, 1885. (A revised and illustrated reprint of No. 2.)

(4) Family Likeness in Stature. *Proc. Roy. Soc.*, 1886.

(5) Family Likeness in Eye Colour. *Proc. Roy. Soc.*, 1886.

(6) Natural Inheritance. (Macmillan and Co., 1889.) (This volume summarises the results of previous work.)

(7) Patterns in Thumb and Finger Marks . . . and the Resemblance of their Classes to Ordinary Genera. *Phil. Trans. Roy. Soc.*, 1891.

(8) Discontinuity in Evolution. *Mind*, 1894. (An article on Mr. Bateson's work.)

[1] Written communications should be addressed to F. Galton, 42, Rutland Gate, London, S.W.

A NEW DETERMINATION OF THE OHM.

A FRESH determination of the value of the ohm in absolute measure has been made by F. Himstedt (*Wiedemann's Annalen*, liv. p. 305). The method employed is that which the author had used in a previous determination, and consists of passing through a galvanometer all the make or break currents induced in a secondary coil when the current in a long primary helix is interrupted a known number of times per second. A known fraction of the primary current is then passed through the same galvanometer. The primary helix in these experiments consists of a single layer of uncovered copper wire, wound, by means of a screw-cutting lathe, in a regular spiral on a glass cylinder. The turns of wire are held in their place, and the insulation improved, by being coated with shellac. As the mean of a number of determinations, the author obtains the value 106·28 cm. as the length of the column of mercury at 0° C., having a cross section of one square millimetre, which has the resistance of 10^9 C.G.S. units. In connection with the above-described experiments, the author has been led to measure some coefficients of self-induction, using for this purpose a modification of the Rayleigh-Maxwell method. The great difficulty in measuring a coefficient of self-induction by this method is that, in order to get a throw of sufficient magnitude to be accurately measured, it is necessary to employ a somewhat strong current. The result is that the temperature of the coil, the self-induction of which is being measured, rises rapidly, and thus the balance of the Wheatstone's bridge for steady currents is upset. Herr Himstedt gets over this difficulty by using the commutator, which he employs in his determination of the ohm, to break the battery circuit a known number of times per second, and to cut the galvanometer out of circuit while either the make or break is taking place. In this way a steady deflection is obtained of sufficient magnitude to be readily measured, even when the current employed is between 0·001 and 0·002 amperes. The above method only differs from that employed by Profs. Ayrton and Perry in their secohm-meter, in that the author takes two separate readings, one with the bridge balanced for steady currents, the other when the commutator is working, instead of bringing the galvanometer deflector to zero by upsetting the steady current balance.

THE SMITHSONIAN INSTITUTION REPORT FOR 1894.

MR. S. P. LANGLEY'S report of the operations of the Smithsonian Institution for the year ending June 30, 1894, has just reached this country, and it furnishes interesting reading on a number of points relating to the U.S. National Museum, the Bureau of Ethnology, the Bureau of International Exchanges, the National Zoological Park, and the Astrophysical Observatory.

The total permanent funds of the Institution are now 911,000 dollars, and interest at the rate of 6 per cent. per annum is allowed upon this by the Treasury, the interest alone being used in carrying out the aims of the Institution. The total receipts during the fiscal year covered by the Report amounted to 69,967 dollars, and the entire expenditure, including a sum of eight thousand dollars added to the permanent fund, was 67,461 dollars. The Institution also disbursed the Treasury grants of 14,500 dollars for International Exchanges; 40,000 dollars for North American Ethnology; 154,000 dollars for the U.S. National Museum; 50,000 dollars for the National Zoological Park; and 9000 dollars for the Astro-Physical Observatory.

It appears to be an essential portion of the original scheme of the government of the Institution that its secretary should be expected to advance knowledge, whether in letters or in science, by personal research; but the increasing demands of time for labours of administration has greatly limited the possibility of doing this. Mr. Langley has, however, found time to continue his researches upon the solar spectrum (see NATURE, November 1, 1894). This work, carried on in the Astro-Physical Observatory, is certainly of more than common importance. His investigations upon aerodynamics have also been continued intermittently. They are not complete, but they appear to point to conclusions of general and unusual interest.

A wide-spread interest seems to have been awakened in the Hodgkins competition, with reference to investigations appertaining to the nature and properties of atmospheric air. A letter printed in NATURE of June 21, 1894, announced that the time within which papers might be submitted was extended to the end of last year. The Report informs us that, up to June 30, 1894, 250 memoirs, printed and manuscript, had been received in connection with the competition, representing correspondents in the United States, Mexico, England, Scotland, Norway, Denmark, Russia (including Finland), France, Belgium, Germany, Austria-Hungary, Servia, Italy, and British India.

A few grants have been made from the Hodgkins Fund, in aid of certain important researches. In this connection we notice that Prof. E. W. Morley's work on the determinations of the density of oxygen and hydrogen, aided by special apparatus provided by the Institution, is approaching completion.

The investigations undertaken by Dr. J. S. Billings and Dr. S. Weir Mitchell into the nature of the peculiar substances of organic origin contained in the air expired by human beings, has been continued under a grant from the Hodgkins Fund, and also the researches by Dr. O. Lummer and Dr. E. Pringsheim, of Berlin University, on the determination of an exact measure of the cooling of gases while expanding, with a view to revising the value of that most important constant which is technically termed the "gamma" function.

Mr. Langley refers again to the unsatisfactory condition of the National Museum. The collections have increased so greatly that unless additional space is provided for their proper administration and exhibition, the efficiency of the Museum will be greatly impaired; but though the collections are growing rapidly in certain directions, they are not increasing as symmetrically and consistently as is manifestly desirable—a very common cause of complaint. A defect which calls for instant attention, however, relates to the most undesirable and dangerous storage of collections in wooden sheds near the Smithsonian building, and in the basement of the building itself, where large alcoholic collections in bottles containing, in the bulk, many thousands of gallons of alcohol, have been put away, as space cannot be found for the specimens in the Museum. It appears that a fire communicated to these rooms would sweep through the entire length of the building, and although the building itself is fireproof as against any ordinary danger, it may well be doubted whether any of the collections therein exhibited can be regarded as safe, if the rooms immediately below should be exposed to so peculiarly severe a conflagration as would be caused by the ignition of these large quantities of inflammable material. Such a calamity would affect the whole scientific world, and we trust that the appeal for a change of the present condition of affairs will not be disregarded.

The investigations relating to the ethnology of the American Indians were carried forward during the year, under the efficient control of Major J. W. Powell, the director, aided by Mr. W. J. McGee, ethnologist in charge, as executive officers. These researches of the Bureau of American Ethnology embrace the subjects of archæology, descriptive ethnology, sociology, pictography and sign language, linguistics, mythology, psychology, and bibliography, and the results obtained during the year have never been exceeded in value.

The Smithsonian Exchange Service was inaugurated nearly half a century ago, with the object of carrying out one of the purposes of the founder of the Institution in the diffusion of that knowledge which the Institution itself helped to create. For this purpose it established correspondence with scientific men all over the world, until there is no civilised country or people, however remote, upon the surface of our planet, so far as is known, where the Institution is not thus represented. These correspondents have grown in numbers until at the present time those external to the United States alone number nearly 17,000. More than one hundred tons of books passed through the exchange office during the fiscal year 1893–94.

It was only five years ago since an appropriation was made for the National Zoological Park at Washington. The park has an area of nearly 167 acres, but there are as yet only four permanent buildings, while the animals number 510, of which 200 are of the larger size. Comparing this with similar establishments at other capitals, it is noted that the Gardens of our Zoological Society cover about 36 acres, are crowded with buildings, and that the magnificent collection of animals, some 2300 in number, is housed in a fairly comfortable manner. In Paris the portion of the Jardin des Plantes assigned to animals is a plat of ground some 17 acres in extent, crowded with 900 animals. In Berlin the portion of the Thiergarten appropriated for animals occupies about 60 acres. Fifteen hundred animals are accommodated and, necessarily, much overcrowded. In the United States the principal collections are in Philadelphia, where the grounds occupy about 40 acres, and the collection comprises 881 animals; in Cincinnati, where 36 acres are occupied with about 800 animals; and in New York, where the city maintains about 700 animals in Central Park, occupying an area of approximately 10 acres. In none of these collections are the grounds of sufficient size to give any extensive range for the animals.

Appended to Mr. Langley's general account of the affairs of the Institution and of its bureaus, are the detailed and statistical reports from the officers in charge of the different branches of work. The whole shows how very great and valuable is the work done in the United States "for the increase and diffusion of knowledge among men."

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

Prof. C. S. Sherrington, F.R.S., has been appointed to the George Holt Chair of Physiology in University College, Liverpool.

Dr. A. W. Crossley, Berkeley Fellow, and Demonstrator of Organic Chemistry, in the Owens College, Manchester, has been elected Demonstrator of Chemistry in the Medical School of St. Thomas's Hospital, in succession to Dr. W. H. Ince, who has been appointed Government Chemist at Trinidad.

Sir Dinshaw Manockjee Petit, Bart., has, through the Principal, Mr. S. Cooke, offered to the Indian Government the sum of 5000 rupees towards the cost of providing residential quarters for Parsee students attending the College of Science at Poona.

The Governors of Colfe's Foundation have selected Mr. F. W. Lucas to be Head Master of Colfe's Grammar School, Lewisham, from September next. Mr. Lucas is at present Head Master of Hipperholme Grammar School, Yorks, and was formerly Senior Assistant and Science Master at Roan School, Greenwich.

We learn from the *Lancet*, that in accordance with the will of the late Dr. G. Y. Heath, Professor of Surgery in the University of Durham, and President of the University of Durham College of Medicine, the trustees of the Heath Scholarship, Prof. W. C. Arnison and Mr. Frederick Page, will award and pay to the writer of the best essay on Surgical Diseases of the Jaws the sum of £200. All graduates in medicine or in surgery of the University of Durham are eligible to compete for this prize. The essay must be type-written or printed, and delivered to the trustees not later than March 31, 1896. The essay, together with any specimens, drawings, casts, microscopical preparations, or other means of illustration accompanying it, will become the property of the College of Medicine, Newcastle-upon-Tyne, but by permission the essay may be printed for general circulation by the Heath Scholar. Mr. Stephen Scott, of Harrogate, has generously presented to the College of Medicine the sum of £1000, which has been devoted, in accordance with Mr. Scott's wish, to founding a scholarship to promote the study of hernia and allied subjects. Any graduate in medicine or surgery of the University of Durham, or any student of the University of Durham College of Medicine is eligible to compete for the scholarship, provided that such student shall have had at least one academical year in attendance at the College, and that in any case his age does not exceed thirty years at the time when the essay is sent in. The competition takes place every year. Essays for this year's competition must be sent not later than July 31, 1895, to Prof. Arnison, University of Durham College of Medicine, Newcastle-upon-Tyne.

On Friday last, Mr. Acland received a deputation at the Education Office, from the representatives of the Association of Head Masters, respecting the recent regulations which have been issued by the Science and Art Department with reference to organised science schools. After hearing the views of the deputation, Mr. Acland, in reply, said it was not desired to make an upheaval of the arrangements for these schools, but to join together in improving the method and the system on which the teaching was carried on. They were all agreed that to lessen too frequent examination, and to introduce the element of inspection, if reasonably carried on in a friendly spirit, would be of great value to these schools. One of the objects of the Department had been to make it clear that, besides the teaching of science, which was the primary object of these schools, they also desired fully to recognise the element of literature and the teaching of special subjects. In order to meet a point which had been raised, as to the change from the old system of organised science schools to the new, the closing words of the syllabus would be:—"Reasonable latitude will be allowed for two years in any departures which may be made from the prescribed course while the changes from the present to the new system are being brought about." Taking these words, together with the words in the earlier part of the syllabus, as to reasonable latitude being allowed to teachers as to the nature of the course which they might pursue, provided that the instruction was sound, satisfactory in amount, and combined with proper practical work, it would be seen that the Department had no intention of being too despotic, and that if really good and reasonable work was done under some more elastic system, these organised science schools would be found of even more benefit in the future than they had been in the past.

SCIENTIFIC SERIALS.

The Mathematical Gazette, No. 4 (Macmillan).—This is the first number of the enlarged series. We are glad to find that the support accorded to the first year's issue has been sufficient to warrant this enlargement; but to make the *Gazette* a success, and not a drag upon the funds of the Association, it is imperatively necessary that a much larger measure of support should be rendered by the general body of mathematical teachers. The opening paper is one on algebra in schools, which was read before the Association at its annual meeting in January of this year. In this article the author, Mr. Heppel, drawing upon his wide experience as a "coach," states that when pupils have come to him he has found that the work in algebra has usually to be done all over again. The reason of this appears to him to be "the ever-growing divergence there is between the conception of the nature and objects of algebra that dominates school teaching and the conception that regulates the application of algebra to more advanced mathematics." Many of the suggestions are likely to be useful, and we commend them to the notice of our brethren in the craft of teaching.—Mr. T. Wilson contributes a note on mathematics for astronomy and navigation, in which he suggests that the elements of spherical trigonometry might occupy a more prominent place in school teaching than they do, and to cover all ages he winds up with, "let no one despair that he is too old for mathematics."—Mr. Rouse adds a second chapter to his previous interesting article on conics.—"Some old text-books" is a review of John Ward's "The Young Mathematician's Guide" (1747), by Mr. J. H. Hooker, which brings before us matter that was served up for the food of students in the time of "good Queen Anne." The rest of the number is taken up with more extended articles (than before) entitled notes, solutions, new questions, and titles of new books. These latter pages should be of general interest, as they are likely to be useful both to students and teachers.

Bulletin of the American Mathematical Society, series 20, vol. i. No, 6, March 1895.—The notice of "Arthur Cayley," pp. 133–141, which opens this number, is a warm appreciation of the character and writings of our great mathematician, by Dr. Charlotte Scott, and is due to her "intense admiration for his work and personality, and to the fact that for the last fourteen years" she has "been privileged to know him and experience his kindness." It is the fullest account we have yet read, and has many more points of interest for an Englishman than Signor Brioschi's *éloge*, which is naturally confined more closely to an appreciation of his mathematical work. One extract we must make:—"Any sketch of Prof. Cayley is self-condemned if it leaves out of account the child-like purity and simplicity of his nature, the entire freedom from the professional touchiness on the score of priority to which mathematicians are as liable as other men. He was ever ready to say what he was working at, to indicate the lines of thought, to state what difficulties he was encountering . . . but his greatness and his simplicity cannot be enshrined in anecdotes.—Prof. Osgood (pp. 142–154) in "The Theory of Functions," analyses, chapter by chapter, Dr. (now Prof.) Forsyth's brilliant work on "The Theory of Functions of a Complex Variable," and winds up thus:—"The book is not one that can safely be put into the hands of the immature student for a first introduction to the study of the theory of functions. But the student who is already familiar with the elements, and who has acquired some degree of critical power, will find its pages incentive to valuable work in this wide field."—A short note follows on the introduction of the notion of hyperbolic functions, by Prof. Haskell, which was read before the Society at its December (1894) meeting.—The second summer meeting of the Society is to be held at Springfield, Mass., on August 27.

Internationales Archiv für Ethnographie, Band vii. Heft iv. 1894. This part commences with a long and thorough study (in German) on the hair-cutting customs of the Southern Slavs, by Friedrich S. Krauss. Several songs are reproduced in the original, which are also translated into German. In this study two elementary ideas of mankind are met with, but imbued with the local colour of the Southern Slavs, and varied in tint according to the stage of culture. Hair-cutting is a means of adoption into kinship, and also as a redemption from the sacrifice of the body or life to the spirit of disease. It is a rite performed for social obligations and for good luck.—Prof. P. J. Veth concludes his exhaustive account (in Dutch) of the Mandrake, which is a valuable contribution to signature-lore or sympathetic magic. The most interesting of the "Notes" is an illustrated communication by A. Hermann, on the cupping and blood-letting appliances of the wandering gypsies.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, March 21.—"On the Diselectrification of Air." By Lord Kelvin, P.R.S., Magnus Maclean, and Alexander Galt.

§ 1. The experiment described in § 14 of our paper on the "Electrification of Air and other Gases by bubbling through Water and other Liquids" (*Roy. Soc. Proc.*, February 21, 1895), proves that air, electrified negatively by bubbling through water and caused to pass through a metallic wire gauze strainer, gives up some, but not a large proportion, of its electricity to the metal. We have now made a fresh experimental arrangement for the purpose of investigating diselectrification of air which has been electrified, whether positively or negatively, by other means than bubbling through water: with apparatus represented in Figs. 1 and 2, which is simplified from that of our former paper by the omission of the apparatus for electrification by bubbling, and for collecting large quantities of electrified air.

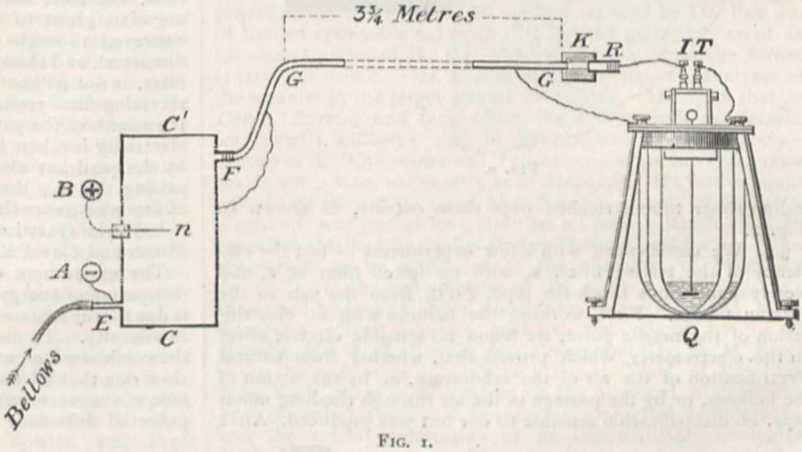

FIG. 1.

§ 2. In Fig. 1, A B represent the two terminals of a Voss electric machine connected, one of them to a metal can, C C′ (a small biscuit canister of tinned iron), and the other to a fine needle, of which the point *n* is in the centre of the can. The wire making the connection to the needle passes through the centre of a hole in the side of the can, stopped by a paraffin plug. Air is blown from bellows through a pipe, E, near the bottom of the can, and allowed to escape from near the top through an electric filter, F, called the tested filter, from which it passes through a long block-tin pipe, G G, about $3\frac{1}{4}$ metres long and 1 cm. internal diameter, and thence through a short tunnel in a block of paraffin, K. From this, lastly, it passes through a second electric filter, R, into the open air. This second filter, which we sometimes call the *testing filter*, sometimes the *electric receiver*, is kept in metallic connection with the insulated terminal, I, of a quadrant electrometer, Q. The metal can and the block-tin pipe are metallically connected to the outer case and uninsulated terminal, T, of the quadrant electrometer.

§ 3. The testing filter or electric receiver consists of twelve discs of brass-wire cloth fixed across the mouth of a short metal pipe supported on the end of the paraffin tunnel in the manner represented in Fig. 2, on a scale of twice the size of the filter which we have actually used, or of true size for a filter on a tube of 2 cm. diameter, which for some purposes may be better. One of eleven similar discs, of size adapted to a tube of 2 cm. diameter, and an outermost disc with projecting lugs, are shown, true size, and with the gauge of the wire-cloth which we have actually used, shown true size, in Fig. 3. The eleven little circular discs of wire cloth are held in position by bending over them the four lugs belonging to the outermost disc, and all are kept compactly together by a short piece of

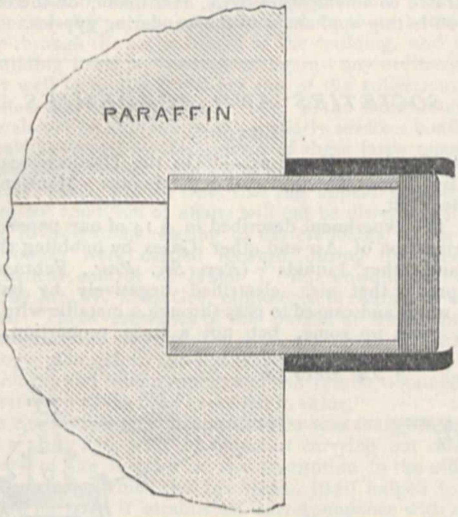

FIG. 2.

india-rubber tube stretched over them outside, as shown in Fig. 2.

§ 4. We commenced with a few experiments to test the efficiency of the testing filter, R, with no tested filter at F, and merely continuous block-tin pipe, F G G, from the can to the paraffin tunnel. First, working the bellows with no electrification of the needle point, we found no sensible electric effect on the electrometer, which proved that, whether from natural electrification of the air of the laboratory, or by the action of the bellows, or by the passage of the air through the long metal pipe, no electrification sensible to our test was produced. After

FIG. 3.—Twenty-five wires to the centimetre. Diameter of each wire is 0·16 mm. Hence each aperture is 0·24 mm. square.

that we kept the needle point, *n*, electrified, either positively or negatively, for five or six minutes at a time by turning the little Voss machine, and we found large effects rising to about $3\frac{1}{2}$ volts in five minutes, positive or negative, according as *n* was positive or negative.

§ 5. The apparatus is now ready to test the efficacy of filters or other appliances of different kinds placed at F for the purpose of diselectrifying air which has been electrified, whether positively or negatively, by the electrified needle point *n*. We began with a filter of 12 wire-gauze discs, placed at F and kept in metallic connection with the tin pipe outside. This nearly halved the electricity shown by the electrometer. We then tried 24, 48, 72, 96, 120 wire-gauze discs, successively, placed in groups of 24, and separated from one another by short lengths of 2 cm. of lead tube, in the line of the flow of the air between F and G (Fig. 1), all kept in metallic connection with the block-tin pipe and the outer case of the electrometer. We were surprised with the smallness of the additions to the diselectrifying efficiency of the 12 strainers first tried: for example, the filter of 120 wire gauzes only reduced the electrical indication to a little less than one-half of what it was with the 12 which we first tried.

We found that cotton-wool between the spaces in the groups of 24 wire gauzes largely increased the diselectrifying effect. Thus, with 72 wire gauzes and cotton-wool we succeeded in reducing the electrical effect to about one-twelfth of what it was with only a filter of 12 wire gauzes; but hitherto we have not succeeded in rendering imperceptibly small the electricity yielded by the outflowing air to the testing filter R in our method of observation.

§ 6. We intend trying various methods of obtaining more and more nearly complete diselectrification of the electrified air flowing out of the can at F; and this for air electrified otherwise than by the needle point, as shown in the diagram: for instance, by an electrified flame in place of the needle point; or again by bubbling through water or other liquids. Meantime, the mere fact that the electricity, whether positive or negative, given to air by an electrified needle point, can be conveyed through 3 or 4 metres of small metal tube (1 cm. diameter), and shown on a quadrant electrometer by a receiving filter, is not without interest. We may add now that, with the receiving filter removed and merely a fine platinum wire put in the mouth of the paraffin tunnel, we have found that enough of electricity is taken from the outflowing air to be amply shown by the quadrant electrometer; which renders even more surprising the fact that the diselectrifying power of 120 strainers of fine wire-gauze should be so small as we have found it.

"On the Question of Dielectric Hysteresis." By Alfred W. Porter and David K. Morris.

The experiment described was intended to test whether the dissipation of energy that occurs in the dielectric of a condenser is due to true hysteresis (as claimed by Riccardo Arnò), or simply to viscosity. To discriminate between them it is essential that the condenser be put through a cyclic series of states at such a slow rate that all viscous effects shall have had time to subside before a measurement of the charge corresponding to a certain potential difference is made. It is essential also to arrange

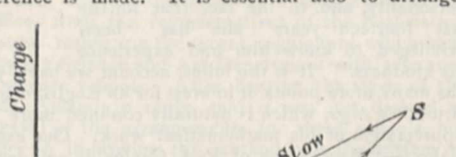

FIG. 1.

that any test of the charge which involves a change of state should itself form part of the cycle. This was accomplished by making the cycles as shown in the figure, the cyclic order being RPURPSQUR; the portions PU and QU representing discharges through a galvanometer. If hysteresis exist, the change of charge QT will be greater than the change PT. As the result of twelve cycles the ratio QT/PT comes out $1 \div \frac{1}{8760}$; a second series of twelve gives $1 - \frac{1}{10350}$.

Hence the charge is sensibly the same for a given value of the potential difference, whether that value has been arrived at from lower or from higher values than itself.

Fresh evidence is also given of the presence of viscosity.

Thus while the condenser here experimented upon exhibits marked viscous effects, yet the authors could detect no hysteresis.

Entomological Society, April 3.—Prof. R. Meldola, F.R.S., President, in the chair.—Mr. C. J. Gahan exhibited two examples, male and female, of a rare Prioned beetle, *Chariea cyanea*, Serville, which had been kindly sent to him for examination by M. René Oberthür; and stated that Lacordaire was mistaken with regard to the sex of the specimen which he described in the "Genera des Coléoptères." He pointed out that the elytra of the male were relatively much shorter than those of the female; and that the joints of the antennæ from the third to the tenth were biramose. Mr. Gahan also exhibited two species of the genus *Decarthria*, Hope, and said he believed these were the two smallest species of Longicorns known.—Dr. Sharp, F.R.S., exhibited the soldiers and workers of a species of Termites found by Dr. Haviland in South Africa. He stated that these insects possessed eyes and worked in daylight like Hymenopterous ants, and that in habits they resembled harvesting ants by cutting grass and carrying it into holes in the ground. Dr. Sharp said that although these holes were probably the entrance to the nests, Dr. Haviland was unable to find the actual nest, even by prolonged digging, so that the winged forms were still unknown. He thought this species was probably allied to *Termes viarum* of Smeathman, in which the soldiers and workers possessed eyes, and had been observed by Smeathman to issue from holes in the ground, but whose nests could not be discovered. Mr. McLachlan observed that it was possible there might be species of Termites without any winged form whatever.—Mr. Rye called attention to the action of one of the Conservators of Wimbledon Common, who, he stated, had been destroying all the aspens on the Common. He inquired whether it was possible for the Entomological Society to protest against the destruction of the trees. Mr. Goss said he would mention the matter to the Commons' Preservation Society.—Mr. Francis Galton, F.R.S., read a paper entitled "Entomological Queries bearing on the Question of Specific Stability." (See p. 570.)—Mr. Merrifield stated that he received some years ago, from Sheffield, ova of *Selenia illustraria*, the brood from which produced, in addition to typical specimens, four of a dark bronze colour, and from these he bred a number of specimens of a similar colour.—Dr. F. A. Dixey referred to a variety of the larva of *Saturnia carpini* with pink tubercles. He said the imago bred from this larva produced larvæ of which 10 per cent. had pink tubercles. Prof. Poulton, F.R.S., said he had found larvæ of *Smerinthus ocellatus* with red spots, and that this peculiarity had been perpetuated in their descendants. Mr. McLachlan, Canon Fowler, and Prof. Meldola made some further remarks on the subject.—Mr. G. F. Hampson read a paper by Mr. C. W. Barker, entitled "Notes on Seasonal Dimorphism in certain species of Rhopalocera in Natal." Mr. Merrifield said he was of opinion that a record of the temperature at different seasons would be a very desirable addition to observations of seasonal dimorphism. Mr. Hampson said he believed that temperature had very little to do with the alteration of forms. At any rate, according to his experience, in India the wet season form succeeded the dry season form without any apparent difference in the temperature. Prof. Poulton remarked that the apparent temperature as felt must not be relied upon without observations taken by the thermometer. Dr. Dixey, Mr. Barrett, Dr. Sharp, and Prof. Meldola continued the discussion.

Zoological Society, April 2.—W. T. Blanford, F.R.S., Vice-President, in the Chair.—Mr. Boulenger exhibited the type specimens of two new chameleons from Usambara, German East Africa. Special interest attached to them from the fact that they appeared to be more nearly related to the Madagascar species than to any of the numerous forms now known from Continental Africa.—Mr. Walter E. Collinge read a paper on the sensory canal system of fishes, treating of the morphology and innervation of the system in the Physostomous Teleostei.—Dr. Mivart, F.R.S., read a paper descriptive of the skeleton in *Lorius flavopalliatus*, comparing it with that of *Psittacus erithacus*, and pointed out a number of differences in detail.—Mr. G. A. Boulenger, F.R.S., made remarks on some cranial characters of the salmonoid fishes, and expressed the opinion that there was no justification for separating *Coregonus* and *Thymallus* from the Salmonidæ, as had been proposed by Cope and Gill.—Prof. T. W. Bridge read a paper in which he pointed out certain features in the skull of *Osteoglossum*. The author directed attention to the existence of a peculiar oral masticatory mechanism in *Osteoglossum formosum*, distinct from that furnished by the upper and lower jaws and their teeth. The existence of an essentially similar mechanism in the Ganoid *Lepidosteus osseus* was also described, and the conclusion suggested that the two genera offer in this respect an interesting example of parallelism in evolution.

Linnean Society, March 21.—Mr. C. B. Clarke, President, in the chair.—Prof. Stewart exhibited and made remarks upon a series of corals, dwelling upon certain characteristic features which illustrated their structure.—Mr. S. Pace brought forward a collection of shells belonging to the genus *Columbella*, and made some observations concerning the peculiarities and the geographical distribution of some of the species exhibited. A paper was then read by the President, "On the terminal flower in the *Cyperaceæ*." After remarking that the order *Cyperaceæ* had been newly arranged by Dr. Pax in Engler's "Jahrbücher" (1886), and in Engler and Prantl's "Pflanzenfamilien," the character taken for primary division of the order being the inflorescence, he pointed out that in the first sub-order Scirpoideæ with an axillary flower were placed *Cyperus*, *Scirpus*, *Psilocarya*, *Dichromena*, and *Hypolytrum*; in the second sub-order Caricoideæ with a terminal flower were placed *Schoenus Rynchospora*, *Mapania* and also *Carex*, *Scleria*, and their allies. The disruption of *Hypolytrum* from *Mapania*, of *Dichromena* and *Psilocarya* from *Rynchospora*, he thought, proved either that the modern method pursued by Dr. Pax was of limited systematic value, or that he had grievously erred in his ascertainment of the fact whether in such genus the flower is terminal or not. Mr. Clarke exhibited his own analyses of the spikelet in the larger genera in dispute. He held that in *Carex*, *Scleria*, and their allies, the flower, male and female, was strictly axillary; that in *Rynchospora* it was axillary—exactly as in *Dichromena* and *Psilocarya*—while in *Hypolytrum* the flower is terminal exactly as in *Mapania*. He further maintained that these facts could be sufficiently shown by the aid of a penknife and pocket-lens, and that no results which might be hereafter obtained by studies in development could affect either the weight to be attributed to the character of "terminal flower," or to the real affinities of the genera. The paper was illustrated by lantern slides illustrating dissections, and, in the discussion which followed, criticism was offered by Sir D. Brandis, Mr. A. B. Rendle, Dr. Prain, and Dr. D. H. Scott.—On the conclusion of this paper, Dr. H. Field, of Brooklyn, New York, made some remarks on the proposed establishment of a central international bureau for zoological bibliography, and the annual publication of an international Zoological Record.

Mathematical Society, April 4.—Major MacMahon, R.A., F.R.S., President, in the chair.—The Rev. T. C. Simmons read a paper on a new theorem in Probability. The author replied to numerous questions put to him by Messrs. G. H. Bryan, Burton, Cunningham, and the President.—The President (Mr. Kempe, F.R.S., in the chair) communicated a note on the linear equations that present themselves in the method of least squares.—The following paper was taken as read: On the Abelian system of differential equations and their rational and integral algebraic integrals, with a discussion of the periodicity of Abelian functions, by the Rev. W. R. W. Roberts.

Geological Society, March 20.—Dr. Henry Woodward, F.R.S., President, in the chair.—On fluvio-glacial and interglacial deposits in Switzerland, by Dr. C. S. Du Riche Preller. This paper is the outcome of one published in the *Geological Magazine* of January 1894, on the "Three Glaciations in Switzerland," in which the author described various glacial deposits near the lake of Zürich. He now describes a series of fluvio-glacial conglomerates and interglacial lignite-deposits near the lakes of Zürich, Constance, Zug, and Thun, which, together with analogous deposits at the base of the Eastern, Western, and Southern Alps, constitute further evidence of two interglacial periods, and therefore of three general glaciations, the oldest of these being of Upper Pliocene, and the others of Middle and Upper Pleistocene age respectively. As regards the origin, age, and the time required for the formation of several of the Swiss deposits referred to in the paper, the author arrives in several respects at conclusions differing from those recently enunciated by others. The author also argues that the first interglacial period was probably of shorter duration than the second; and in confirming his former conclusion that every general glaciation marks a period of filling-up, and every interglacial period marks a period of erosion of valleys, he avers that, if this conclusion be correct, it must needs be destructive of the theory of glacial erosion.—The Bajocian of the Mid-Cotteswolds, by S. S. Buckman. The Mid-Cotteswolds is defined as the district between the valleys of the Frome and the Chelt. A description of twenty-five sections is given, dealing principally with the strata found between the Upper *Trigonia*-grit and the Upper Freestone.

Paris.

Academy of Sciences, April 1.—M. Cornu in the chair.—On the composition of drainage waters, by M. P. P. Dehérain. An account is given of experiments made on a large scale with fallow-land and with crops of barley, wheat, beet-root, and the vine. The observations began regularly in March 1892; the results are given for three seasons. Comparing the crops of 1893 and 1894 as regards nitrogen, it is seen that the abundant crop of the latter year leaves the soil no more exhausted than the medium crop of 1893. The nitrates produced in the soil, or added as manure, were better utilised in 1894; with the poorer crop a proportion was lost. The author differs from M. Schlœsing, insomuch as the latter believes the loss of nitrogen in drainage water to be so insignificant as to be able to be neglected in practical farming, whereas his own results confirm the Rothamsted experiments, and show that the loss from fallow-lands is much greater than from lands covered with vegetation. The deduction is drawn that it is good practice to follow up crops such as wheat by some form of autumn growth.—Ultra-violet radiation of the solar corona during the total eclipse of April 16, 1893, by M. H. Deslandres. A description of a photograph of the spectrum of the corona obtained in the Senegal expedition of 1893. The photo-spectrometer used had lenses and prisms of quartz and calcite, and thus enabled a great prolongation of the ultra-violet region to be photographed. In accordance with previous observations, it was found that the ultra-violet spectrum was very feeble in intensity as compared with the red; this may be due in part to the great absorbing power of the atmosphere for light in this region of the spectrum.—Solar observations of the second, third, and fourth quarters of 1894, by M. Tacchini.—On the theory of equations to the derived partials of the second order, by M. E. Goursat.—On the sequences of circular permutations, by M. Désiré André. An analogy is pointed out between circular and rectilinear permutations, and it is shown that the former are, in general, more simple. They are not subject to the irregularity introduced into rectilinear permutations by the terminal terms.—On the application of the theory of probability of errors to levelling operations of precision, by M. M. d'Ocagne.—On gratings used in "photogravure," by M. Ch. Féry. A grating of 40 to 60 lines to the centimetre is used to enable the production of a photograph which can be directly reproduced by mechanical processes. Such a photograph must necessarily be devoid of half-tints; the device of placing a grating at a short distance before the sensitive surface replaces these half-tints by alternate black and white squares of the same size. An explanation of this effect is given by the author on the basis of the elementary theory of shadows. On the "molecular deviation" or the "molecular rotatory power" of active substances, by M. A. Aignan. The author asserts that M. Guye's formula for molecular rotatory power is inexact, and cannot be used instead of Biot's formula for the specific rotatory power in the case of solutions.—On a radiometer of symmetrical construction, turning under the action of unsymmetrical illumination, by M. G. Seguy.—An absolute electrometer for high potentials, standard and simplified types, by MM. H. Abraham and J. Lemoine.—An extremely sensitive galvanometer, by M. Pierre Weiss.—On the oldest French series of meteorological and thermometric observations, by M. l'abbé Maze. An account of the contents of a newly-discovered register, entitled "Ad thermometrum observationes anno 1658 Parisiis: Thermometrum Florentiæ fabricatum."—On the first mercury thermometer, by M. l'abbé Maze. Ismael Boulliau used a mercury thermometer together with his Florence thermometer in March 1659, or sixty-two years before Fahrenheit's invention.—Thermal study of the anhydrous barium and strontium iodides, by M. Tassilly.—On the properties of salts of nickel and cobalt, by M. de Koninck. A priority claim.—On the alcoholates of lime and baryta, by M. de Forcrand. A thermal study of the compounds—$(C_2H_6O)_4(CaO)_3$, $(CH_4O)_4(BaO)_3$, and $(C_2H_6O)_4(BaO)_3$. The action of alcohols on the alkaline-earthy oxides does not give true metallic alcoholates, but addition compounds.—On the ammonium bases derived from hexamethyltriamidotriphenylmethane and their action on the fuchsines, by M. A. Rosenstiehl.—On some new combinations of hexamethylene-amine, by M. Delépine.—On the gases of the swimming bladder of fishes, by M. Jules Richard. These consist of oxygen, nitrogen, and traces of carbon dioxide. The oxygen varies in three cases given from 78·6 to 87·7 per cent.—Action of the nervous system on the principal lymphatic canals, by MM. L. Camus and E. Gley.—On the genus *Eurya*, of the family Ternstræmiaceæ, by M. J. Vesque.—On the basic rocks occurring as narrow veins in the lherzolite of the Pyrenees, by M. A. Lacroix. There are two families of granular basic rocks, without peridote and felspar, which are allied to the peridotites.

BOOKS and SERIALS RECEIVED.

Books.—Rainfall in the East Indian Archipelago, 1893 (Batavia).—Short Studies in Nature Knowledge: W. Gee (Macmillan).—Stéréochemie: E. G. Monod (Paris, Gauthier-Villars).—Practical Microscopy: G. E. Davis, 3rd edition (Allen).—Cambridge Natural History—Molluscs and Brachiopods: Rev. A. H. Cooke, A. E. Shipley, and F. R. C. Reed (Macmillan).—Manual of Geology: Prof. J. D. Dana, 4th edition (New York, American Book Company).—The Spirit of Cookery: Dr. J. L. W Thudichum (Baillière).—The Evolution of Industry: Hy. Dyer (Macmillan)—Horses, Asses, Zebras, Mules and Mule-Breeding: W. B. Tegetmeier and C. L. Sutherland (Cox).—Sir Samuel Baker—A Memoir: T. D. Murray and A. S. White (Macmillan).—Our Teeth—Care and Preservation: Dr. V. Ditcham (Baillière).—Clinical Lectures on the Prevention of Consumption: Dr. W. Murrell (Baillière).—La Fonctionnement des Machines a Vapeur: G Leloutre (Paris, Gauthier-Villars).—Des Marées: P. Hatt (Paris, Gauthier-Villars).—Manchester Museum: Owens College, Catalogue of the Library: W. E. Hoyle (Manchester, Cornish).—Methodisches Lehrbuch der Elementar-Mathematik: Dr. G. Holzmüller, Erster und Zweiter Teil (Leipzig, Teubner).—The Book of the Dead—Fac-simile of the Papyrus of Ani in the British Museum, 2nd edition (British Museum).—The Partition of Africa: J. S. Keltie, 2nd edition (Stanford).

Serials.—Scribner's Magazine, April (Low).—Meteoric Papers, No. 1: J. Calvert (London).—Natural History of Plants: Kerner and Oliver, Part 12 (Blackie).—Notes from the Leyden Museum, Vol. xvi. Nos. 3 and 4 (Leyden, Brill).—Mind, April (Williams and Norgate).—Proceedings of the Society for Psychical Research, March (Paul).—Phonographic Quarterly Review, April (Pitman).—Transactions of the Natural History Society of Queensland, Vol. 1 (Brisbane).—Bulletin of the Geographical Club of Philadelphia, March (Philadelphia).—Records of the Geological Survey of India, Vol. xxviii. Part 1 (Calcutta).—Bulletin of the New York Mathematical Society, March (New York, Macmillan).—Mathematical Gazette, February (Macmillan).—Annals of Scottish Natural History, April (Edinburgh, Douglas).—Reliquary and Illustrated Archæologist, April (Bemrose).—Archiv für Pathologische Anatomie und Physiologie und für Klinische Medicin, Band 140, Heft 1 (Berlin, Reimer).—Himmel und Erde, April (Berlin, Paetel).—Proceedings of the Physical Society of London, Vol. 13 Part 5 (Taylor and Francis).

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