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ELECTRO-MAGNETISM.

The Electro-magnet and Electro-magnetic Mechanism. By S. P. Thompson, D.Sc., F.R.S. (London: E. and F. N. Spon, 1891.)

THIS is a reprint, with additions, of a course of Cantor Lectures which the author delivered during 1890 before the Society of Arts. The book, it may be stated at the outset, is an excellent account of electro-magnetic mechanism, and abounds in information at once of historical, scientific, and practical value. Evidently the author is willing to spare no pains to give completeness to any work of this kind he undertakes, and the present book, like his treatise on dynamo-electric machinery, will no doubt be widely read and appreciated.

In the preface, and elsewhere in the body of the book itself, Dr. Thompson indulges in some statements which we think are a blemish on an excellent and well-written treatise. It serves no good purpose to distribute in a scientific book praise or blame to certain classes of scientific investigators, and while glorifying a certain section of workers, to pour what seems little short of contempt and derision on the labours of another. We refer here to the statements (chiefly in the preface) regarding the earlier mathematical theories of magnetism, and their alleged influence in retarding electro-magnetic discovery.

It is quite true that some of the older theorists, concentrating their attention on the magnetic field of a system of permanent magnets, gave only a secondary attention to the problem of the internal constitution of a magnet. But it is hardly fair to put down against them, by inference, the errors of the persons who persisted in assuming that, because it had been proved that a magnet produces a field which can also be accounted for by a distribution of imaginary magnetic matter over the surface of the magnet, the magnetization of the magnet did consist in such a distribution. Such an equivalent distribution is a conception helpful in itself, inasmuch as it can be experimentally determined, and expresses exactly the manner in which the lines (unit tubes) of magnetic induction leave the surface. But to accuse it of misleading those who misunderstood it, is to put on the shoulders of Gauss and others who considered surface distributions a responsibility which is not theirs, and a blame which properly belongs to the perverse experimentalists who have insisted on determining the actual positions of "poles" in bar magnets, and on measuring other things equally indeterminate or non-existent.

But is the following statement quite free from possibility of misconstruction? "Gradually, however, new light dawned. It became customary, in spite of the mathematicians, to regard the magnetism of a magnet as something that traverses or circulates around a definite path, flowing more freely through such substances as iron than through other relatively non-magnetic materials." If any student of the subject does get into his head the idea that an actual material something flows round a magnetic circuit, he might quite as justly hold Dr. Thompson responsible for this false notion as blame the mathematicians for causing him to suppose a magnet to be a body plastered over with imaginary magnetic matter.

In tracing the evolution of the notion of magnetic permeability, Dr. Thompson might have made some mention of the contributions to molecular theory given by Poisson and others; for after all, these men really knew as well as we do "that magnetism, so far from residing in the end or surface of the magnet, is a property resident throughout the mass." But, as has already been stated, their attention was chiefly directed to the fields of permanent magnets; and to find a doctrine which "will afford a basis of calculation such as is required by the electrical engineer" is the business of the electrical engineer himself, and others interested in the proble m Hitherto, indeed, so far as such a doctrine has been found, it has been discovered in great measure by the physical mathematician! The mathematical doctrine o "magnetic permeability," or—to use Faraday's phrase—"conductivity for magnetic lines of force," was given so long ago as 1872 by Sir William Thomson, and fully illustrated by analogies and applications; and it is certainly curious that so accurate an historian as Dr. Thompson should, in his historical *résumé*, have made no mention of this very important paper. It is also the fact that our knowledge of the properties of iron, which, with the "simple law of the magnetic circuit," constitutes, according to Dr. Thompson, the stock-in-trade of the designers of dynamos, has come in no small degree from the same source. The most eminent investigator and improver of the dynamo is also a mathematician; but perhaps, as Sir William Hamilton (of Edinburgh, not of Dublin!) said of a certain mathematician who, he was forced to admit, reasoned correctly, he did it in spite of his mathematics, not because of them.

All honour to the great mathematicians who first attacked the difficult subject of magnetic action. But even if they had sinned so grievously by their unpracticality as it seems now the fashion to try to make out, their successors, entering into their labours, have been able to do much to atone by helping the engineer in the difficulties which beset his advance into a region, viewed indeed from afar, but hitherto untrodden. In this great work the engineer and mathematician are necessarily companions, and for either to reproach the other, as to what happened or did not happen in the past, is only to provoke useless recrimination, and delay their onward progress.

It is time now to come to the subject-matter proper of the book, and for this we have, on the whole, nothing but commendation. There are some things which might, perhaps, have been expressed differently with advantage, but this, of course, is only a matter of opinion. In the first chapter a very interesting account is given of the inventions and researches carried out by Sturgeon, Henry, and Joule, and ends with a description of notable electro-magnets. The sketch of Sturgeon's career, given in an appendix to this chapter, is of remarkable interest as a plain unvarnished record of heroic toil in the pursuit of knowledge, carried on amid uncongenial surroundings, and in spite of the hard pinch of poverty. In happier circumstances, this experimental genius would no doubt have done much for science. Still, to us there is some compensation, for, had it not been for the *res angusta domi* of his later years, we might not have had his "Annals of Electricity" or his "Scientific Researches." As it is, it seems well established that he was the pioneer of electro-magnetic discovery, the maker of the first electro-magnet and the first magneto-electric machine; and Dr. Thompson has done a simple act of justice in bringing Sturgeon's claims in this respect before the great public now interested in the progress of electrical science.

In chapter ii. we have generalities concerning electro-magnets and electro-magnetism, descriptions of typical forms of electro-magnets, and materials of construction. First are discussed such topics as the uses of electro-magnets, magnetic polarity, magnetic units, and elementary propositions in electro-magnetism. Here we would remark, as a merit in this work, the fact that the author has not loaded what is intended to be a thoroughly practical treatise with long discussions of purely theoretical matters, however important in relation to practice. The sketches of theory given in many works—which are really only collections of tables of numerical data, lists of formulæ, and workshop recipes—would be much better left out, and their places supplied by other matter, or the books lightened by their absence.

An excellent chapter follows, on the properties of iron. The various methods of measuring permeability are well explained, and for the space devoted to it a good account (with tables) is given of the results obtained by Hopkinson and Ewing in their researches. On p. 108 the effect of opening gaps in a magnetic circuit of iron is discussed, and a reference is given to an experiment described later in the work. On turning to this experiment (p. 212), it is found to be a description of an exploration of the effect produced at different parts of a horseshoe steel magnet by pulling off the keeper. A narrow coil of a few turns of wire is wound on a light frame capable of being slided round the magnet, and is connected with a ballistic galvanometer. The effect of pulling off the keeper is then tested with the coil in different positions on the horseshoe, and is shown by the deflection of the needle of the ballistic galvanometer. The theory of the author is, that putting on or taking off the keeper of a permanent steel magnet does not affect the magnetization at the middle of the horseshoe; that by putting on the keeper, and so diminishing the magnetic "reluctance" of the circuit, the lines of magnetic force are only collected, not altered in amount. Hence, if this theory be true, the coil, when placed at the middle of the magnet, should show no effect on the removal of the keeper. It is stated that careful and repeated experiments made at Finsbury gave an effect at the middle of the magnet which did not amount to 1/3000 of that found when the coil was close up to either end of the magnet.

Some time ago, when informed by a friend of this statement and result, the writer, feeling extremely doubtful of their general truth, had a magnet constructed for the purpose of repeating the experiments. As a large magnet was required for other purposes, one was constructed of eight steel bars of a mean length of about 3 feet. Each bar was 2 inches broad by ¼ inch thick, so that when put together they formed a large horseshoe of square cross-section 2 inches in side. A keeper, made of a block of soft iron, fitted between the ends of the horseshoe. The steel, which was tool steel obtained from a local firm, took magnetism readily, and an excellent horseshoe magnet was obtained.

A coil in circuit with a ballistic galvanometer was used in the manner described above, to test the effect of removing the keeper. Careful experiments made by students, and repeated by the writer, gave an entirely different result from that obtained by Prof. Thompson. A very large throw was obtained by placing the coil close to either end of the magnet and detaching the keeper; but with the coil as nearly as possible at the middle ot the horseshoe, the throw was about *one-eighth* of the maximum. It was verified, moreover, that the minimum throw was obtained at the middle.

This result was exactly what the writer had expected would in general happen. The so-called free magnetism at the extremities of the magnet, in the absence of the keeper, produced a demagnetizing effect throughout the magnet, and thus diminished the induction through the coil, even when at the middle. This action was counteracted by the magnetizing effect of the keeper when in position, and therefore itself inductively magnetized, but had full play as soon as the keeper was removed. The much greater deflections near the ends were undoubtedly due to the cause to which Dr. Thompson would assign the whole effect—the alterations of the arrangement of the lines near the ends which accompanied the removal and replacement of the keeper.

It is certain that this effect will depend on the permeability of the magnet steel, which is a function of the magnetization; but that such an effect will in general be produced there does not seem to be any room for doubt.

The following chapters deal with specially designed electro-magnets, such as, for example, those used in relays and clockwork, electro-magnetic mechanism, alternate-current electro-magnets, electro-magnetic motors and machine tools, and the purely electro-magnetic part of the book winds up with a very interesting chapter on the electro-magnet in surgery. The last chapter of all is devoted to permanent magnets. To give a satisfactory account of these chapters is here impossible; but it may be mentioned that the electro-magnetic mechanism fully described and figured includes no less than nine classes, beginning with the different forms of magnet with moving armature or plunger, magnets with armatures moving against counterpoises of different kinds, polarized devices, electro-magnetic vibrators, magnetic brakes, &c.

In chapter xi., on alternate-current electro-magnets, the modes of laminating magnets for the prevention of eddy-currents are described; then follows a discussion of effects of alternating electro-magnets, depending on the difference of the phase relations of the magnets and the eddy-currents excited in conductors in the form of disks and rings placed near the extremities of the iron cores. Thus we have a very interesting account of Elihu Thomson's remarkable experiments. The throttling or impedance effect of electro-magnets included in circuits is next treated. It would have been worth noticing, where the relations of maximum current, maximum electromotive force, mean current, mean electromotive force, and impedance are given, that the true mean value of the total electrical activity in an alternating circuit, in which work is done against resistance only, as in lighting glow-lamps, is equal to the mean square of the current multiplied by the total resistance in circuit.

In the chapter on the electro-magnet in surgery, Dr. Thompson, as was to be expected, gives a careful summary of the history of the subject. It seems that hitherto the principal use has been in ophthalmic surgery, for the extraction of small particles of iron from the eyeball. A very interesting case is narrated, in which the author's brother, Dr. Tatham Thompson, of Cardiff, successfully removed, by a magnetic probe constructed according to his own design, a fragment of iron which had passed through the eye of a blacksmith and lodged in the retina. The probe, excited by a coil, was inserted through the vitreous humour of the eye along the track of the wound, and catching the bit of steel, drew it from its resting-place in the retina. With the exception of a slight limitation of the field of vision, the patient perfectly recovered his sight.

The writer has a lively remembrance of being present some years ago at a search made, by means of a magnetic probe which he had arranged for one of the surgeons of Glasgow Infirmary, for a fragment of iron which was alleged to have entered the knee of a blacksmith. The blacksmith was exceedingly lame, and an examination of the knee by means of a magnetometer for signs of the presence of iron had resulted in some very puzzling indications. All iron, it was thought, had been removed from the patient, and with the knee in certain positions very decided indications of magnetized iron or steel had been observed. The probe, magnetized by a battery current, was inserted in an incision made at the place where the iron was supposed to have entered, but without effect. An adhesion was found to exist in the joint, and was broken down by the surgeon, with the result that after the patient's recovery the lameness had disappeared. It was found on stripping the patient that he wore a truss, and hence the magnetometer effects!

It seemed rather unlikely, in any case, that the magnetic probe would be effective in removing a splinter deeply embedded in muscular tissue.

The last chapter, on permanent magnets, though no part of the subject-matter proper of the book, is nevertheless very complete, and full of valuable information regarding the magnetizability of different kinds of steel, effects of temperature, use of laminated magnets, lifting power of magnets, and permanence of magnets with lapse of time.

The chapter concludes with a short sketch of astatic arrangements of needles. In connection with these, it may be remarked that the vertical pair of astatic needles—which has the advantage of perfect and permanent astaticism, inasmuch as each needle is astatic—was described, with other astatic arrangements, in a paper by Mr. T. Gray and the present writer, in the Proceedings of the Royal Society for 1884 (vol. xxxvi.).

Here we must take leave of a most interesting volume. The outside of the book is tastefully got up, the printing and paper are excellent, and the whole is worthy of the reputation of both author and publishers.

A. GRAY.

FUNGUS EATING.

British Edible Fungi: how to Distinguish and how to Cook them. By M. C. Cooke. (London: Kegan Paul, Trench, Trübner, and Co., 1891.)

THERE is a fascination in the minds of many people for the eating of fungi, which has often been expressed in popular books on the subject by authors of various degrees of power. Some of these are enthusiasts, who suffer palpably from that malady of intolerance which characterizes so many propagandists, who would persuade us that those things are best to eat and drink which they have found so; others, again, temper their recommendations by the calm arguments of the scientific man, though, speaking as having found, they must write with a tinge of that persuasiveness necessary to overcome wide-spread prejudice, or ignorance, if they are to be understood by the multitude; others, again, are content to state the facts, and let the logic of their sentences make its own impression in due course.

It is not easy to say off-hand in which of these categories the writer of the book under review should be classed, nor is it of much consequence to look at his writings according to the decision arrived at in that connection. It is certainly no more than fair to say that Mr. Cooke has compiled a little book of no small value as a guide to common edible fungi of this country; and that, while the information is singularly clear, and told in plain and homely language, it loses little or nothing in the simple telling, so far as the facts about these cryptogams are concerned. In so far, the author's well-known reputation as a mycologist is not likely to suffer; but it must be added that, while we do not pretend to criticize in detail all that relates to the cooking of these delicacies, and while it must always be more or less a matter of individual opinion whether *Champignons* stewed with pepper and butter, or *Boletus* with sauce of lemon-juice and powdered lump-sugar, are the more delicious, there is something that smacks of prejudice in the more than hearty commendation of some of the cooked favourites. Be this as it may, Mr. Cooke is unquestionably a high authority on his subject, and he has done good service in supplying the public with a well-written account of it, and with excellent coloured drawings of about forty of the chief forms of edible fungi.

Few people in this country are aware that nearly two hundred of the things called toadstools are at least edible, and fewer still will be prepared to believe that there are people in the world who regard some fifty-odd of these as dainties. Mushrooms, truffles, and morels exhaust the list for most Englishmen, and many are dubious about even these, and eat them, when served, with various degrees of trustful confidence, or the reverse.

It is a severe shock to these persons when they learn that such species as *Amanita rubescens*, *Lepiota procerus*, *Coprinus comatus*, and other "horrid toadstools," may not only be eaten, but are even extolled as delicious; and when it comes to recommending the green-topped *Clitocybe odorus*, smelling of anise, or the purple *Tricholoma nudus*, and other violently coloured species, the amateur may be pardoned for hesitating. Nevertheless, these and many other forms popularly held as dangerous are not only edible, but are also capable of distinction with a little practice. It may, perhaps, be doubted whether specialists do not commonly underrate the difficulties in diagnosis of these forms to an amateur, but Mr. Cooke can at least be felicitated on having made the distinguishing characters as clear as popular language admits.

In addition to the chapters on edible fungi, there is an interesting one on poisonous species, which may convince the reader of error in supposing that these are so numerous as is vulgarly supposed. The fact is, we know very little about the poisonous forms. Some few are thoroughly established as poisonous, and great care should be exercised by amateurs in view of this fact; but by far the majority of toadstools are simply not known to be edible. The following paragraph, from p. 208, may be worth quoting:—

"Over and over again have we been urged to lay down some rules, or instructions, whereby poisonous may be distinguished from innocuous fungi. As often have we declared, as we do now, that such general instructions are impossible. No rules can be given whereby a poisonous can be distinguished from a harmless species, nothing except knowledge and experience. The poisonous species already known are known because they have a past history which has condemned them, and not from any evidence written upon them. The most experienced mycologist cannot tell by any character, feature, or behaviour, that this or that fungus is poisonous or the reverse. He only knows its antecedents and the company it keeps. A large order of flowering plants, such as the *Solanaceæ*, may be looked upon with suspicion; but the potato and tomato are not poisons. In the *Agarics* the sub-genus *Amanita*, with warted caps, have always been regarded with suspicion because of *Agaricus muscarius* and *Agaricus phalloides*; but two others of the same group, *Agaricus rubescens* and *Agaricus strobiliformis*, are most excellent food."

This, in spite of traces of obscurity, may be taken as sound talk, showing an appreciation of the position by the author, who is, moreover, quite alive to what generalizations can be made as to the dangerous character of pink-spored genera, and the bright red forms of *Agaricus* and *Russula*, and so on; and we recommend this chapter of warnings to all who are inclined to taste rashly.

The printing of the text and illustrations is decidedly good, and we regard this little work as a gain to the popular naturalist's library.

"*EXTENSION*" *PSYCHOLOGY*.

Psychology: a Short Account of the Human Mind. By F. S. Granger, M.A. (London: Messrs. Methuen and Co., 1891.)

THIS little volume, which forms one of Messrs. Methuen's "University Extension Series," is not a mere compilation from larger works on psychology. Short as it is, it presents an independent development of the subject; and it is not infrequently characterized by a freshness and vigour most helpful to the student. We feel sure that Mr. Granger's short account of the human mind will be a most useful little book in the "psychology circles" of that National Home-Reading Union to the existence and work of which Dr. Hill drew attention in a recent issue of NATURE. And few subjects are better suited than psychology for study in this way.

Mr. Granger lays great stress on the essential unity of the wave of consciousness that in its onward course constitutes our mental life. By introspection (which is also retrospection) we may analyze the waves that have passed; we may find elements of cognition, of feeling, and of will; we may separate out dominant and subdominant ideas, sensations from without the body and from within. But in the wave as it passes all these are fused into one state of consciousness from which no one of the constituent elements could be omitted without destroying its identity. Students are only too apt to fancy that the elements reached by psychological analysis have a reality more real than the state of consciousness with which they started. Mr. Granger does his best to help them to avoid this fallacy.

Welcome as the little book is from its independence of treatment, it shows signs of hurried composition. Apart from bits of sheer carelessness—*e.g.* where Mr. Granger speaks of "the facts with which we have to deal in"—the statements are occasionally ambiguous or misleading "The retina, for example, may be supposed to transform all sorts of stimuli into sight impressions." That is not well put. "The causes which give rise to states of mind may be divided into states of mind and states of body. Thus the sensation of red is due to a light-stimulus received from some object by way of the eye. Here, a state of mind, the sensation of red, is caused by a state of body—namely, a change in the retina. All cases of this kind, in which a mental effect follows upon a physical cause, are instances of physical laws. On the other hand, states of mind are very often due to previous states of mind," &c. This, again, is awkward and misleading. The physical basis of a "sensation of red" is found in certain molecular changes in the brain; and these may be induced by changes in the retina (external origin), or by other changes in surrounding parts of the brain (internal origin). So far as the molecular changes which are the basis of the "sensation of red" are concerned, whether they be due to causes external to the brain or to causes within the brain, they are alike "instances of physical laws." Here, and elsewhere in the book, the fundamental distinction between energy and consciousness, between the physical and the psychical, is not brought out with sufficient clearness.

In his preface, Mr. Granger tells us that "the illustrations are largely drawn from every-day experiences and familiar books." If the following illustration correctly represents Mr. Granger's every-day experiences, Nottingham must be a somewhat dangerous place to live in. "A group of ideas may come into the mind, and by the process of association may bring up an idea of some movement or series of movements which is forthwith realized. The sight of a stranger leads to the idea, and then to the act, of throwing half a brick at him." Even if the experience is of every-day occurrence, it can hardly be regarded as a happy illustration. C. LL. M.

OUR BOOK SHELF.

Arithmetical Exercises in Chemistry. By Leonard Dobbin, Ph.D. With Preface by Prof. Crum Brown. (Edinburgh: James Thin, 1891.)

THIS book is a short collection of problems dealing with chemical changes, and such physical changes as are of special importance to junior students of chemistry Examples are given on the metric system, thermometric scales, specific gravity, the gaseous laws, the weights and volumes of materials entering into chemical reactions, and on the calculation of the empirical formulæ and percentage composition of compounds. As distinguished from others on the same subject, the book contains no more than the beginner requires, and is therefore less apt to confuse than one more elaborate. The introductory part of each section, giving the principles involved in the exercises, is clear, and the examples, 128 in all, seem well chosen. A table of contents, index, and answers, are included. A few points in connection with specific gravity might with profit be attended to. On p. 6, density, in reality absolute density, is defined, and then specific gravity. Relative density, as it is referred to on p. 31, might here be introduced, and the account would be more complete if the relation between density and specific gravity were clearly stated. After the formula $s = w/v$, on p. 9, it might be definitely pointed out that, with the units chosen, specific gravity is the weight of unit volume. It is not quite accurate to lead the student, as on p. 8, to infer that specific gravities are usually given for the temperature of 4° C.

The Colliery Manager's Hand-book. A Comprehensive Treatise on the Laying-out and Working of Collieries, designed as a Book of Reference for Colliery Managers, and for the use of Coal-mining Students preparing for First-class Certificates. By Caleb Pamely. Pp. 552, Index, and 472 Woodcuts. (London: Crosby Lockwood and Son, 1891.)

NO doubt colliery managers, and students preparing for examinations, would find it convenient to have a cyclopædia of mining, but the hand-book under consideration will scarcely satisfy their wants. The work is another illustration of the proverb, "Ne sutor ultra crepidam." Instead of being satisfied with describing mining processes and mining plant, the author deliberately plunges into geology and chemistry, and then finds himself quite out of his depth.

It is evident that much labour has been expended in bringing together information upon various matters connected with coal-mining; but there are gaps which require to be filled up, and errors that should be corrected in a second edition. For instance, the chapter upon boring is meagre. The part relating to percussive boring is in the main borrowed from Greenwell, and refers solely to boring by hand. Not a word is said about free-falling tools. It is not correct to say that "a great advantage of the diamond-drill boring is that the hole is kept true and vertical." The surveys made with the ingenious "clinograph" of Macgeorge have shown beyond a doubt that this is not invariably the case.

The description of tools is insufficient; and strange to say, the book contains no figure of a pick. Probably no work on mining was ever written before without a figure of this characteristic miner's implement. It is not for want of space, because there are figures to illustrate the manner of preparing oxygen and nitrous oxide. These are really unnecessary: if the student wants to learn a little elementary chemistry, he had better have recourse to one of the numerous text-books on that subject.

The examples of the different methods of working coal are likely to confuse students, owing to the mass of details by which they are accompanied. As the original articles are available in the Transactions of the Mining Institute of Scotland, short abstracts would have been quite sufficient.

Serious omissions are somewhat numerous. Rittinger pumps, which are doing such excellent work on the Continent, are not mentioned; and the following important subjects are also entirely ignored: coal-washing, coking, utilization of the gases for making tar and ammonia, and manufacture of patent fuel. In these days, when warning notes are being sounded concerning the duration of our coal supply, the attention of mining engineers and students should be specially directed to methods of turning small coal and inferior seams to profitable account. The author of a hand-book ought to be in advance of the times, and point out the path of economy and progress.

In spite of its defects, Mr. Pamely's work is by no means destitute of value. It contains a great deal of information which managers and students will find of use to them, and the excellent index will enable them to lay their hands at once on any part they desire to consult.

C. L. N. F.

Photography applied to the Microscope. By F. W. Mills. (London: Iliffe and Son, 1891.)

THE subject of photo-micrography is one of such absorbing interest that it is no wonder it has become so popular among amateurs. For those commencing, and for those who have already made some steps in pursuing this subject, the present little book is intended, and it will be found to contain just that sort of information and advice that is so necessary to a beginner. The main point to insure good photographic results lies in the preparation of the object, which requires both patience and care; the chapter dealing on this has been written by Mr. Charters White, who gives good directions for cutting hard and soft tissues, and for bringing materials, that are too soft in their natural condition, to a state of firmness previous to cutting. With regard to the choice of the necessary apparatus, the author describes various forms that are cheap, and which with care can be made to yield fair results. In the remaining chapters all the photographic manipulations are dealt with, such as exposing, developing, and printing, &c., and at the end is added a useful list of works which treat of the subject under consideration.

Copernic et la Découverte du Système du Monde. By Camille Flammarion. (Paris: Marpon and Flammarion, 1891.)

AN interesting book is this, and one full of information. In ten chapters and 250 pages M. Flammarion traces the history of astronomy from Copernicus to Newton, with special reference to the life of the former and the development of his system. There is little doubt that this work will be as well received as others by the same writer, and it really deserves the favour.

G.

Annals of British Geology, 1890. By J. F. Blake. (London: Dulau and Co., 1891.)

IT is intended that this shall be the first issue of an annual publication; and, if future volumes are prepared with as much care as the present one, the work ought to be of much service to geologists. Its scope is restricted to writings which have appeared in the United Kingdom. The author is not content with noting merely the titles of the works he records. When they are of the least importance, he gives a general idea of their contents, and presents what seems to him an adequate critical estimate of their value. The classification is by subjects. He begins with general geology; then come stratigraphical geology, palæontology, palæobotany, petrology, economics, maps and sections, and works relating to foreign geology, but published in Britain. A section headed "Personal Items" brings together a number of facts to which it may sometimes be convenient for the student of geology to refer. The volume deserves to be all the more cordially welcomed because Mr. Blake is not of opinion that he has at one stroke reached perfection. He hopes that future volumes may be improved by the co-operation of specialists in the several departments.

LETTERS TO THE EDITOR.

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

Warning Colours.

IN the experiments on "Comparative Palatability," recorded in NATURE of November 19 (p. 53), Mr. E. B. Titchener refers to the unpalatability of the brimstone butterfly. The insect was "fairly seized several times," but "was always rejected," by a frog. Some of your readers may not be aware that Mr. F. Gowland Hopkins, of Guy's Hospital, has recently shown that the yellow pigment of this butterfly, and of several others of its allies, is due to a substance formed as a urinary pigment; it is also known that the colours of other butterflies, and other animals, bear a relation to the urinary pigments. These substances may be in many cases of a disagreeable flavour. Dr. Eisig, of the Naples Zoological Station, has suggested that if intense and varied coloration is primarily due to a great quantity and variety of such bitter-tasting pigments, we do not need to assume that the brilliant coloration has been brought about in order to advertise the nauseous taste. The bright and varied colour will be, in fact, a consequence of the deposition in the integument of bitter pigments. This view—which has for the most part escaped the attention of those who have written upon animal colours, owing doubtless to its having been put forward in a special monograph upon a group of worms (*Capitellidæ*)—better explains how it is that brightly-coloured unpalatable creatures are in so many (? the majority of) cases tasted before being refused. I have laid some stress upon this view of warning coloration in a forthcoming book upon "Animal Coloration," which is to be published by Messrs. Swan Sonnenschein and Co. FRANK E. BEDDARD.

Zoological Society's Gardens, Regent's Park.

The Salts in Natural Waters.

THE communication of Mr. Lyons, in NATURE of November 12 (vol. xlv. p. 30), giving an analysis of the water of the salt lake of Aalia Paakai, affords a suitable opportunity for asking a question, to which, I trust, some chemist among the readers of NATURE will be able to give a satisfactory answer.

The usual analysis of the "solid constituents" of a given specimen of natural water only *directly* determines, I believe, the respective quantities of the metallic bases—sodium, calcium, &c.—and of the non-metallic constituents—chlorine, carbonic acid, &c.—contained in the "total solids." How does the chemist, then, proceed to mate these two classes of constituents together, so as to be able to state with confidence what salts, and in what quantities, are held in solution in the water? The problem itself would appear to be an *indeterminate* one, at any rate if there are more than two of either class of constituents. What additional considerations are introduced to render the problem determinate? Are they definite chemical conditions; or is there more or less arbitrariness in the assumptions made, so that two chemists would not necessarily arrive at the same result?

In the case of the Honolulu lake, there are, according to Mr. Lyons's analysis, three non-metallic constituents (*chlorine, bromine, sulphuric acid*)—(is not the absence of *carbonic acid* remarkable?)—and four metals (*sodium, potassium, calcium, magnesium*); the quantities of which have, I suppose, been obtained by direct analysis. From the *twelve* possible combinations of these constituents to form simple salts, *five* have been excluded, the sulphates of sodium and potassium, and the bromides of these metals and calcium also, thus reducing the number to *seven*, the quantities of which can, of course, be definitely determined from the *seven* direct data of the analysis. Is it *certain*, however, on assured chemical grounds, that none of these excluded salts are contained in the water, and if not, on what principle has their possible existence been ignored?

I write, as is evident, with but very slight knowledge of chemical analysis; and possibly answers to my questions are to be found in some text-book. If so, I should be obliged by a reference to any easily accessible work in which the question is discussed. R. B. H.

Mental Arithmetic.

THE following method of multiplying large numbers together mentally, if new, may interest some of your readers. If it has been published before, I should be glad to learn where it may be found. The process is so simple that, though I have no special gift for mental arithmetic, I was able, almost without practice, to multiply together correctly seven figures by seven, and to write down the result from left to right.

Suppose it is desired to multiply 123 by 456. The sum is usually written thus:—

$$
\begin{array}{r}
123 \\
456 \\
\hline
738 \\
615 \\
492 \\
\hline
56088
\end{array}
$$

If, instead of completing each step in the multiplication as we arrive at it, and carrying the tens to the left, the digits are merely connected by the multiplication sign and written down in their proper places, the result is:—

$$
\begin{array}{ccccc}
 & & 1 & 2 & 3 \\
 & & 4 & 5 & 6 \\
\hline
 & & 1\times 6 & 2\times 6 & 3\times 6 \\
 & 1\times 5 & 2\times 5 & 3\times 5 & \\
1\times 4 & 2\times 4 & 3\times 4 & & \\
\hline
1\times 4 & (1\times 5+2\times 4) & (1\times 6+2\times 5+3\times 4) & (2\times 6+3\times 5) & 3\times 6
\end{array}
$$

If the figures in the lowest line are multiplied out and the tens carried to the left in the usual way, the result is, of course, the same as that given by the ordinary procedure. Thus, to obtain the first figure, beginning at the right, we say: "$3 \times 6 = 18$;—8 and carry 1." To obtain the second figure: "$2 \times 6 = 12$; $3 \times 5 = 15$; $12 + 15 + 1$ (which has been carried) $= 28$;—8 and carry 2." And so on. Thus each figure of the answer can be obtained by multiplying together certain digits of the multiplier and multiplicand, and adding the amount to be carried from the calculation of the previous figure, without the strain of remembering all the horizontal rows of results and their relative positions vertically. It remains only to show which digits of the multiplier and multiplicand must be combined. A consideration of the example worked out above will show that, to obtain the first figure of the answer, we multiply the 1st digit (from the right) of the multiplier (6) by the 1st of the multiplicand (3). To obtain the second, we multiply the 1st of the multiplier by the 2nd of the multiplicand and the 2nd of the multiplier by the 1st of the multiplicand (*i.e.* the first two of each line) crosswise, and add the products. Similarly, the third figure is obtained by multiplying the first three digits of each line crosswise, *i.e.* 1st by 3rd, 2nd by 2nd, and 3rd by 1st, and adding the products. The number of digits employed in the process is now at a maximum, and begins to diminish. To obtain the fourth figure, we multiply together crosswise all the digits except the first of each line, *i.e.* the group $\begin{smallmatrix}12\\45\end{smallmatrix}$. And to obtain the last figure, we multiply all except the first two of each line, *i.e.* the group $\begin{smallmatrix}1\\4\end{smallmatrix}$.

If the number of digits in the multiplier is less than that in the multiplicand, the procedure is the same till all the digits of the multiplier are used in the combination. For each successive figure, the group of digits in the multiplicand to be used shifts along one place to the left till it comes to the end. The group then diminishes as before, by dropping the right-hand digit in each line. For example: the groups, the digits of which are multiplied together crosswise, in multiplying 123456 by 789, are as follows:—

Digits—	8th.	7th.	6th.	5th.	4th.	3rd.	2nd.	1st.
	1	12	123	234	345	456	56	6
	7	78	789	789	789	789	89	9

It will be found, on trial, that this method is quite easy, and can be accomplished by anyone who can add together in his head the products of two digits, and can remember the string of figures which form the answer. This is most easily done by repeating the whole series from left to right, as each successive figure is arrived at.

The power of multiplying as many as seven figures by seven is not likely to be of much practical value, and when carried to this stage the method is merely a curiosity; but it may be of use in helping us to multiply together small amounts, consisting of two or three digits, which lie beyond the scope of the multiplication table.

I will conclude with a short example, showing what passes through the mind in working the method.

Multiply 987 by 654.

987
654

First figure.—$4 \times 7 = 28$. Eight and carry two. Answer (so far) 8

Second figure.—$4 \times 8 = 32 + 2$ (carried) $= 34 + 5 \times 7 (= 35) = 69$. Nine and carry six. Answer (so far) 98

Third figure.—$4 \times 9 = 36 + 6$ (carried) $= 42 + 5 \times 8 (= 40) = 82 + 6 \times 7 (= 42) = 124$. Four and carry twelve. Answer (so far) 498

Fourth figure.—$5 \times 9 = 45 + 12$ (carried) $= 57 + 6 \times 8 (= 48) = 105$. Five and carry ten. Answer (so far) 5498

Fifth and sixth figures.—$6 \times 9 = 54 + 10$ (carried) $= 64$. Answer 645498

CLIVE CUTHBERTSON.

A Rare Phenomenon.

I HAVE read with much interest the accounts of "the rare phenomenon" observed by several of your correspondents (published in NATURE, vol. xliv. pp. 494, 519), as I noticed a similar appearance here in Nova Scotia, at about the same time (September 11).

A narrow ray—apparently of auroral light—spanned the whole heavens from east to west, passing overhead a little to the south of the zenith. There was little or no display of auroral light in the north at the time.

A "harvest-home" was held here on September 11, and I noticed the appearance, I think, the same evening about 11 o'clock.

A number of persons in the town of Baddeck observed the same or a similar phenomenon "very shortly before September 12."

ALEXANDER GRAHAM BELL.

Beinn Bhreagh, near Baddeck, Cape Breton, N.S., November 6.

HENRY NOTTIDGE MOSELEY, F.R.S.

I HAVE been asked to write for the readers of NATURE some account of my dear friend Moseley, who, after an illness which removed him from all active life and work for more than four years, died at Clevedon, in Somersetshire, on November 10. He was only forty-seven years of age; and when seized with the illness which necessitated his retirement from active life, was at the zenith of a wonderful career of scientific productiveness and value. He had for six years held the Linacre Professorship of Human and Comparative Anatomy in the University of Oxford; and by his great energy and commanding talent had succeeded in collecting around him a most promising band of younger men devoted to the investigation of embryological and morphological problems. Baldwin Spencer, Gilbert C. Bourne, S. J. Hickson, and G. Herbert Fowler, were his pupils, and have shown by their numerous published works the value of the teaching and impulse which he gave to them. In the early days of his illness (1887), he was cheered by receiving from the Royal Society the Royal Medal, in recognition of the value of his researches on Peripatus, the Hydrocorallinæ, the Land Planarians, and the Chitons. The blow caused by his serious illness was felt not only in the scientific and social life of Oxford, but in many other centres. We missed his valuable and practical help in carrying to completion the Plymouth Laboratory of the Marine Biological Association, of which he had been a most active and enthusiastic promoter; in the editorship of the *Quarterly Journal of Microscopical Science* I found myself once more deprived of the aid of my most valued comrade, as I had been but a few years previously when Frank Balfour died. The readers of NATURE and of the *Athenæum* missed his varied and always strongly-original contributions; and the Zoological, Anthropological, and Royal Societies had to regret his absence from their meetings and Councils. Moseley had, moreover, at this time made it a practice to give evening lectures in the larger provincial towns as well as in London: from all quarters came expressions of the deep regret which his retirement from public work excited. The amount and variety of work in which he engaged, in addition to the remarkable and extraordinarily minute course of lectures and laboratory work provided by him for his pupils, were certainly more than was wise for him to undertake. But it was a strange and to him a disastrous fact that he never felt tired. He was an exceedingly strong man, and I never saw him fatigued either by physical or mental exertion.

We made acquaintance in Rolleston's laboratory at the Oxford University Museum in 1866, and became fast friends and constant companions. Moseley's father was a distinguished mathematician and Canon of Bristol, Rector of Olvaston, near the Severn. Here, when I was staying with Moseley in 1871, we dissected and prepared the skeleton of a huge grampus which is now in the Oxford Museum; the carcass had made a tour of the neighbouring villages for three weeks before we obtained possession of it. Moseley was at school at Harrow, where he chiefly occupied himself in birds'-nesting and "bug-hunting," in conjunction with a small band of kindred spirits. He was essentially a sportsman, knew every kind of game and how to pursue it. He thoroughly disliked the ordinary routine of school work, such as it was in those days, and it was not until he had entered at Exeter College, and come under the teaching of the late Prof. Rolleston, that his really keen and remarkable intellectual powers began to show themselves. He had somehow developed in early youth the most deep-rooted scepticism which I ever came across among men of my own age; hence it was the *reality* of the work which he did in the dissecting-room at the Museum which delighted him and gave him confidence that there was "something in it" worthy of his intellectual effort. With unfeigned astonishment he would say, on dissecting out the nervous system of a mollusk or some such structure, "It is like the picture, after all!" He had a profound disbelief in the statements made in books unless he could verify them for himself, and it was this habit of mind, perceived and encouraged by Rolleston, which made him in after life so admirable an observer and so successful as a discoverer of new facts. Rolleston used to say that you had only to put down Moseley on a hill-side with a piece of string and an old nail, and in an hour or two he would have discovered some natural object of surpassing interest. He took great interest in all games, and was himself a first-rate racket-player. In the vacations he got a fair amount of shooting, and spent one "long" shooting and fishing in Norway. In the summer of 1867 we visited the Channel Islands together, for the purpose of studying marine animals. Whilst in Sark, after I had left him, he made the acquaintance of an American painter named Dix, and discovered himself to be no mean artist, bringing back a number of really clever water-colours, his first attempts in that direction. At this time and throughout his life, those who met him were struck by his singularly soft and agreeable voice, and by his great courtesy and power of interesting, I may even say fascinating, the most unpromising and unlikely of the companions amongst whom he chanced to find himself—I mean stiff old gentlemen and demure old ladies. To companions of his own age he was fond of adopting the free discourse and chaff of school-boy days. His friendship was like that of the explorers and prairie-hunters of whom he loved to read—absolutely staunch. If you had the good fortune to be his "chum," he would stand by you through thick and thin, and share all he had with you. I do not think there was any limit to what he would have done for his friend. We took our degrees together in 1868; and in the following spring—he having been elected Radcliffe Travelling Fellow, and I Burdett-Coutts Scholar—we spent six weeks in the Auvergne and the country between that and Marseilles. In the following winter (February 1870) we took up our quarters together at Vienna, and studied with Stricker, and in Rokitanski's laboratory. He entered, on our return, at University College, London, as a student of the Medical Faculty. In 1871, after his winter medical session, he joined me at Leipzig, where his great abilities were discerned and thoroughly appreciated by Prof. Ludwig, in whose laboratory we had the privilege of working. His first scientific memoirs were published whilst he was here—one, on the nerves of the cornea of mammals, as shown by the gold method (then not so familiar as it is now), and one on the circulation in the wing of the cockroach.

In the autumn of the same year, Moseley went, as member of the Government Eclipse Expedition, to Ceylon, under Mr. Norman Lockyer, whilst I joined Anton Dohrn at Naples. Moseley made valuable spectroscopic observations of the eclipse at Trincomali, and also brought home a large booty of Land Planarians, which he at once studied by means of sections, going to Oxford for the purpose of using the laboratory and the library attached to the Museum. This admirable piece of work delighted Rolleston, who communicated it to the Royal Society; it was published in the Philosophical Transactions after Moseley had sailed on the *Challenger*, as one of the naturalists of the Expedition, in December 1872. We did not see him again until May 1876, but I had frequent letters from him, and sometimes a small parcel, or some photographs. Of the scientific staff of the Expedition, Wyville Thomson and Suhm are dead, as well as Moseley; John Murray and J. Y. Buchanan are the two survivors. Moseley, although not a botanist, undertook the collecting of plants whenever the Expedition touched land; he also made important anthropological studies on the Admiralty Islanders, and has published a wonderful mass of notes and observations, accompanied by plates and woodcuts, in his "Notes of a Naturalist on the *Challenger*." He showed the stuff he was made of very soon after the Expedition started, viz. on the arrival of the *Challenger* at the Cape. He immediately started off in quest of Peripatus—a strange, imperfectly described beast which we had discussed together over some spirit specimens of it which I had received from Roland Trimen, of Cape Town. Moseley had made up his mind before he left England to "tackle" Peripatus, and he did so. He obtained living specimens, discovered the tracheæ and the most important features in the development, showing that the "jaws" are in-turned parapodia—and sent home a memoir which was at once published in the Philosophical Transactions. In the later part of the voyage he was occupied with the corals, and especially the Millepores and Stylasterids. The wonderfully elaborate plates, and the discovery they embodied, necessitating the formation of a new group of animals, the Hydrocorallinæ, were the first-fruits of his voyage which he produced on landing in 1876. During his absence both his father and his mother had died. His old College, Exeter—where I became a Fellow in the year of the *Challenger's* departure—now was inspired through the good offices of an eminent Greek scholar, with the happy thought of offering Moseley a Fellowship and a home in the College, so that he found on landing a welcome awaiting him, and a place in which to store for a while his treasures. I do not think that a College Fellowship was ever better bestowed: that was in the good old days before Lord Selborne's Commission. In his rooms in Exeter, Moseley displayed his Japanese and Melanesian curiosities, and wrote many papers embodying the observations made during his voyage, besides the book above mentioned. He was elected F.R.S. in 1879, and after a visit to Oregon (of which he published an account) was appointed (1879) Assistant Registrar of the University of London. He took up his residence in Burlington Gardens, but not for long. In 1881 he married the youngest daughter of Mr. J. Gwyn Jeffreys, F.R.S., the distinguished conchologist, and in the same year was elected, on the death of his teacher and close friend, Prof. Rolleston, to the Linacre Professorship in the University of Oxford.

Moseley had had no previous experience in teaching, but he set to work with that unbounded energy and strength which characterized him. He spared no pains to make his lectures absolutely up to date, and arranged a thorough laboratory course extending over two years to illustrate them. The regulations of the University as to examinations and curriculum were at that time not unfavourable to the study of animal morphology, and Moseley usually had ten or a dozen serious students besides the elementary class. Lincoln, University, and New Colleges encouraged his and their efforts by offering and awarding Fellowships to students of the University distinguished in animal morphology; and after six years all was progressing as satisfactorily as possible, when he was attacked by illness which brought his work to an end. Not only was he unable to carry on his work, but his absence naturally enough was unfavourable to the interests of those studies which he would have fostered and guarded, had he been able to take part in the legislation of the University.

During the happy and busy six years which Moseley spent as Linacre Professor at Oxford, he trained Bourne, Hickson, and Fowler to carry on his coral work; with Baldwin Spencer he investigated the pineal eye of Lacertilia, and himself published his remarkable discovery of eyes and other sense-organs in the shells of Chitonidæ. He was largely instrumental in securing the Pitt-Rivers collection of anthropological objects for the University, and superintended the preliminary arrangement of the collection in the building erected for it. He served twice on the Council of the Royal Society, was a founder and member of Council of the Marine Biological Association, and was President of Section D of the British Association at the Montreal meeting.

His love of travel was shared by his wife, who went with him from Montreal to Arizona to visit the town-building Indians of that remote region, and who, only a year before his illness, accompanied him on an Easter holiday trip to Tangier and Fez. During his illness she has been his constant companion. He leaves, besides her, two daughters and a son.

E. RAY LANKESTER.

ON THE VIRIAL OF A SYSTEM OF HARD COLLIDING BODIES.

A RECENT correspondence has led me to examine the manner in which various authors have treated the influence of the finite size of molecules in the virial equation, and I should like to lay a few remarks upon the subject before the readers of NATURE.

To fix the ideas, we may begin by supposing that the molecules are equal hard elastic spheres, which exert no force upon one another except at the instant of collision. By calling the molecules hard, it is implied that the collisions are instantaneous, and it follows that at any moment the potential energy of the system is negligible in comparison with the kinetic energy.

If the volume of the molecules be very small in comparison with the space they occupy, the virial of the impulsive forces may be neglected, and the equation may be written

$$pv = \tfrac{1}{3}\Sigma m V^2, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1$$

where p is the pressure exerted upon the walls of the inclosure, v the volume, m the mass, and V the velocity of a molecule.

In his essay of 1873 Van der Waals took approximate account of the finite size of the molecules, using a peculiar process to which exception has been taken by Maxwell and other subsequent writers. It must be said, however, that this method has not been proved to be illegitimate, and that at any rate it led Van der Waals to the correct conclusion—

$$p(v - b) = \tfrac{1}{3}\Sigma m V^2, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

in which b denotes four times the total volume of the spheres. In calling (2) correct, I have regard to its character as an approximation, which was sufficiently indicated by Van der Waals in the original investigation, though perhaps a little overlooked in some of the applications.

In his (upon the whole highly appreciative) review of Van der Waals's essay, Maxwell (NATURE, vol. x. p. 477, 1874) comments unfavourably upon the above equation, remarking that in the virial equation v is the volume of the vessel and is not subject to correction.[1] "The effect of the repulsion of the molecules causing them to act like elastic spheres is therefore to be found by calculating the virial of this repulsion." As the result of the calculation he gives

$$pv = \tfrac{1}{3}\Sigma m V^2\left\{1 - 2\log\left(1 - 8\frac{\rho}{\sigma} + \frac{17\rho^2}{\sigma^2} - \ldots\right)\right\}, \quad . \quad (3)$$

where σ is the density of the molecules, and ρ the mean density of the medium, so that $\rho/\sigma = b/4v$. If we expand the logarithm in (3), we obtain as the approximate expression, when ρ/σ is small,

$$pv = \tfrac{1}{3}\Sigma m V^2(1 + 4b/v), \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

or, as equally approximate,

$$p(v - 4b) = \tfrac{1}{3}\Sigma m V^2, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

which does *not* agree with (2).

The details of the calculation of (3) have not been published, but there can be no doubt that the equation itself is erroneous. In his paper of 1881 (*Wied. Ann.*, xii. p. 127), Lorentz, adopting Maxwell's suggestion, investigated afresh the virial of the impulsive forces, and arrived at a conclusion which, to the order of approximation in question, is identical with (2). A like result has been obtained by Prof. Tait (Edin. Trans., xxxiii. p. 90, 1886).

It appears that, while the method has been improved, no one has succeeded in carrying the approximation beyond the point already attained by Van der Waals in 1873. But a suggestion of great importance is contained in Maxwell's equation (3), numerically erroneous though it certainly is. For, apart from all details, it is there implied that the virial of the impacts is represented by $\tfrac{1}{3}\Sigma m V^2$, multiplied by some function of ρ/σ, so that, if the volume be maintained constant, the pressure as a function of V is proportional to $\Sigma m V^2$. The truth of this proposition is evident, because we may suppose the velocities of all the spheres altered in any constant ratio, without altering the motion in any respect except the scale of time, and then the pressure will necessarily be altered in the square of that ratio.

It will be interesting to inquire how far this conclusion is limited to the suppositions laid down at the commencement. It is necessary that the collisions be instantaneous, in relation, of course, to the free time. Otherwise, the similarity of the motion could not be preserved, the duration of a collision, for example, bearing a variable ratio to the free time. On the same ground, vibrations within a molecule are not admissible. On the other hand, the limitation to the spherical form is unnecessary, and the theorem remains true whatever be the shape of the colliding bodies. Again, it is not necessary that the shapes and sizes of the bodies be the same, so that application may be made to mixtures.

In the theory of gases, $\Sigma m V^2$ is proportional to the absolute temperature; and whatever doubts may be felt in the general theory can scarcely apply here, where the potential energy does not come into question. So far, then, as a gas may be compared to our colliding bodies, the relation between pressure, volume, and temperature is

$$p = T\phi(v), \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

where $\phi(v)$ is some function of the volume. When v is large, the first approximation to the form of ϕ is

$$\phi(v) = \frac{A}{v}.$$

In the case of spheres, the second approximation is

$$\phi(v) = \frac{A}{v} + \frac{Ab}{v^2},$$

where b is four times the volume of the spheres.

Thus far we have supposed that there are no forces between the bodies but the impulses on collision. Many and various phenomena require us to attribute to actual molecules an attractive force operative to much greater distances than the forces of collision, and the simplest supposition is a cohesive force such as was imagined by Young and Laplace to explain capillarity. We are thus led to examine the effect of forces whose range, though small in comparison with the dimensions of sensible bodies, is large in comparison with molecular distances. In the extreme case, the influence of the discontinuous distribution of the attractive centres disappears, and the problem may be treated by the methods of Laplace. The modification then required in the virial equation is simply to add[1] to p a term inversely proportional to v,[2] as was proved by Van der Waals; so that (6) becomes

$$p = T\phi(v) - av^{-2}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

According to (7), the relation between pressure and temperature is *linear*—a law verified by comparison with observations by Van der Waals, and more recently and extensively by Ramsay and Young. It is not probable, however, that it is more than an approximation. To such cases as the behaviour of water in the neighbourhood of the freezing-point it is obviously inapplicable.

In their discussions, Ramsay and Young employ the more general form—

$$p = T\phi(v) + \chi(v); \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

and the question arises, whether we can specify any generalization of the theoretical conditions which shall correspond to the substitution of $\chi(v)$ for av^{-2}. It would seem that, as long as the only forces in operation are of the kinds, impulsive and cohesive, above defined, the result is expressed by (7); and that if we attempt to include forces of an intermediate character, such as may very probably exist in real liquids, and must certainly exist in solids, we travel beyond the field of (8), as well as of (7). It may be remarked that the equation suggested by Clausius, as an improvement on that of Van der Waals, is not included in (8).

[1] In connection with this it may be worth notice that for motion *in one dimension* the form (2) is *exact*.

[1] It thus appears that, contrary to the assertion of Maxwell, p *is* subject to correction. It is pretty clear that he had in view an attraction of much smaller range than that considered by Van der Waals.

Returning to the suppositions upon which (7) was founded, we see that, if the bodies be all of one *shape*, *e.g.* spherical, the formula contains only two constants—one determining the size of the bodies, and the second the intensity of the cohesive force; for the mean kinetic energy is supposed to represent the temperature in all cases. From this follows the theorem of Van der Waals respecting the identity of the equation for various substances, provided pressure, temperature, and volume be expressed as fractions of the critical pressure, temperature, and volume respectively. If, however, the *shape* of the bodies vary in different cases, no such conclusion can be drawn, except as a rough approximation applicable to large volumes. RAYLEIGH.

Terling Place, Witham, November 18.

THE IMPLICATIONS OF SCIENCE.[1]

II.

I MIGHT now at once return to further consider those implications of science to which I have called your attention, but I think it will be better to first briefly pass two important matters in review.

The first concerns our means of investigation as to such fundamental questions.

The second relates to our ultimate grounds for forming judgments about them. We have to consider how fundamental truth can be acquired and tested.

Evidently the only means of which we can make use are our *thoughts*, our reason, our intellectual activity. "Thoughts" may be, and should be, carefully examined and criticized; but however much we may do so, and whatever the results we arrive at, such results can only be reached by thoughts, and must be expressed by the aid of our thoughts. This will probably seem such a *manifest* truism that I shall be thought to have committed an absurdity in enunciating it. To suppose that by any reasoning we can come to understand what we can never think, may seem an utterly incredible folly; yet at a meeting of a Metaphysical Society, in London, a speaker, not long ago, expressly declared "thought" to be a misleading term, the use of which should be avoided.

Now I am far from denying that unconscious activities of various different orders take place in our being, yet whatever influence such activities may have they cannot affect our judgments save by and in thoughts.

If a man is convinced that thoughts are worthless tools, he can only have arrived at that conclusion by using the very tools he declares to be worthless. What, then, ought his conclusion to be worth even in his own eyes?

It is simply impossible by reason to get behind or beyond conscious thought, and our thoughts are and must be our only means of investigating problems however fundamental.

Even in investigating the properties of material bodies, it is to self-conscious reflective thought that our final appeal must be made.

For it is to our thoughts, and not to our senses only, that our ultimate appeal must be made, even with respect to the most material physical science matters.

Some persons may imagine that with respect to investigations about the properties of material bodies, it is to our sensations alone that we must ultimately appeal. But it is not so; anyone would be mad to question the extreme importance, the absolute necessity, of our sensations in such a case. Nevertheless, after we have made all the observations and experiments we can, how can we know we have obtained such results as we may have obtained, save by our self-conscious thought? By what other means are we to judge between what may seem to be the conflicting indications of different sense impressions?

Our senses are truly tests and causes of certainty, but not *the* test. Certainty belongs to thought, and self-conscious reflective thought is our ultimate, absolute criterion.

As to the ultimate grounds on which our judgments respecting such problems must repose, as Mr. Arthur Balfour has forcibly pointed out, that it is a question altogether distinct from that of the origin of our judgments, or from reasonings about their truth. Such matters are very interesting, but they are not here in point, since it is plain that no proposition capable of proof can be one the certainty of which is fundamental. For, in order to prove anything by reasoning, we must show that it necessarily follows as a consequence from other truths, which therefore must be deemed more indisputable. But the process must stop somewhere. We cannot prove everything. However long our arguments may be, we must at last come to ultimate statements, which must be taken for granted, like the validity of the process of reasoning itself, which is one of the implications of science. If we had to prove either the validity of that process or such ultimate statements, then either he must argue in a circle, or our process of proof must go on for ever without coming to a conclusion, which means there could be no such thing as "proof" at all.

Therefore the "grounds of certainty" which any fundamental proposition may possess cannot be anything *external* to it—which would imply this impossible proof. The only ground of certainty which an ultimate judgment can possess is its own *self-evidence*—its own manifest certainty *in and by itself*. All proof, all reasoning, must ultimately rest upon truths which carry with them their own evidence, and do not therefore need proof.

It is possible that some of my hearers may be startled at the suggestion of believing anything whatever on "its own evidence," fancying it is equivalent to a suggestion that they should believe anything *blindly*. This, I think, is due to the following fact of mental association. The immensely greater part of our knowledge is gained by us indirectly—by inference or testimony of some kind.

We commonly ask for some proof with regard to any new and remarkable statement, and no truths are brought more forcibly home to our minds than are those demonstrated by Euclid. Thus it is that many persons have acquired a feeling that to believe anything which cannot be proved, is to believe *blindly*. Hence arises the tendency to distrust what is above and beyond proof. We are apt to forget, what on reflection is manifest—namely, that if it is not blind credulity to believe what is evident to us by means of something else, it must be still less blind to believe that which is directly evident in and by itself.

And self-conscious reflective thought tells *me* clearly, that the law of contradiction is not only implied by all science, and necessary to the validity of all science, but that it is, as I said, an absolute, necessary truth which carries with it its own evidence. It must be a truth, then, applicable both to the deepest abyss of past time and the most distant region of space. But here, again, I think it possible that one or two of my hearers may be startled, and perhaps doubting how things in this respect may be in the Dog-star now, or how they were before the origin of the solar system. I fancy I hear someone asking: "How is it possible that we, mere insects, as it were, of a day, inhabiting an obscure corner of the universe, can know that anything is and must be true for all ages and every possible region of space?"

In the first place, I think the difficulty which may be thus felt is due to the abstract form of the law of contradiction. And yet, as I said before, it is but the summing up of all the particular instances, as to each one of which no difficulty at all is felt, but each is clearly

[1] Friday Evening Discourse delivered at the Royal Institution by Dr. St. George Mivart, on June 5, 1891. Continued from p. 62.

seen to be true. Any man who really doubted whether, if his legs were cut off, they might not at the same time remain on, would have a mind in a diseased condition.

There is, however, another reason which indisposes some persons to see the necessary force of this law. It is due, I think, to a second fact of mental association.

Things which are very distant, or which happened a long time ago, are known to us only in roundabout ways, and we often feel more or less want of certainty about them. On the other hand, we have a practical certainty concerning the things which are about us at any given moment. Thus we have come to associate a feeling of uncertainty with statements about things very remote. But nothing can well be more remote from us than "the most distant regions of space" or "before the origin of the solar system." It is not surprising, then, that this mental association should call forth a feeling of uncertainty with respect to any statement about universal truth.

It is, no doubt, wonderful that we should be able to know any necessary and universal truths; but it is less exceptionally wonderful, when we come to think the matter all round, than it may at first sight appear to be. It is wonderful; but so, deeply considered, is all our knowledge. It is wonderful that through molecular vibrations, or other occult powers of bodies, we have sensations—such as of musical tones, sweetness, blueness, or what not. It is wonderful that through sensations, actual and remembered, we have perceptions. It is wonderful that on the occurrence of certain perceptions we recognize our own existence past and present. So, also, it is wonderful that we recognize that what we know "*is*," cannot at the same time "not be." The fact is so, and we perceive it to be so; we know things, and we know that we know them. HOW we know them is a mystery, indeed, but one about which it is, I think, perfectly idle to speculate. It is precisely parallel to the mystery of sensation. We feel things savoury, or odorous, or brilliant, or melodious, as the case may be, and with the aid of the scalpel and the microscope we may investigate the material conditions of such sensations. But *how* such conditions can give rise to the feelings themselves is a mystery which defies our utmost efforts to penetrate. I make no pretension to be able to throw any light upon the problem "How is knowledge possible?" any more than on the problem "How is sensation possible?" or on the questions "How is life possible?" or "How is extension possible." But "*Ignorantia modi non tollit certitudinem facti.*" And we know that we are living, that we feel, and that we do know something—if only that we know we doubt about the certainty of our knowledge.

And *à propos* of such doubt, let me here put before you the intellectual penalties which have to be paid for any *real* and *serious* doubt with respect to the implications of science. I think we shall see that nothing less than intellectual suicide or mental paralysis must be the result. And such a result must also be logically fatal to every branch of science. The first implication I put before you was the *validity of inference*.

Now, no one who argues, or who listens to or reads—with any serious intention—the arguments of others, can, without stultifying himself, profess to think that no process of reasoning is valid. If the truth of no mode of reasoning is certain, if we can make no certain inferences at all, then all arguments must be useless, and to proffer, or to consider, them must be alike vain. But not only must all reasoning addressed to others be thus vain, the silent reasoning of solitary discursive thought must be vain also. Yet what does this amount to save an utter paralysis of the intellect? It is scepticism run mad.

But the implication I regard as one of the most important of all is the implication of *our knowledge of our own continuous existence*, concerning which I said I must crave your permission to speak at some length. It was the mention of this implication which led me to refer to that system of thought it is my object here to controvert.

I have heard it proclaimed in this theatre by Prof. Huxley that we cannot have supreme certainty as to our own continuous existence, and that such knowledge is but secondary and subordinate to our knowledge of our present feelings or "states of consciousness."

Of course I am not thus accusing him of *originating* any such erroneous view. In that matter he is but a follower of that daring and playful philosopher Hume. I say "playful," because I cannot myself think that he really believed his own negations. He seems to me too acute a man to have been himself their dupe. But however this may be, I here venture directly to contradict Hume's and Prof. Huxley's affirmation, which is also adopted by Mr. Herbert Spencer, and to affirm that we have the *highest* certainty as to our own continuous existence.

It is, of course, quite true that we have complete certainty about our present feelings, as also that we cannot know ourselves apart from our feelings. But it is no less true that we cannot be conscious of feelings apart from the "self" which has those feelings. Now, it is assumed by those I oppose that we can know nothing with absolute certainty unless we know it by itself or "unmodified," or as existing "*absolutely*." But in fact nothing, so far as we know, exists apart from every other entity and unmodified—or "*absolutely*," as it is, in my opinion, absurdly called. No wonder, then, if we do not know things in a way in which they never do, and probably never can, exist. We can really know nothing by itself because nothing exists by itself. It is not wonderful, then, if we only know ourselves as related to our simultaneously known feelings, or *vice versâ*.

It is quite true that we never know our own substantial essential being alone and unmodified, but then we have never for an instant so existed. Our knowledge of ourselves in this respect is like our knowledge of anybody and everybody else. Most persons here present doubtless know Prof. Tyndall, yet they never knew him, no one ever knew him, except in some "state"—either at home or away from home, either sitting or not sitting, either in motion or at rest, either with his head covered or uncovered—and this for the very good and obvious reason that he never did or could exist for a moment save in some "state." But this does not prevent your knowing him very well, and the same consideration applies to our knowledge of ourselves. When I consider what is my primary, direct consciousness at any moment, I find it to be neither a consciousness of a "state of feeling" nor of my "continuous existence," but a consciousness of doing something or having something done to me—action or reaction. I have always, indeed, some "feeling" and also some sense of my "self-existence"; but what I perceive primarily, directly, and immediately is neither the "feeling" nor the "self-existence," but some concrete actual doing, being, or suffering then experienced. We can, indeed, become distinctly and explicitly aware of either the "feeling" or the "self-existence" by turning back the mind upon itself. But to know that one "has a feeling" or is in a "state," or even that a "feeling exists," is plainly an act by which no one begins to think. It is evidently a secondary act—an act of reflection. No one begins by perceiving his perception a bit more than he begins by expressly adverting to the fact that it is he himself who perceives it.

Let us suppose two men to be engaged in a fencing match. Each man, while he is parrying, lunging, &c., has his "feelings" or "states," and knows that it is "he" who is carrying on the struggle. Yet it is neither his "*mental states*" nor the "*persistence of his being*" which he directly regards, but his concrete activity—what he is doing and what is being done to him. He may, of course, if he chooses, direct his attention either to the feelings he is experiencing or to his underlying continuous personality. Should he do so, however, a hit from his adversary's foil will be a probable result.

But to become aware that one has any definite feeling is a reflex act *at least* as secondary and posterior as it is to become aware of the "self" which has the feeling. I say "*at least*," but I believe that of the two perceptions (1) of "feelings," and (2) of "self," it is the "self" which is the *more* prominently given in our primary, direct cognitions.

I believe that a more laborious act of mental digging is requisite to bring *explicitly* to light the *implicit* mental state, than to bring forward *explicitly* the *implicit* "self-existence." Men continually and promptly advert to the fact that actions and sufferings are *their own*, but do not by any means so continually and promptly advert to the fact that the feelings they experience are "*existing feelings*."

Therefore I am convinced that one of the greatest and most fundamental errors of our day is the mistake of supposing that we can know our "mental states" or "feelings," more certainly and directly than we can know the continuously existing self which has those feelings.

Our perception of our continuous existence also involves the *validity of our faculty of memory*, which is implied in this way, as well as in every scientific experiment we may perform. For we cannot obviously have a reflex perception either of our "feelings" or our "self-existence," without trusting our memory as to the past; since, however rapid our mental processes may be, no mental act takes place without occupying some period of time, and, indeed, nervous action is not extremely rapid. In knowing, therefore, such facts by a reflex act, we know by memory what is already past. Thus our certainty as to our own continuous existence necessarily carries with it a certainty as to our faculty of memory. Therefore, the mental idiocy of absolute scepticism is the penalty that has to be paid for any *real* doubt about our own existence or the trustworthiness of the *faculty of memory*, for all our power of reposing confidence in our observations, experiments, or reasonings, would, in that case, be logically at an end. On the other hand, the validity of our faculty of memory establishes once for all (as we have seen) the fact that we can transcend our present consciousness and know real *objective truth*.

Let us now see the consequences of the denial, or *real* doubt of the second implication of science—the "law of contradiction." Without it we can be certain of nothing, and it therefore lands us in absolute scepticism. And if we would rise from that intellectual paralysis we must accept that dictum as it presents itself to our minds; and the dictum presents itself to my mind, not as a law of *thought* only, but a law of *things*. It affirms, for example, that no creature anywhere or anywhen can at the same time be both bisected and entire.

An amusing instance of the way in which very distinguished men may be misled as to the question of our power of perceiving necessary truth is offered by an imaginary case which has been put forward by Prof. Clifford and Prof. Helmholtz. Their object in advancing it was to show, by an example, how truths which appear necessary to us are not objectively necessary. But the result appears to me to show the direct contradictory of what they intended. Their intention evidently was to support the proposition that we can know "*no truths to be absolutely necessary*," and the result is to show that, even according to them, "*some truths are absolutely necessary*." The necessary truths they propose to controvert are that "a straight line is the shortest line between two points," and that "two straight lines cannot inclose a space."

For this purpose, curious creatures, possessing length and breadth but no thickness, were supposed, by them, to be living on a sphere with the surface of which their bodies would coincide. They were imagined to have experience of length and breadth in curves, but none of height and depth, or of any straight lines. To such creatures, it was said, our geometrical necessary truths would not appear "truths" at all. A straight line for them would not be the shortest line, while two parallel lines prolonged would inclose a space.

To this imaginary objection I reply as follows:—"Beings so extraordinarily defective might, likely enough, be unable to perceive geometrical truths which to less defective creatures—such as ourselves—are perfectly clear. Nevertheless, *if they could conceive of such things* at all, as those we denote by the terms 'straight lines' and 'parallel lines,' then there is nothing to show that they could not also perceive those same necessary truths concerning them which are evident to us."

It is strange that the very men who make this fanciful objection, actually show, by the way they make it, that they themselves perceive the necessary truth of those geometrical relations the necessity of which they verbally deny. For how, otherwise, could they affirm what would or would not be the necessary results attending such imaginary conditions? How could they confidently declare what perceptions such conditions would certainly produce, unless they were themselves convinced of the validity of the laws regulating the experiences of such beings? If they affirm, as they do, that they perceive what must be the truth in their supposed case, they thereby implicitly assert the existence of some absolutely necessary truths, or else their own argument itself falls to the ground.

But this same implication of science, respecting the objective absolute validity of the law of contradiction, also refutes that popular system of philosophy which declares that all our knowledge is merely relative, and that we can know nothing as it really exists independently of our knowledge of it, the system which proclaims the "*relativity of knowledge*."

Of course anything which is "*known to us*" cannot at the same time be "*unknown to us*," and so far as this, our knowledge may be said to affect the things we know. But this is trivial. Our "knowing" or "not-knowing" any object is—apart from some act of ours which results from our knowledge—a mere accident of that body's existence, which is not otherwise affected thereby.

Again, as I before remarked, nothing, so far as we know, exists by itself, and unrelated to any other thing. To say, therefore, that "all our knowledge is relative" might only mean that knowledge concords with objective reality. But this is by no means what the upholders of the "relativity of knowledge" intend to signify. They deny the objective validity, the actual correspondence with reality, of any of our perceptions or convictiohs—even, as Mr. Herbert Spencer tells us, our cognition of "difference."

Every system of knowledge, *however*, must start with the assumption, implied or expressed, that something is true. By the teachers of the doctrine of the "relativity of knowledge" it is evidently taught that the doctrine of the relativity of knowledge is true. But if we cannot know that anything corresponds with external reality, if *nothing* we can assert has more than a relative or phenomenal value, then this character must also appertain to the doctrine of the "relativity of knowledge." Either this system of philosophy is merely relative or phenomenal, and cannot be known to be true, or else it is absolutely true, and can be known so to be. But it must be merely relative and phenomenal, if everything known by man is such. Its value, then, can be only relative and phenomenal, therefore it cannot be known to correspond with external reality, and cannot be asserted to be true; and anybody who asserts that we can know it to be true, thereby asserts that it is false to say that our knowledge is only relative. In that case some of our knowledge must be absolute; but this upsets the foundation of the whole system. Anyone who upholds such a system as this may be compared to a man seated high up on the branch of a tree which he is engaged in sawing across where it springs from the tree's trunk. The position taken up by such a man would hardly be deemed the expression of an exceptional amount of wisdom.

My time has expired, and I may say no more. The considerations I have put before you this evening, should they commend themselves to your judgment, will, I think, lead you to admit that, if we feel confidence and certainty in *any* part of *any* branch of physical science, we thereby implicitly affirm that the human mind can, by consciousness and memory, know more than phenomena—can know some objective reality—can know its own continuous existence—the validity of inference and the certainty of universal and necessary truth as exemplified in the law of contradiction. In other words, the system of the relativity of knowledge is untrue. Thus the dignity of that noble, wonderful power, the human intellect, is fully established, and the whole of our reason, "from turret to foundation-stone," stands firmly and secure. If I have succeeded in bringing this great truth home to one or two of my hearers who before doubted it, I am abundantly repaid for the task I have undertaken. It only remains for me now to thank you for the kind and patient hearing you have been so good as to accord me.

EXAMINATIONS IN SCIENCE.

THE Committee of the Privy Council on Education have just announced an important decision with regard to the examinations of the Science and Art Department in science.

The number of candidates presenting themselves for examination in science is already so large—about 190,000 papers in various branches of science were worked at the examination in May last, besides above 14,000 practical examinations—that the machinery of examination and registration is already severely strained. These numbers will in all probability soon be so increased as to render it impossible to make satisfactory arrangements for the examination of the candidates at the local centres, or for the examination of the worked papers under any system of central examination.

At the same time the means recently placed at the disposal of local authorities for providing or aiding instruction seem to render it unnecessary for the Science and Art Department to continue to give direct aid for very elementary instruction in science. Such instruction can now be more effectually organized and maintained locally.

Under these circumstances it has been decided that after the May examinations of 1892 the payments of £1 now made for the second class in the elementary stage of each science subject shall cease.[1] An elementary paper will continue to be set in each subject, but the results will be recorded simply as *pass* or *fail*, the standard for passing being about the same as that now required for a first class, *i.e.* about 60 per cent. of the marks obtainable.

At the same time, with a view to encourage more advanced instruction, which does not seem to be adequately provided for at present, the payments for the advanced stage and for honours will be considerably increased. The payments on results will then be £2 for a pass in the elementary stage; £5 and £2 10*s*. for a first or second class respectively in the advanced stage; and £8 and £4 for a first or second class respectively in honours, in each subject of science, and in each subdivision of subject 6, theoretical mechanics, or of subject 8, sound, heat, and light, with the following exceptions:—The payments for practical chemistry will be £3 for a pass in the elementary stage, and £6 and £3 10*s*. respectively for a first or second class in the advanced stage; the payments for mathematics will be £2 for a pass in stage 1, £3 and £2 respectively for a first or second class in stages 2 and 4, £4 and £3 for a first or second class respectively in stage 3, £5 and £4 for a first or second class respectively in stages 5, 6, and 7, and £8 and £4 respectively for a first or second class in honours. The payment for section 1 (geometrical drawing) of subject 1 will remain as at present, 10*s*.

The payment for attendance in an organized science school will be increased to £1 in the day school and 10s. in the night school.

As it is of great importance to prevent large numbers of wholly unqualified candidates being presented at the examinations, the examiners will be instructed to note the papers of all such as would not obtain above twenty-five per cent. of the marks, and a deduction will be made from the grant to each school for each such paper sufficient to cover the cost incidental to its examination.

The committee of a science school in a place in Great Britain with less than 5000 inhabitants which does not receive aid from the local authority, or of any science school in Ireland, will be allowed to continue until further notice on the present system, if they so desire it.

[1] The payments on the results of the examinations in 1892 will not be affected by this Minute.

NOTES.

THE subject of an International Congress of Electricity, to be held at Chicago in connection with the World's Fair, continues to attract much attention in America. A report about the matter has been presented to the Director-General of the Exhibition by Mr. J. Allen Hornsby, secretary of the department of electricity. During a recent visit to Europe, Mr. Hornsby discussed the question with several leading men of science in England and on the Continent, and he was encouraged by them to believe that, if certain conditions were complied with, the success of the Congress would be certain. They all agreed that the Congress should be held under the auspices of the U.S. Government. Invitations, they thought, should be issued by the Government to individual scientific men through the Governments of the countries to which the individuals belong. "This course of action," says Mr. Hornsby, "in the opinion of the authorities whom I consulted, will insure an official character to the proceedings of the scientific Congress, and will virtually pledge the various Governments to a recognition and adoption of the standards created."

PROF. JOSEPH WOLSTENHOLME, whose name was well known to mathematicians, died on November 18 in his sixty-third year. He graduated at Cambridge as third wrangler in the Mathematical Tripos of 1850, and became a Fellow first of St. John's College, then of Christ's, where he was for many years a member of the tutorial staff. After vacating his Fellowship by marriage in 1869, he was appointed the first Professor of Mathematics in the Engineering College at Cooper's Hill—a position from which failing health compelled him to withdraw a year or two ago. With the Rev. Percival Frost, he wrote a treatise on solid geometry, published in 1863. He also collected many original mathematical problems, devised by himself, in a volume which appeared in 1867, and again in 1878.

WE regret to announce the death of Mr. S. F. Downing Principal of the Civil Engineering College, Seebpur, Calcutta, which took place at Coonoor, Madras, on October 16 last, at the comparatively early age of forty-seven. The *Englishman* of October 24 says:—"The deceased gentleman was educated at Trinity College, Dublin, and was a graduate of Dublin

University in Arts and Engineering. He came out to India in 1869 as Professor of Civil Engineering in the Engineering Department of the Presidency College, Calcutta, and when that Department was amalgamated in 1880 with the Dehree Training School, and transferred to Seebpur with the title of Government Engineering College, Mr. Downing was chosen as first Principal of the new College. In no College in Bengal has so strict a system of discipline been introduced. The beneficial results of that system, consistently adhered to in the face of strong native opposition, have long been apparent; and the present flourishing condition of the College affords the best monument which could be erected to the indomitable perseverance and uniform justice of the administration of its late Principal."

THE death of Mr. Thomas Wharton Jones, F.R.S., is announced. He was nearly eighty years of age. Prof. Huxley, who was one of his pupils forty years ago, gives in the *British Medical Journal* a bright and pleasant account of his intercourse with his "old master."

THE third series of Hooker's "Icones Plantarum" (vols. xi.–xx. of the whole work) is now complete, and the Bentham Trustees, who are continuing the work under the editorship of Prof. D. Oliver, are offering a limited number of sets of this series of ten volumes, for £5 the set. It contains figures of a thousand new plants, including the most interesting discoveries of the last thirty years, and the most striking of the new genera described by Bentham and Hooker during the progress of their "Genera Plantarum." As the whole impression consists of only 250 copies, the work will soon become unpurchasable. Thanks to the provision made by the late Mr. Bentham, the trustees are issuing a fourth series at the rate of one volume, of 100 plates, annually, at the very low price of 16*s*. Persons wishing to secure a copy of the third series should apply at once to Dulau and Co., 37 Soho Square, W.

THE external part of the laboratory which is being built in the Paris Museum of Natural History for Prof. Chauveau, from the designs provided by him, is now being finished. This laboratory will be used only for original research in physiology and bacteriology, and when completed will be the finest laboratory in France. But the Museum is deeply in debt, and this may cause some delay.

MEMBERS of the Royal Microscopical Society, and the several London and provincial Societies of a kindred nature, have been invited to subscribe to a fund for the benefit of the family of the late Mr. John Mayall. An influential Committee has been formed to secure the success of the scheme. Communications should be addressed to Mr. T. Curties, treasurer to the Committee, 244 High Holborn, W.C. The Committee has issued a circular setting forth Mr. Mayall's great services to the science of microscopy.

ACCORDING to a telegram despatched to the *Standard* from Bangkok on Monday night, Chaiya and Bandon, towns situated on the coast of the Gulf of Siam, have been practically destroyed by a cyclone. The loss of life is estimated at three hundred.

SOME details of the earthquake which caused so much havoc in Japan at the end of October have been received. A large part of the Empire was affected, the shocks being strongly felt in no fewer than thirty-one provinces. In the provinces of Ezozi, Mino, and Owari, several towns and villages were ruined, 3400 persons being killed and 43,000 houses destroyed. An up train and a down train on the Tokaido Railway were just meeting at the station of Gifu when the first shock was felt there. It was accompanied by subterraneous rumblings and violent oscillation, which put the passengers of the train into a great state of alarm. They were further terrified by seeing cracks in the earth, two or three feet wide, opening and closing in all directions, some of which threw up volcanic mud and ashes. A number of the passengers alighted and made their way into the town. Many houses had already fallen, and immense heaps of ruins were visible on every side. Other buildings which were then standing were so severely shattered that further earth-tremors which followed threw them to the ground. There was a marked subsidence of the earth for a considerable area round Gifu. Very soon after the houses collapsed, and while hundreds of persons remained buried under the ruins, flames burst out and spread with such rapidity that the citizens were compelled to desist from the work of rescue. The fire was not subdued until the next morning, when it was found that almost the whole town had been destroyed. The potteries in the prefectures of Owari and Mino, and at Seto and other towns, were reduced to ruins. At Gobo, a temple belonging to the Shin sect of the Buddhists, which was crowded with persons, suddenly collapsed, burying fifty of the worshippers. A slight shock occurred at Nagerio on the night of October 25. On the following Wednesday morning, while forty Christians were assembled in the Methodist school, the building began to totter, and the worshippers fled, several being killed or fatally injured. Many streets were blocked with fallen houses, and others were rendered all but impassable by the crowds of panic-stricken people who were endeavouring to make their escape. Hundreds of persons were killed by the collapse of a thread factory, and a large brick building. A castle four hundred years old, however, remained intact and suffered no damage. It is estimated that in the three towns comprising the city of Nagoya from 750 to 1000 persons lost their lives. From the time of the first disturbance up to the morning of October 30, no fewer than 368 distinct shocks were reported. Fissures 2 feet wide and several feet deep appeared in the earth, while railway metals were twisted, iron bridges broken, river embankments engulfed or destroyed, and fields flooded. A lake 600 yards long and 60 wide was formed at the foot of the Hukusan Mountain in the Gifu prefecture, and great cracks were formed in the ground near the hills. Water sprang from the cracks, and that in the wells was changed to a brownish tint and rendered unfit for drinking. The embankments of most of the rivers were destroyed, and in the Gifu district it will be necessary to rebuild them for a distance of 350 miles. The general appearance of the Mizushima division of the Mortosu district underwent a complete transformation, and at Nogo in one district there was a marked subsidence of the earth. Of 700 temples in the Gifu prefecture, over one-third were destroyed, and it will take many months to repair the river embankments. In some parts of the town of Gifu boiling mud spouted from the fissures for over two hours. The top of the sacred mountain of Fusiyama was rent asunder, a chasm being formed 1200 feet wide and 600 feet deep.

IN a special report to the Secretary of Agriculture, Mr. Mark W. Harrington, Chief of the U.S. Weather Bureau, has presented a general summary of the operations of the Weather Bureau during the three months which followed its transfer to the Department of Agriculture on July 1, 1891. The Service has been reorganized with a view of carrying out the expressed intention of Congress to develop and extend its work with special reference to agriculture. The office force in Washington has been formed into three principal divisions, called respectively the Executive Division, the Records Division, and the Weather-Crop Bulletin and State Weather Service Division. Outside of Washington, local forecast officials have been appointed, the person chosen being in every case selected from the most experienced and competent observers of the Service These officials have been placed in the larger cities, with authority to make predictions for their stations and vicinity, giving the weather more in detail than the Washington forecasts. They are instructed to make a careful study of the climatology of their respective sections, both for their own use as an aid in predicting and for publication for the information of the public; and they are directed to give particular attention to the effect of the weather on the principal crops at their various stages of growth, so as to be able to include in their forecasts reference to this all-important subject. A vast improvement has been effected in the weather maps issued at nearly all the more important stations. They contain not only the forecasts prepared at Washington and the local forecasts, but the data on which the forecasts are based. With regard to weather-signal display stations, Mr. Harrington makes a most striking statement. On June 30 there were about 630 stations to which the forecasts were telegraphed. On September 30 the number was 1200—an increase of about 100 per cent.; and large numbers of new stations are being rapidly established. Altogether, the Bureau is evidently in a state of high efficiency, and has profited largely by the attention which has lately been devoted to it by Congress.

MR. HARRINGTON refers in his report to the enormous accumulation of meteorological records now in the U.S. Weather Bureau. These include the observations for the twenty years during which the meteorological work was in the charge of the Signal Service, and also those for the many preceding years when it was in the charge of the Smithsonian Institution. Mr. Harrington proposes to utilize these data by special studies by officers of the Bureau. He also desires that they may be thrown open to all students of meteorology who are competent to use them, subject only to such restrictions as may suffice to preserve them from injury.

REFERRING to the International Conference of Meteorologists at Munich, Mr. Harrington notes that it was attended by four American delegates, of whom he himself was one. He was much pleased with the cordial way in which European meteorologists expressed appreciation of the meteorological work done in the United States. He speaks especially of the interest excited among students on this side of the Atlantic by the international bibliography of meteorology, begun by General Hazen and published in part by General Greely. "Evidently," he says, "the general sentiment in Europe is to the effect that the work thus far done by the Signal Office is too important to be left unfinished, and that the interests of meteorology and of climatology alike demand that the Weather Bureau should publish the complete work in proper style, after obtaining from European co-labourers all possible corrections to the manuscript that has already been milleographed." Mr. Harrington studied closely the meteorological methods adopted in Europe; and he was particularly struck by the fact that the study of climate has, in general, been prosecuted by European meteorologists to a degree of refinement that has not yet been attained, and is, perhaps, scarcely appreciated, in America. For instance, an eminent climatologist, criticizing the location of some instruments on a rise of ground and amid trees, possibly a hundred feet above the surrounding plain, objected that these instruments could not represent properly the climate of the surrounding country, but that they should have been placed in the open flat fields near at hand. "If this person be correct," says Mr. Harrington, "it is evident that the demands of agricultural climatology are very different from those of dynamic meteorology or the study and prediction of daily weather, and it will be an important result of our European journey if we shall have received a decided stimulus in the direction of minute climatology."

DR. E. BIESE, the Director of the Meteorological Office of Finland, has published the observations taken at Helsingfors during the year 1890. In addition to the ordinary hourly observations and summaries, the volume contains hourly values of atmospheric electricity. Owing to want of funds, the publication of the observations had ceased with those for 1883; but a fresh subsidy to the institution has been granted by a decree of the Emperor, so that the publication will be continued regularly in future, and the arrears also worked off. A summary show that in 1890 rain fell on 178 days and snow on 84 days. The temperature varied from 74°·5 in June to 5°·3 in November, giving an annual range of 69°·2.

IN a recent paper on the camel (*Zeits. für wissen. Geogr.*) Herr Lehmann refers, among other things, to its relations to temperature and moisture. Neither the most broiling heat, nor the most intense cold, nor extreme daily or yearly variations hinder the distribution of the camel. It seems, indeed, that the dromedary of the Sahara has better health there than in more equably warm regions; though, after a day of tropical heat, the thermometer sometimes goes down several degrees below freezing-point, and daily variations of 33°·7 C. occur. In Semipalatinsk again, where the camel is found, the annual variation of temperature sometimes reaches 87°·3. In Eastern Asia, winter is the time the animals are made to work. In very intense cold, they are sewn up in felt covers. Of course each race of camel does best in the temperature conditions of its home: a Soudan camel would not flourish in North-East Asia. Camels are very sensitive to moisture. In the region of tropical rains they are usually absent, and if they come into such with caravans, the results of the rainy season are greatly feared. The great humidity of the air explains the absence of the camel from the northern slopes of the Atlas, and from well-wooded Abyssinia. This sensitiveness expresses itself in the character of different races. The finest, most noble-looking camels, with short silk-like hair, are found in the interior of deserts (as in the Tuarek region, in North Africa), and they cannot be used for journeys to moist regions. Even in Fezzan (south of Tripoli) the animals are shorter and fatter, with long coarse hair; and in Nile lands, and on coasts, it is the same. These animals, too, are less serviceable as regards speed and endurance. Herr Lehmann states it as a law that the occurrence of the camel finds its limits wherever the monthly average vapour tension in the air exceeds 12 mm.

LAST week Prof. Cossar Ewart lectured on "Scottish Zoology" to the newly-formed Edinburgh University Darwinian Society, of which he is President. Having given an account of some of the eminent investigators who have devoted themselves to zoology in Scotland, Prof. Ewart spoke of the need for the encouragement of research at the Scottish Universities. In the case of his own department, it ought, he thought, to be possible for him to say to any exceptionally able student, after the completion of his curriculum, "If you are willing to remain for a year or more, I shall be glad to recommend your being elected a research scholar, and to arrange for your obtaining a small sum from a research fund to provide material, &c., required in any investigation you may undertake." Were there two research scholars, or even but one, at work in each of the scientific departments, Prof. Ewart thinks the Scottish Universities would, before long, have a reputation altogether higher and grander than they at present enjoy, to the gain of science and, in all probability, the further amelioration of humanity.

IN his interesting Rectorial address, at Edinburgh, on the use of the imagination, Mr. Goschen referred to the need for imaginative activity in the exact sciences. It would have been difficult for him to say anything new on a subject with which so many distinguished thinkers have dealt; but the ideas he set forth about science and the imagination were sound and well expressed. Referring to the work of Sir William Thomson, he said: "When I think of your fellow-countryman, Sir William

Thomson, engaged on atoms and molecules, piercing the secrets of the smallest entities, brooding over the mystic dance of ethereal vortices, while his magic wand summons elemental forces to reveal the nature of their powers to his scientific gaze, I forget the disciplined accuracy of the man of science, while lost in wonder at the imaginative inspiration of the poet."

THE trustees of the Missouri Botanical Garden have issued their third announcement concerning garden pupils. The object of the trustees, as we have already stated, is to provide adequate theoretical and practical instruction for young men desirous of becoming gardeners. It is not intended at present that many persons shall be trained at the same time, nor that the instruction shall resemble exactly that given by many State Colleges, but that it shall be quite distinct, and limited to what is thought to be necessary for training practical gardeners. Three scholarships will be awarded by the Director of the Garden before April 1 next. The course extends over six years, so the trustees are particularly anxious that scholarships shall be won by boys who are not much over fourteen years of age.

THE Bulletin of the Botanical Department of Jamaica, for September, contains a report, by Mr. W. Fawcett, Director of Public Gardens and Plantations, on a disease causing the death, on a large scale, of the cocoa-nut palms in the neighbourhood of Montego Bay. The disease first attacks the tissues of the youngest parts. There is no evidence that it is produced by an insect, and Mr. Fawcett considers it is due to an "organized ferment." In the supplement of the *Jamaica Gazette* for September is the remark that the disease is "rapidly destroying the cocoa-nut walks in the parish of St. James, and that, if not checked, in a very few years the cocoa-nut will cease to be a product of this parish, indeed if not of the island."

THE Batavian Society of Arts and Sciences publishes in its Proceedings (Part 46) a list of the chief relics of the Hindu period in Java, and along with it an archæological map indicating the sites of the ruins of temples, statues, and other antiquities. Both list and map are the work of Dr. R. D. M. Verbeek, a well-known engineer.

A PAPER on water and water-supply, with special reference to the supply of London from the chalk of Hertfordshire, by Mr. John Hopkinson, appears in the Transactions of the Hertfordshire Natural History Society (vol. vi., Part 5, October 1891), and has now been published separately. Mr. Hopkinson insists that instead of more water being taken from Hertfordshire for the supply of London the amount at present taken should be reduced. London, he thinks, must sooner or later follow the example of other and much less wealthy towns by obtaining a supplementary supply from a distant source. Liverpool obtains its water from the Vyrnwy, Manchester from Thirlmere, Glasgow from Loch Katrine, and there is a project on foot for Birmingham to obtain a supply from Central Wales. The most feasible scheme for London appears to Mr. Hopkinson to be to obtain a supplementary supply from Bala Lake, or some other lake or lakes in North Wales, or from Central Wales or Dartmoor.

THE White Star liner *Teutonic*, which arrived the other day from New York, after a rapid passage, brought particulars of a collision between the Anchor Line steamer *Ethiopia* and a large whale, eight hundred miles east of Sandy Hook, on the 15th inst., on the passage to New York from Glasgow. At 10.45 a.m. Captain Wilson and Second Officer Fife were on the bridge keeping a close watch ahead. Suddenly a whale came to the surface directly in the path of the ship, and only a few feet ahead. The ship was rushing towards the whale at the rate of sixteen miles an hour. There was no time to check the speed of the vessel, and almost before the astonished officers realized it, the ship's sharp iron prow crashed into the monster. The blow was a square, incisive one. The ship seemed to sail right through the whale, which disappeared almost immediately, leaving a trail of crimson as far as the eye could see. Shortly afterwards the whale was sighted astern, floating lifelessly. When the ship came into collision with the whale the shock caused the vessel to tremble from stem to stern, and startled the passengers for a moment. The passengers who were below rushed on deck, and a panic seemed to be imminent. Captain Wilson hurriedly left the bridge and appeared on deck. "Have no fear," he said, "we have only killed a whale. The ship is not hurt." His words allayed the fears of the passengers.

IN his recent Presidential address to the Royal Society of New South Wales, Dr. A. Leibius referred with satisfaction to the progress made by the cause of scientific and technical education in New South Wales. In addition to the opportunities given by the University of Sydney for the study of science, the Government, by the establishment of a technical college and technological museum at Sydney, with branches in different parts of the colony, have brought within the reach of all who desire it the means of acquiring scientific and technical knowledge. As an illustration of the extent to which the colony is developing this part of its educational system, Dr. Leibius mentioned that contracts already let in connection with the Sydney College alone amount to close upon £48,000, while £20,000 have been voted by Parliament for technical colleges and technological museums at Bathurst, Broken Hill, Maitland, and Newcastle.

THE Michigan Mining School, at Houghton, sends us its "Catalogue" for 1890–91. The course of instruction for the regular students at this institution extends over a period of three years, the work continuing through most of the year. The authorities of the school express an earnest desire to secure as students young men who, before beginning their professional studies, have obtained "an education of the broadest and most liberal character." Every regular student is required "to spend seven hours a day for five days each week in the laboratory or field work, or in recitation or lecture." His "recitations" are prepared "in time taken outside of the seven hours a day." On Saturdays, or on other days, as occasion may require, excursions are made to the mines, mills, and smelting-works in the neighbourhood.

AT a meeting of the Pharmaceutical Society at Edinburgh on November 11, a capital address was delivered by Prof. I. Bayley Balfour, on botanical enterprise in relation to pharmacology. Prof. Balfour devoted himself especially to the task of showing how vast are the obligations of pharmacologists to the Royal Gardens, Kew. The address is printed in the current number of the *Pharmaceutical Journal*.

MR. J. E. DIXON records, in the *Victoria Naturalist* for October, a curious fact which came under his own observation. During a ramble along the Kooyong Creek, Oakleigh, on August 15, he was somewhat surprised to see a specimen of the ring-tailed opossum, hanging, as he thought, by her claws, to a sharp-pointed limb of a gum-tree, about twenty feet from the ground. Upon closer observation he found that the creature was dead, and that death was due to the fact that in her flight she had become impaled by her pouch. In the pouch were two young ones almost old enough to leave her.

MR. ANGELO HEILPRIN contributes to the New York *Nation* of November 12 an interesting paper in which he describes the charms of a summer tour to Greenland. A journey to the 75th parallel of latitude, or thereabouts, could, he says, be arranged annually with much of the certainty of a trans-

Atlantic trip, and would involve neither hardship nor danger. During the latter part of July and throughout the whole of August the coast is mainly free of ice, and even the passage of the much-dreaded Melville Bay can very generally be effected during this season of the year without danger from a "nip," and frequently with not so much as an acre of ice to interfere with the traveller's journey. Once beyond Cape York, the free North Water opens up a passage to the 79th or the 80th parallel of latitude, or to within some 700 miles of the Pole. In the course of such a trip the traveller would see much that is novel and interesting, much that is grandly picturesque, and still more that is striking in its deviation from the rest of the earth. A country inhabited by a race of people so remarkable as are the Eskimos is always worthy of a visit, especially at a time when a greatly increasing interest in the science is fostering the study of ethnology. But merely in the contemplation of the forms of the almost endless number of icebergs, the vacation tourist would probably consider himself amply repaid for a journey to this easily-reached land of the midnight sun, with its almost numberless glaciers, its sky-splitting mountains, and a boundless ice-cap. The artist, too, would find abundant suggestion for his brush and palette.

Prof. August Weismann's "Amphimixis: oder, Die Vermischung der Individuen," has been published at Jena by Herr Gustav Fischer. An English translation, we believe, will shortly be issued.

A French translation—edited by Dr. H. de Varigny—of Weismann's "Essays on Heredity" (Reinwald) has been issued in Paris.

The third volume of Dr. McCook's "American Spiders and their Spinning Work," will be ready for delivery in the coming spring. The numerous lithographic plates are many of them prepared and in the colourists' hands. The cost of preparing the numerous engravings and plates has greatly exceeded the expectations of the author (who is also the publisher).

Dr. Adolf Fritze contributes to the *Mittheilungen der Deutschen Gesellschaft für Natur- und Völkerkunde Ostasiens, in Tokio* (Heft 46) a valuable paper on the fauna of Yezo in comparison with that of the rest of Japan. He does not, of course, profess to give a complete account of the subject; but the natural history of Yezo has hitherto been so imperfectly investigated that his work will be very welcome to zoologists.

Mr. Robert E. C. Stearns gives in the Proceedings of the U.S. National Museum (vol. xiv., pp. 307–335), a valuable list of shells collected on the west coast of South America, principally between latitudes 7° 30′ S., and 8° 49′ N., by Dr. W. H. Jones, Surgeon, U.S. Navy. This collection, with various other treasures, was presented to the National Museum in 1884; but until lately Mr. Stearns had not an opportunity of preparing a list. A great part of the shells were picked up on the beaches, and in poor condition; but our knowledge of the distribution of west South American species is so limited that the collection, Mr. Stearns says, has its special value for the information it furnishes on this point.

The following science lectures will be given at the Royal Victoria Hall on Tuesday evenings during December:—December 1, "North Wales," by A. Hilliard Atteridge; 8, "The Ways in which Animals hide Themselves," by E. B. Poulton; 15, "Old Stones," by H. G. Seeley.

At the meeting of the Chemical Society on Thursday last some further particulars were given by Mr. Mond concerning his work in conjunction with Dr. Langer upon iron carbonyl. They have succeeded in isolating two distinct compounds of iron and carbon monoxide. One of them is a liquid of the composition $Fe(CO)_5$, to which the name ferro penta-carbonyl is given. The other is a solid corresponding to the formula $Fe_2(CO)_7$, and is termed di-ferro hepta-carbonyl. Liquid ferro penta-carbonyl is obtained by heating finely-divided iron, obtained by reduction of ferrous oxalate, in a stream of carbon monoxide. The operation is a very slow one, 100 grams of metallic iron yielding one gram of the liquid in twenty-four hours. Ferro penta-carbonyl is a light amber-coloured liquid, which may be distilled without decomposition. It boils constantly at 102°·8 C. Its specific gravity, compared with water at 18°, is 1·44. It solidifies at −21°, forming yellow acicular crystals. Its vapour density has been determined, the number obtained being 6·5, agreeing fairly well with the value 6·7 calculated for $Fe(CO)_5$. The liquid is quite stable in the dark, but when exposed to light an important change occurs. Gold-coloured crystals rapidly form in it, which upon analysis are found to consist of a second iron carbonyl, the di-ferro hepta-carbonyl $Fe_2(CO)_7$. These crystals are almost insoluble in the ordinary solvents. When warmed to 80°, however, they decompose, the products of decomposition being the penta-carbonyl metallic iron, and carbon monoxide. It appears, therefore, that iron does not exactly resemble nickel in its behaviour with carbon monoxide, for the carbonyl compound of the latter metal, it will be remembered, possesses the composition $Ni(CO)_4$.

A note upon the products of oxidation of nickel carbonyl is contributed by M. Berthelot to the current number of the *Comptes rendus*. M. Berthelot states that nickel carbonyl behaves towards oxygen in a manner somewhat similar to an organic radicle. The products of its spontaneous oxidation do not consist entirely of the oxides of nickel and carbon. The liquid may be preserved in a glass vessel under a layer of water without change so long as air is excluded; but as soon as air is admitted, the compound slowly oxidizes, and a quantity of apple-green hydrated oxide of nickel free from carbon is deposited. At the same time a portion of the nickel carbonyl volatilizes and oxidizes in the air, forming a white cloud which deposits upon all the objects in the neighbourhood. M. Berthelot has succeeded in collecting a considerable quantity of this white deposit, and has subjected it to analysis. He considers it to be the hydrate of the oxide of an organic radicle containing nickel. The numbers obtained from the analysis agree with the formula $C_2O_3Ni_3 . 10H_2O$, but as it appears likely that the preparation contained more or less nickel hydrate this formula is not considered final. M. Berthelot is of opinion that the substance probably contains an organo-nickel compound of the composition C_2ONi, belonging to a type derived from ethylene. He is continuing the study of this interesting substance.

The additions to the Zoological Society's Gardens during the past week include a Bonnet Monkey (*Macacus sinicus* ♀) from India, presented by Mr. J. Robinson; a Rhesus Monkey (*Macacus rhesus* ♀) from India, presented by Mrs. K. Clark-Ourry; a Macaque Monkey (*Macacus cynomolgus* ♀) from India, presented by Captain J. F. C. Hamilton; two Ourang-outangs (*Simia satyrus* ♂ ♂) from Borneo, a Greater Sulphur-crested Cockatoo (*Cacatua galerita*) from Australia, four —— Pelicans (*Pelecanus* sp. inc.) from India, deposited; a Bronze-winged Pigeon (*Phaps chalcoptera* ♂) from Australia, a Blood-breasted Pigeon (*Phlogœnas cruentata* ♀) from the Philippine Islands, purchased.

OUR ASTRONOMICAL COLUMN.

Determination of the Solar Parallax.—A. Auwers, in *Astronomische Nachrichten* (No. 3066), gives the results obtained in the determination of the solar parallax from the heliometer observations made by the German Transit of Venus Expedition, in the years 1874 and 1882. The number of measurements taken amounted to 754, of which 308 were made from the 1874, and the remaining 446 from the 1882 transit. Taking each series of measurements of each transit separately, and applying the corrections of Leverrier's tables,

$$\text{Transit of 1874 Dec. 8} \quad \Delta\alpha = +4''.69 \quad \Delta\delta = +2''.30$$
$$\text{,, 1882 Dec. 6} \quad +9''.13 \quad +1''.99$$

he obtains the following values for the parallax—

$$\text{Transit of 1874} \quad \pi = 8''.873$$
$$\text{,, 1882} \quad \pi = 8''.883$$

Both the above numbers are subject to the mean errors $\pm 0''.062$ and $\pm 0''.037$ respectively, and are computed in the first case from 307, and in the second from 444 measurements.

By taking now the two series together, and finding the most probable number, he obtains the following result subject to the two adjoined errors—

$$\pi = 8''.880$$
$$\text{Mean error} = \pm 0.032$$
$$\text{Probable error} = \pm 0.022$$

A comparison of the above results with those of other observers, taking the transits of 1874 and 1882, may be gathered from the following list—

Transit 1874.		Transit 1882.	
Harkness	8″.888	Auwers	8″.883
Todd	8·883	Cornu	8·86
French measures ...	8·88	Harkness	8·842
Stone	8·88	Faye	8·813
Auwers	8·873	Todd	8·803
Tupman	8·81		
Airy	8·76		

PHOTOMETRIC OBSERVATIONS.—The *Publications of the Potsdam Astro-Physical Observatory*, No. 27, contains a series of photometric measurements made by Dr. Müller at a station on the Säntis, situated 2500 metres above sea-level, with a Zöllner's photometer. The observations extend over two months, and they show that the form of the curve of extinction from the zenith to a point very near the horizon is satisfactorily represented by Laplace's Theory. But a comparison of the curves calculated separately for the various days of observation shows considerable differences, which approach and even exceed 0·4 of a magnitude near the horizon. The superiority of the Säntis station over Potsdam as regards conditions of atmospheric transparency is very striking. For a star in passing from the zenith to an altitude of about 2° has its light diminished nearly by a whole magnitude more in the plain than on the top of the mountain. From the observations, according to Laplace's Theory, the loss of light produced by the atmosphere in the zenith at Säntis is about 12 per cent.; or, in other words, a star viewed from a point above the atmosphere would appear brighter by about 0·14 of a magnitude. Since the corresponding value for Potsdam is 0·2 magnitude, it follows that the absorption produced by a stratum of atmosphere between sea-level and a height of 2500 metres amounts to 0·06 magnitude. Before this value, however, can be accepted as definite, simultaneous observations of stellar magnitudes must be made at stations lying closer together than the two between which the comparison is instituted.

THE PAMIRS.

AT the meeting of the Royal Geographical Society on Monday the paper read was on a recent journey across the Pamir by Mr. and Mrs. Littledale. In introducing the paper, Mr. Douglas Freshfield made some remarks on the subject generally.

The Pamir or Pamirs (Mr. Freshfield said)—for Pamir is a generic term, the different strips of tableland are distinguished by separate names—is a vast tableland averaging 12,000 feet in height and 200 miles in length by 120 to 150 miles in breadth, ringed by a rough horseshoe of mountain ranges, and intersected by snowy ridges and shallow trenches that deepen westwards, where the streams of the Oxus descend towards Bokhara. The numerous photographs taken by Mr. Littledale exhibit a characteristic type of landscape:—tent-shaped, glacier-coated ridges, bare heights naked of verdure and shorn of forests by the bitter winds and frosts, desolate bituminous lakes; a region where for the most part there is neither fuel nor fodder; an Engadine of Asia, with nine months winter and three months cold weather; the home of the wild sheep, the summer haunt of a few wandering shepherds; nomads' land if not no man's land. Long ago Marco Polo described it well. That is the scene of Mr. and Mrs. Littledale's adventures; that is the region where the emissaries of three nations are now setting up rival claims. "The half-way house to heaven" is a Chinese appellation for the Pamirs. "Cœlum ipsum petimus stultitiâ" our and the Russian soldiers and diplomats may now almost say of one another. For the tales of summer pastures of extraordinary richness, told to Marco Polo and repeated to Mr. Littledale, refer, so far as they are true at all, only to isolated oases. The country in question cannot feed the caravans that cross it; far less could it sustain the baggage animals of an army on the march. No one in his senses could consider that in itself the Pamir is a desirable acquisition. Any value it may have is in relation to adjoining lands. From the north there is comparatively easy access to it from Russian Turkistan. From the east the Chinese and their subjects climb up the long ascent from the Khanates, and pass through easy gaps in the encircling horseshoe of mountains on to the portions of the tableland they claim. From the south, a route which seems from Mr. Littledale's experience to be anything but a military route, leads over glaciers, passes, and through well-nigh impassable gorges into Gassin and Chitral, and so to Kashmir. To the south-west easier routes, little known or little described as yet, lead into the wild regions of Kaffiristan and Afghanistan. We do not here deal with politics, but we do deal with the geographical and cartographical facts on a knowledge of which politics and policy ought to be—but unfortunately for our country have not always been—based. Certain portions of the Pamir have been more or less closely attached to Afghanistan. The Amir lays claim to Wakhan, Chignan, and Roshan, tracts stretching along the sources of the Oxus. It is obvious that England will claim an interest in these, but probably, owing to the deficiencies in exact knowledge of the geographers of Cabul, we have not as yet formulated publicly our claims.

In 1873 the Russian Government, at the time of their advance to Khiva, undertook never to pass the Oxus. Shortly afterwards, Sir Henry Rawlinson argued with great force that the Murgabi, the stream that cuts the Pamirs in two, and not the Pandja, which flows along their southern skirts, was the true and proper source of the Oxus. Seven years ago, in the negotiations which followed the Penjdeh incident, the negotiators deliberately left this portion of the frontier out of their calculations.

Why, undeterred by the experiences of which that entertaining traveller and Anglophobe, M. Bonvalot, had lately given so alarming a picture, should an Englishman and his wife cross this desert? Mr. and Mrs. Littledale are eager in the pursuit of rare game. They were old travellers; they had sojourned in the forest wildernesses of the western Caucasus; they had, on a previous occasion, penetrated Central Asia. A pair of horns were to them what a bit of rock from a maiden peak is to others.

And lastly, why did Mr. and Mrs. Littledale go from north to south? Why did they, being English, make Russian territory their starting-point? Thereby hangs a tale. Because our Anglo-Indian Government prohibits all independent travel in its trans-frontier lands. Something may be said for this course, but it does not stop there. It also gags its own official explorers. It carries yearly farther and farther the policy deprecated by Sir H. Rawlinson in this hall, when he said: "Russia deserves all honour for her services to geographical science in Asia. I only wish I could say as much for ourselves as regards our own frontiers."

No one, least of all the Council of this Society, would ask for the publication of any tactical information our military authorities desired to withhold. But the military authorities go along with us in asking for an intelligent censorship in place of a wholesale system of suppression of the mass of knowledge, general and scientific, acquired by the servants of the State in our frontier and trans-frontier lands. We believe, and the Council have represented to H.M. Government, that the present practice is not in accordance with the existing official rules, that it was intended and has been ordered that expurgated copies of all official reports of public interest should be given to the public. They hope that the departments concerned will before long be instructed to give practical effect henceforth to any such instructions that may exist, and thus that the forward march of English power may once more, as it should, be accompanied by a general advance of scientific knowledge.

Leaving Samarcand early in May, Mr. and Mrs. Littledale drove in Russian post-carts up the beautiful valley of the Syr-Daria, which reminded them in parts of the Vale of Kashmir, as far as Osh, the last post-station. Here they organized their caravan for their great adventure, the crossing of the Pamirs into Kashmir. They had the advantage of previous experience of Central Asian travel, and of the cordial assistance of the Russian Commandant, Colonel Deubner, who could hardly have done more for the travellers had they been his own nearest relatives. After much hesitation from the difficulty of obtaining any trustworthy information as to the state of the Alai passes, they selected the Taldik, 11,600 feet, before crossing which, they left behind the last tree and bush they were to see untill reaching the valley of the Gilgit.

Crossing the Alai plateau they proceeded by the Kizil Art Pass to Karakul Lake. Thence their route led over passes of 15,500 feet, in sight of the great Mustag Atta to the Murgab or North Oxus, which they struck at 12,300 feet, their correct elevation between the Alai and Sarbad. Another pass of 14,200 feet led over the Alichur Pamir—where *Ovis poli* horns lie about in hundreds—to the Boshgumbaz Valley. The pass of the same name was found impracticable. Mr. and Mrs. Littledale made a long detour to visit the Victoria Lake, one of the sources of the South Oxus, for purposes of sport. Thence they turned eastwards and crossed by the Little Pamir Lake into the Valley of Wakhan. When near Sardab they met with their first misadventure, and this was the encounter with the troops of our ally the Ameer. The civil authorities detained Mr. and Mrs. Littledale for many days, and only let them go at last grudgingly, and after having despoiled them as far as they could without open robbery.

ELIZABETH THOMPSON SCIENCE FUND.

THIS fund, which has been established by Mrs. Elizabeth Thompson, of Stamford, Connecticut, "for the advancement and prosecution of scientific research in its broadest sense," now amounts to $26,000. As accumulated income will be available in December next, the trustees desire to receive applications for appropriations in aid of scientific work. This endowment is not for the benefit of any one department of science, but it is the intention of the trustees to give the preference to those investigations which cannot otherwise be provided for, which have for their object the advancement of human knowledge or the benefit of mankind in general, rather than to researches directed to the solution of questions of merely local importance.

Applications for assistance from this fund, in order to receive consideration, must be accompanied by full information, especially in regard to the following points:—

(1) Precise amount required. Applicants are reminded that one dollar ($1.00 or $1) is approximately equivalent to four English shillings, four German marks, five French francs, or five Italian lire.

(2) Exact nature of the investigation proposed.

(3) Conditions under which the research is to be prosecuted.

(4) Manner in which the appropriation asked for is to be expended.

All applications should reach, before December 10, 1891, the Secretary of the Board of Trustees, Dr. C. S. Minot, Harvard Medical School, Boston, Mass., U.S.A.

It is intended to make new grants at the end of 1891.

⁎ The trustees are disinclined, for the present, to make any grant exceeding three hundred dollars ($300); decided preference will be given to applications for smaller amounts.

(Signed) HENRY P. BOWDITCH, President.
WILLIAM MINOT, JR., Treasurer.
EDWARD C. PICKERING.
FRANCIS A. WALKER.
CHARLES-SEDGWICK MINOT, Secretary.

List of Grants hitherto made.[1]

1. $200, to the New England Meteorological Society, for the investigation of cyclonic movements in New England. [*American Meteorological Journal* for 1887, and May 1888.]
2. $150, to Samuel Rideal, Esq., of University College, London, England, for investigations on the absorption of heat by odorous gases.
3. $75, to H. M. Howe, Esq., of Boston, Mass., for the investigation of fusible slags of copper and lead smelting. [*Trans. Amer. Institute of Mining Engineers*, Feb., 1890.]
4. $500, to Prof. J. Rosenthal, of Erlangen, Germany, for investigations on animal heat in health and disease, [*Sitzungsber. K. Akad. Wiss.*, 1888, 1309–1319; 1889, 245–254. *Arch. Anat. u. Physiol.*, Suppl. 1888, 1–53.]
5. $50, to Joseph Jastrow, Esq., of the Johns Hopkins University, Baltimore, Md., for investigations on the laws of psycho-physics. [*American Journal Psychology*, 1890, III., 43–58.]
6. $200, to the Natural History Society of Montreal, for the investigation of underground temperatures. [*Canadian Record of Science.*]
7. $210, to Messrs. T. Elster and H. Geitel, of Wolfenbüttel, Germany, for researches on the electrization of gases by glowing bodies. [*Sitzungsber. K. Akad. Wiss. Wien.*, xcvii., Abth. ii., 1175–1264, 1889.]
8. $500, to Prof. E. D. Cope, of Philadelphia, Penn., to assist in the preparation of his monograph on American fossil vertebrates.
9. (Withdrawn.)
10. $125, to Edw. E. Prince, Esq., of St. Andrews, Scotland, for researches on the development and morphology of the limbs of Teleosts. ["Inaugural Dissertation," Pp. 24, Pls. II., Glasgow, 1891.]
11. $250, to Herbert Tomlinson, Esq., of University College, England, for researches on the effects of stress and strain on the physical properties of matter. [*Philos. Magazine*, Jan., 1890, 77–83.]
12. $200, to Prof. Luigi Palmieri, of Naples, Italy, for the construction of an apparatus to be used in researches on atmospheric electricity.
13. $200, to Wm. H. Edwards, Esq., of Coalburg, W.Va., to assist the publication of his work on the butterflies of North America. ["Butterflies of North America," 3rd Series, Part V.]
14. $150, to the New England Meteorological Society, for the investigation of cyclonic phenomena in New England.
15. $25, to Prof. A. F. Marion, for researches on the fauna of brackish waters.
16. $300, to Prof. Carl Ludwig, for researches on muscular contraction, to be carried on under his direction by Dr. Paul Starke. [*Abhandl. math. phys. Classe K. sächs. Ges. Wiss.*, xvi., 1890, 1–146, Taf. i.–ix.]
17. $200, to Dr. Paul C. Freer, for the investigation of the chemical constitution of graphitic acid.
18. $300, to Dr. G. Müller, for experiments on the resorption of light by the earth's atmosphere. [*Publicationen Astrophys. Observ. Potsdam.*, viii., 1–101, Taf'n II.]
19. $300, to Prof. Gerhard Krüss, for the investigation of the elementary constitution of erbium and didymium. [*Liebig's Annalen*, Bd. 265, 1–27.]
20. $50, to Dr. F. L. Hoorweg, for the investigation of the manner and velocity with which magnetism is propagated along an iron bar.
21. $150, to Mr. W. H. Edwards, to assist the publication of his work on North American butterflies. ["Butterflies of North America," 3rd Series, Part VIII.]
22. $250, to Dr. Ernst Hartwig, for researches on the physical libration of the moon (see Grant No. 27).
23. $200, to Prof. Charles Julin, for researches on the morphology of Ascidians.
24. $250, to Prof. M. Nencki, for researches on the decomposition of albumenoids by microbes. [*Arch. Expt. Path. Pharmak.*, xxviii., 311–350, Taf. IV.–V.]
25. $200, to Prof. Carl Frommann, for researches on the minute organization of cells.
26. $300, to Edward Atkinson, Esq., for experiments on cooking, to be carried on under the direction of Mrs. Ellen H. Richards. [Proc. Amer. Assoc. Adv. Sci., 1890.]
27. $250, to Dr. Ernst Hartwig, to continue the work of Grant No. 22.

[1] The results published are given within brackets.

28. $200, to Edward S. Holden, Esq., for researches on stellar spectroscopy, to be carried on at the Lick Observatory.
29. $150, to Prof. J. Kollmann, for investigations on the embryology of monkeys.
30. $25, to Prof. J. P. McMurrich, Clark University, Worcester, Mass., to study embryology of Aurelia.
31. $200, to Dr. Johannes Dewitz, Zoolog. Institute, Berlin, Germany, for researches on the laws of movement of Spermatozoa.
32. $150, to Alexander McAdie, Clark University, Worcester, Mass., for experiments on atmospheric electricity.
33. $250, to Prof. Julien Fraipont, University of Liége, Liége, Belgium, for the exploration of the cave of Engihoul.
34. $50, to Prof. M. E. Wadsworth, Houghton, Michigan, for observations on the temperature in mining-shafts.
35. $50, to Prof. A. B. Macallum, University of Toronto, Toronto, Canada, to study the digestion and absorption of chromatine.
36. $250, to Dr. G. Baur, Clark University, Worcester, Mass., for the exploration of the Galapagos Islands.
37. $300, to Prof. Edw. S. Holden, Lick Observatory, Cal., for astronomical photography.
38. $250, to Prof. Louis Henry, Louvain, Belgium, for researches on the fundamental solidarity of carbon compounds.
39. $300, to Prof. L. Hermann, Königsberg, Prussia, for phonographic experiments on vowels.
40. $50, to Prof. Alpheus Hyatt, Cambridge, Mass., for researches on the evolution of Cephalopoda.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

OXFORD.—Convocation on Tuesday arrived at the following decision :—

"That the University accept the offer of Mr. G. J. Romanes, F.R.S., Christ Church, to give an annual sum of £25 for a lecture to be delivered once a year on some subject approved by the Vice-Chancellor relating to science, art, or literature. The lecturer to be called the Romanes Lecturer, and to be appointed by the Vice-Chancellor annually in the Michaelmas Term, the lecture to be delivered in the next following Easter or Trinity Term on a day to be fixed by the Vice-Chancellor, who shall give public notice thereof to the University in the usual manner. Also, that the thanks of the House be given to Mr. Romanes for his liberality."

We understand it was Mr. Romanes's wish that the foundation should be anonymous; but as such a course was found to be without precedent, and otherwise impracticable, he yielded the point to the University authorities.

Mr. H. T. Gerrans, Fellow of Worcester College, has been elected by the Board of the Faculty of Natural Science a member of the Committee for nominating Masters of the Schools from Hilary Term 1892 to Hilary Term 1895. Mr. C. H. Sampson, Fellow of Brasenose College, has been elected by the same Board of Faculty a member of the Committee for nominating Mathematical Honour Moderators.

SCIENTIFIC SERIALS.

A GOOD deal of interesting geological information is given in the last number of the *Izvestia* of the East-Siberian Branch of the Russian Geographical Society (vol. xxii., 2 and 3). M. Obrutcheff gives an orographical and geological sketch of the highlands of the Olekma and the Vitim, with the exploration of which he was intrusted by the mining administration. Besides the upheavals of these highlands, which have a general direction from the south-west to the north-east, M. Obrutcheff found another series of upheavals stretching west-north-west to east-south-east, the chief ridge of that system (named Kropotkin's ridge by the author) rising to the height of from 1300 to 1500 metres, and separating the tributaries of the Lena from those of the Vitim. Several lower chains seem to have the same direction. The whole series consists of metamorphic slates and limestones, intersected by granites and gneisses, and belongs to the Lower Silurian and Cambrian system, a closer definition of its age being difficult on account of a total want of fossils. M. Obrutcheff also confirms the glaciation of the whole of these highlands. The valleys are filled up with morainic deposits, with polished and striated boulders, and there are traces of inter-glacial layers. The *dômes arrondis* and the *roches moutonnées*, so familiar to the glacialist, are frequent, and the author gives interesting facts to confirm the transport of boulders at great distances over the mountain-ridges, which cannot be explained without admitting that the whole of the highlands was covered with a mighty ice-cap. The same number contains a note by the same author on the Jurassic fossil plants recently discovered on the Bureya River (a tributary of the Amur), and a list of 290 flowering plants collected by Mme. Klements in South Yeniseisk and Tomsk, and described by M. Preyn.

SOCIETIES AND ACADEMIES.

LONDON.

Chemical Society, November 5.—Mr. W. Crookes, F.R.S., Vice-President, in the chair.—The following papers were read :—The magnetic rotatory power of solutions of ammonium and sodium salts of some of the fatty acids, by Dr. W. H. Perkin, F.R.S. Ostwald has argued that the peculiar results obtained by the author in the case of solutions of acids and of ammonium salts, &c., are in accordance with the electrolytic dissociation hypothesis; and has suggested that since salts formed from weak acids are as good conductors as those formed from strong ones, we may expect in this case also, marked deviations from the calculated values. He also considers that such salts as ammonium formate, &c., when in aqueous solution would show molecular rotations which would not be the sums of the rotations of the components of the salts, as must nearly be the case if the view put forward by the author be correct, that such salts are almost entirely dissociated into acid and base. The author has obtained results which show that the rotatory powers of the ammonium and sodium salts do not vary with dilution; and on comparing the experimental values obtained in the case of ammonium salts with those afforded by the constituent acid and ammonia, as might be expected, as reduction of rotatory power always attends combination, the values are slightly less in the case of the salts. This reduction is very nearly the same as that which takes place in the formation of the corresponding ethereal salts, and as the latter are anhydrous, the results show that the values for ammonium salts in solution are practically those of the dry salts, and therefore that Ostwald's views are inapplicable.—Note on the action of water gas on iron, by Sir H. E. Roscoe and F. Scudder. Whilst making experiments on the application of water gas for illuminating purposes, the authors have observed that occasionally the Fahnehjelm comb becomes coated with a deposit of ferric oxide, and a further examination of the tips of the steatite burners showed that the deposit of ferric oxide was "coralloid," and therefore could not be produced from dust in the atmosphere. They also observe that water gas which has been standing in steel cylinders at a pressure of 8 atmospheres for about a month contains a much larger quantity of iron. A preliminary determination of the iron in this gas amounted to 2·4 milligrams per litre. Although the compound, which is doubtless the iron carbonyl of Mond and Quincke, is only present in this small quantity, the authors have succeeded in proving that it can readily be liquefied. In the discussion which followed, the Chairman referred to the fact that at the recent British Association meeting at Cardiff, Mr. Mond had exhibited specimens not only of liquid iron carbonyl, but also of a solid compound of iron with carbonic oxide. Prof. Ramsay stated that he had found that the compound of nickel with carbonic oxide was formed in the cold.—The dissociation of liquid nitrogen peroxide, by J. Tudor Cundale. The author has determined by colorimetric methods the relative amount of NO_2 formed in liquid nitrogen peroxide, (1) by dilution with chloroform, (2) by rise of temperature. He has also aseertained the absolute amounts of dioxide by comparing the colour of the liquid solution with that of the gas containing a known amount of nitrogen peroxide. The results show that, on dilution, (1) dissociation takes place very slowly at first, but more rapidly when less than 5 per cent. of the peroxide is present; (2) that solutions of the peroxide dissociate more rapidly than the pure liquid on rise of temperature.—Ortho- and para-nitro-ortho-toluidine, by A. G. Green and Dr. T. A. Lawson. The authors find that when ortho-toluidine sulphate is nitrated in a large excess of sulphuric acid at a low temperature, three isomerides are formed—namely, *para*-nitro-ortho-toluidine (about 75 per cent.), *meta*-nitro-ortho-toluidine, $C_6H_3Me(NH_2)(NO_2)[1:2:5]$ (about 3 or 4 per cent.), and *ortho*-nitro-ortho-toluidine, $C_6H_3Me(NH_2)(NO_2)[1:2:6]$ (about 20 per cent.). The separation of the ortho-nitro-ortho-toluidine from the mixture is effected by taking advantage of the greater solubility of this isomeride in slightly warm water. The authors give a table of the properties of the ortho- and para-nitro-ortho-toluidines, and of their products on reduction and other derivatives.—Researches on the gums of the arabin group: Part ii. Geddic acids—Gedda gums; the dextro-rotatory varieties, by C. O'Sullivan. The Gedda gums described consist of the calcium, magnesium, and potassium salts of gum acids, the calcium salt predominating, and more or less nitrogenous matter, which is probably combined with a true gum acid. They dissolve easily in water, forming a yellow or reddish syrup, neutral to test-paper, which is dextro-rotatory. The gum acids are obtained pure by dialyzing their acidified solution, and by fractional precipitation with alcohol. The gum acids in any one sample of gum bear a very simple relation to one another, and are closely related to the gum acids contained in other samples. A table of their relationships is given. The composition and partial constitution of any one of the gum acids which have been as yet examined may be expressed by the general formula, $C_{23}H_{38-2n}O_{22-n}.nC_{12}H_{20}O_{10}.pC_{10}H_{16}O_8$. These gum acids, when heated at 80°–100° for 10–30 minutes with a solution containing 2 per cent. H_2SO_4, are hydrolyzed, yielding arabinon and a gum acid of lower molecular weight. The gum acids thus produced closely resemble the gum acids existing in the natural gums, but are less optically active and more insoluble in weak alcohol. The most marked difference between these gum acids and those existing in the natural gums is that they are only hydrolyzed with difficulty with 2 per cent. sulphuric acid. They are, however, slowly broken down by several hours' digestion, and acids of successively lower weight are formed. The lowest stage of the hydrolysis is represented by the general equation:—

$$C_{23}H_{38-2n}O_{22-n}.nC_{12}H_{20}O_{10} + 3nH_2O = C_{23}H_{38}O_{22} + 2nC_6H_{12}O_6.$$

The compound $C_{23}H_{38}O_{22}$ has not yet been obtained in sufficient quantity for an examination of its properties. Those gum acids obtained from Gedda gum are highly dextro-rotatory, whilst those from gum arabic, although otherwise identical, are inactive.—Some compounds of the oxides of silver and lead, by Emily Aston. The author finds that on following the directions given by Wöhler for the preparation of the compound $Ag_2O,2PbO$ the product varies in composition. A substance of the composition $2Ag_2O,PbO$ is obtained when a mixture of lead and silver hydroxides is allowed to stand in presence of caustic soda, and also by precipitating the mixed nitrates of lead and silver, and exhaustively extracting with caustic soda.—The electrolysis of potassium acetate solutions, by Dr. T. S. Murray. On electrolyzing a dilute aqueous solution of potassium acetate only hydrogen and oxygen are evolved; with concentrated solutions a mixture of ethane, hydrogen, oxygen, methyl acetate, and carbon dioxide is evolved. On diluting the solution the amount of ethane decreases, at first very slowly, but finally with great rapidity. Reducing the current has a similar influence. With rise in temperature, the ethane diminishes, and ceases to be formed at 100°. In contradiction to Jahn, the author finds that the employment of a large anode reduces the yield of ethane; the largest yield is obtained with a very small anode; variations in the cathode do not influence the electrolysis. The results of the experiments are illustrated by curves. The author believes that the ethane is formed, not by partial oxidation of acetic acid, but by a simple interaction of the acetions (CH_3COO). He finds that the yields of ethane from equivalent solutions of potassium, sodium, and calcium acetates are equal.—A new method of preparing β-dinaphthylene oxide, and the constitution of its tetra-sulphonic acid, by W. R. Hodgkinson and L. Limpach. Beta-dinaphthalene oxide is obtained by heating 2 : 3′ β-naphthol-sulphonic acid to low redness; the distillate is freed from β-naphthol by extraction with alkali, and the residue crystallized from acetic acid. It crystallizes in rhombic plates, and melts at 153°. On sulphonation it yields a tetra-sulphonic acid, which is identical with the product obtained by the continued action of sulphuric acid on β-naphthol.

Linnean Society, November 5.—Prof. Stewart, President, in the chair.—On behalf of a number of subscribers, Mr. Carruthers presented to the Society a half-length portrait in oils of Sir John Lubbock, Bart., M.P., P.C., F.R.S., a former President, painted by Mr. Leslie Ward; and the remarks which he made on the services rendered to biological science by Sir John Lubbock drew from the latter a graceful acknowledgment of the honour conferred upon him.—Amongst the exhibitions which followed, Mr. E. M. Holmes showed some new marine Algæ from the Ayrshire coast; Mr. J. G. Grenfell showed some Diatoms with pseudopodia, illustrating his remarks with diagrams, upon which an interesting discussion followed.—The President exhibited and made some observations on a tooth of the walrus, which illustrated in a curious manner the periods of growth.—Mr. R. V. Sherring called attention to a large series of framed photographs which had been taken under his direction in Grenada, and illustrated the general character of the West Indian flora as well as the physical features of that particular island.—Mr. J. E. Harting exhibited a specimen of Wilson's Petrel which had been picked up in an exhausted state in the Co. Down on October 2 last, and had been forwarded for inspection by Mr. R. Patterson, of Belfast. Mr. Harting gave some account of the species, and remarked upon the unusual number of Petrels, Shearwaters, Skuas, and other marine birds which had been driven inland to a considerable distance during the recent gales.—A paper was then read by the Rev. Prof. Henslow, entitled "A Theory of Heredity based on Forces instead of any special form of Matter." The author maintained that no special form of matter (as is generally supposed) other than protoplasm is required; the latest discoveries of the organized structure of protoplasm militating against the idea of any other special form of matter. Taking illustrations from the animal and vegetable kingdoms, he inquired why two varieties of chickens fed from the first day to full growth were different? It seemed to him more probable that the results were due to different arrangements of the same kinds of molecules rather than to different kinds of "germ-plasm." *Ranunculus heterophyllis*, he pointed out, produced a "land-form" and a "water-form" according to its environment; it therefore exhibited both "heredity" and "acquired characters." As the materials of its structure were the same in both cases, the different results, he considered, must be due to different arrangements of its molecules, and must be effected by forces. The sudden appearance of stomata on the "land-form" illustrated a case of forces normally "potential" while the leaf is submerged, becoming "actual" when the leaf developed in air. After some further deductions, Prof. Henslow concluded that protoplasm and the forces bound up with it were perfectly able to do all the work of transmitting parental characters, as well as to acquire new characters, which in turn might become hereditary as well.

Physical Society, November 6.—Dr. E. Atkinson, Vice-President, in the chair.—Prof. Sydney Young read a paper on the generalizations of Van der Waals regarding "corresponding" temperatures, pressures, and volumes, in which he gave the results of an investigation made with a view of testing whether these theoretical deductions agree with experimental facts. From his virial equation,

$$\left(p + \frac{a}{v^2}\right)(v - b) = R(1 + \alpha t),$$

Van der Waals showed that, if the absolute temperatures of various substances be proportional to their absolute critical temperatures, their vapour pressures will be proportional to their critical pressures, and their volumes, both as liquid and as saturated vapour, will be proportional to their critical volumes. These deductions have now been put to the test of experiment. Some years ago, Prof. Ramsay and the author published data relating to the temperatures, pressures, and specific volumes of methyl-, ethyl-, and propyl-alcohols, ether, and acetic acid. Since then, experiments have been made on benzene and its halogen derivatives—fluor-, chloro-, bromo-, and iodo-benzene—carbon tetrachloride and stannic chloride, and in a few cases the observations have been carried to the critical points. The critical volumes being in many cases difficult to determine with any exactness, the author, instead of expressing the temperatures, pressures, and volumes of each substance in terms of their critical values, found it necessary to compare the various substances with one of them taken as a standard. Fluorbenzene was chosen as standard on account of the very simple relations observed between the monohalogen derivatives of benzene, and the fact of its critical constants (temperature, pressure, and volume) having been determined with considerable accuracy. Some of the critical constants of the various substances examined are given in the following table, the brackets indicating calculated values—

Substance.	Formula.	Temperatures in °C.	Pressures in mms. of mercury.	Volumes in c.c.'s	
				of a gramme	molecular.
Fluorbenzene ...	C_6H_5F	286·55	33,912	2·43	233
Chlorobenzene ...	C_6H_5Cl	(360)	(33,912)	2·34	(262)
Bromobenzene ...	C_6H_5Br	(397)	(33,912)	1·76	(275)
Iodobenzene	C_6H_5I	(448)	(33,912)	1·47	(298)
Benzene	C_6H_6	288·5	36,395	2·82	219
Carbon tetrachloride	CCl_4	283·15	34,180		
Stannic chloride ...	$SnCl_4$	318·7	28,080		
Ether	$(C_2H_5)_2O$	194·4	27,060		
Methyl alcohol ...	CH_3OH	240·0	59,760		
Ethyl alcohol ...	C_2H_5OH	243·1	47,850		
Propyl alcohol ...	C_3H_7OH	263·7	38,120		
Acetic acid	$CH_3.COOH$	321·6	43,400	2·46	147

Other tables of experimental data—including boiling-points at corresponding pressures, vapour pressures at corresponding temperatures, molecular volumes of liquid and saturated vapours at corresponding pressures and at corresponding temperatures, and ratios calculated therefrom, accompany the paper. From these the author infers: (1) that Van der Waals's generalizations are nearly true for chloro-, bromo-, and iodo-benzene when compared with fluorbenzene; (2) that for benzene, carbon tetrachloride, stannic chloride, and ether, the generalizations may only be taken as rough approximations to the truth; and (3) that for the three alcohols and acetic acid, they do not hold good at all. The tables further show that more consistent results are obtained when the comparisons are made at corresponding pressures rather than at corresponding temperatures, particularly in the case of molecular volumes of saturated vapours. The subject of saturated vapours is also treated by another method. If Van der Waals's deductions were strictly true, then the ratios of the actual densities of the saturated vapours of different substances to their theoretical densities should be equal at corresponding pressures. These ratios have therefore been calculated, and show an approximate agreement amongst benzene and its halogen derivatives, carbon tetrachloride, stannic chloride, and ether. For the other substances the agreement is less satisfactory. It is also noted that the ratio of the actual critical density to the theoretical density is for many substances about 4·4. The alcohols differing so widely from the other compounds, were compared amongst themselves instead of with fluorbenzene, with the result that somewhat closer agreement was found, but the deviations were still far outside the limits of experimental error. Of the critical constants the volumes are the most difficult to determine, because at the critical point the curves connecting temperature and volume, and pressure and volume, are parallel to the axes of volume. Accordingly, the author, in some cases, has deduced this quantity by plotting against temperature the numbers representing the ratios of the molecular volumes both of liquid and saturated vapour to those of fluorbenzene at corresponding temperatures and also at corresponding pressures. Four curves result, which should intersect at the critical temperature, and the point of intersection gives the ratio of the molecular critical volume of the substance to that of fluor-benzene. This method leads to results in fair accord with direct determinations. In the discussion which followed the reading of the paper, Prof. Ramsay said the results proved that Van der Waals's generalizations were only rough approximations, and he suggested that some force had been neglected or a term omitted from the equations. Perhaps the assumption that the molecules are incompressible was not correct. He also strongly protested against the tacit assumption of Van der Waals's laws, and deductions made therefrom, which had recently become so common, particularly in German text-books. Prof. Perry inquired whether the quantities *a*, *b*, and *α*, had been determined for different substances and found to be constant. Prof. Ramsay said that for substances in states analogous to those of perfect gases, the quantities were approximately constant, but when the liquid state was approached this was no longer true. According to Prof. Tait, the two states were not continuous. Prof. Herschel remarked that Prof. Tait had established his law on the assumption that the co-volume is four times the volume occupied by the molecules. This law, he said, had been amply verified by experiments on explosions. Dr. Burton, referring to Prof. Ramsay's remarks on the compressibility of molecules, said the law of force between attracting molecules should be accurately known before any deductions were made; and he pointed out that, at constant volume, the pressure should be proportional to the absolute temperature, if allowance be made for the negative pressure of attraction. Mr. Blakesley, in speaking of molecular forces, said he had observed that, when water is allowed to evaporate from glass, a furrow is formed in the glass, which marks out the original boundary of the liquid. To all appearance, the particles of glass are torn away by the molecular forces acting along the boundary.

Geological Society, Nov. 11.—Sir Archibald Geikie, F.R.S., President, in the chair.—The following communications were read:—On *Dacrytherium ovinum* from the Isle of Wight and Quercy, by R. Lydekker. The author described a cranium and mandible of *Dacrytherium Cayluxi* from the Quercy phosphorites, which proved the identity of this form with the *Dichobune ovina* of Owen from the Oligocene of the Isle of Wight. This species should thus be known as *Dacrytherium ovinum*. It was shown tha tthe mandible referred by Filhol to *D. Cayluxi* belongs to another animal.—A discussion followed, in which Mr. Charlesworth and Mr. E. T. Newton took part.—Supplementary remarks on Glen Roy, by T. F. Jamieson. The author discusses the conditions that preceded the formation of the Glen Roy Lake, and appeals to a rain-map of Scotland in support of his contention that the main snowfall in glacial times would be on the western mountains. He gives reasons for supposing that, previously to the formation of the lake, the valleys of the Lochaber lakes were occupied by ice, and that the period of the formation of the lakes was that of the decay of the last ice-sheet. He supports the correctness of the mapping of the terraces by the officers of the Ordnance Survey, and shows how the absence of the two upper terraces in Glen Spean and of the highest terrace in Glen Glaster simplifies the explanation of the formation of the lakes by ice-barriers. The alluvium of Bohuntine is considered to be the gravel and mud that fell into the lake from the front of the ice when it stood at the mouth of Glen Roy during the formation of the two upper lines. During the last stage of the lake, the ice in the valley of the Caledonian Canal is believed to have constituted the main barrier, whilst the Corry N'Eoin glacier played only a subordinate part. The author suggests the possibility of a *débâcle* during the drop of water from the level of the highest to that of the middle terrace, and in support of this calls attention to the breaking down of the moraines of the Treig glacier at the mouth of the Rough Burn. He believes that when the water dropped to the level of the lowest terrace, it drained away quietly, at any rate until it receded from Upper Glen Roy. In discussing Nicol's objections, he maintains that notches would not be cut at the level of the *cols*, and observes that the discrepancy between the heights of the terraces and those of the *cols* has probably been increased by the growth of peat over most of the ground about the watersheds. The horizontality of the terraces is stated to be a fact, and cases are given where waterworn pebbles are found in connection with the "roads," these being especially noticeable in places where the south-west winds would fully exert their influence, and the structure of the terraces is considered to be such as would be produced at the margins of ice-dammed lakes. Further information is supplied concerning the distribution of the boulders of Glen Spean syenite. These are found on the north side of the Spean Valley, at the height of 2000 feet above the sea and 1400 feet above the river, and fragments of the syenite have been carried towards the north-east, north, and north-west. In an appendix, the author discusses Prof. Prestwich's remarks on the deltas, and his theory of the formation of the terraces. After some remarks from Prof. Bonney and Mr. Marr, the President said he agreed that no explanation that had yet been proposed for the parallel roads of Lochaber was free from difficulties. Yet he had long felt that these were far fewer and less formidable in the glacier theory than in any other. Had the terraces been marine, there ought surely to be similar terraces in some at least of the hundreds of sheltered glens in the Scottish Highlands, where the conditions for their formation and preservation were at least as favourable as in Glen Roy and its adjacent valleys. And though the absence of marine shells in the Lochaber shelves might not be a serious difficulty, it was hard to understand why such shells should not be found in many localities had the whole country been submerged to the height of the highest Glen Roy "road." Then no satisfactory explanation on the marine theory had ever been given of the coincidence of the terraces with well-marked *cols;* while a further formidable objection to this theory lay in the nature and distribution of the detritus of the shelves, which, in his opinion, was very unlike material arranged in a tidal sea, but was quite what might be looked for in a freshwater lake. He thought that the author's present paper lessened some of the difficulties of the glacier theory by simplifying the grouping of the ice-dams. There still remained the objection that if the Great Glen and the valleys round Ben Nevis were choked up with ice, Glen Roy and its neighbours could hardly have been filled with water. But this difficulty, which every glacialist must have felt, was probably more formidable in appearance than in reality. As Mr. Marr had pointed out, conditions did actually now exist in Greenland very similar to those which, according to the theory so ably expounded by the author, formerly existed in Lochaber.

Royal Meteorological Society, November 18.—Mr. Baldwin Latham, President, in the chair.—Mr. R. H. Scott, F.R.S., gave an account of the proceedings of the International Meteorological Conference, which was held at Munich from August 26 to September 2.—The following papers were also read:—Account of an electric self-recording rain-gauge, by Mr. W. J. E. Binnie. This is a very ingenious instrument, and has been constructed on the assumption that all drops falling from an orifice or tube are identical in weight, as long as the dimensions of the orifice are not varied.—On wet and dry bulb formulæ, by Prof. J. D. Everett, F.R.S. This is a criticism of the methods investigated some years ago by M. August and Dr. Apjohn for determining, by calculation, the maximum vapour tension for the dew point from the temperatures of the dry and wet bulb. Prof. Everett also criticizes the values adopted by Regnault, and says that, in presence of the uncertainty as to a rational formula, he thinks Mr. Glaisher did wisely in constructing his table of factors, which give the dew point approximately by the most direct calculation which is admissible. The inherent difficulties of hygrometric observation and deduction are great, and have not yet been fully overcome.—Results of meteorological observations made at Akassa, Niger Territories, May 1889 to December 1890, by Mr. F. Russell, F.R.G.S. This is in continuation of a former communication respecting the same place. After detailing the results of the various observations, the author says that this period was very unhealthy, and the year 1890 especially so. The weather was exceptionally dry, with small-pox and phthsis amongst the native population. The West Coast reports generally were also unfavourable in reference to the condition of resident Europeans, and at the principal ports quarantine regulations were put in force consequent upon an outbreak of yellow fever in places situated to the south-west. At Bonny ten deaths occurred from November to February out of a population of some sixteen Europeans.

SYDNEY.

Royal Society of New South Wales, September 2.—H. C. Russell, F.R.S., President, in the chair.—The following papers were read:—On a wave-propelled vessel, by Lawrence Hargrave.—Notes on a spontaneous disease among Australian rabbits, by M. Adrien Loir.—Notes on recent celestial photographs, by H. C. Russell, F.R.S.—Some folk-songs and myths from Samoa, by Rev. G. Pratt and Dr. John Fraser.—A quick filter without the aid of pumps was exhibited and described by W. M. Hamlet.

October 7.—H. C. Russell, F.R.S., President, in the chair.—The following papers were read:—Notes on the use, construction, and cost of service reservoirs, by C. W. Darley.—Dr. Fraser presented some myths and historical records from Samoa. The myths had reference to an ancient practice of offering every day a human sacrifice to the sun, and to a chief called "Malietoa the Fierce," and showed how that was stopped. The histories were chiefly genealogies of the kings of Manu'a.

PARIS.

Academy of Sciences, November 16.—M. Duchartre in the chair.—On the secular acceleration of the moon and the variability of the sidereal day, by M. F. Tisserand. From the author's investigation it seems that the increase in the length of the day produced by tidal action is almost the same in amount as the decrease resulting from the secular contraction of the earth due to its cooling, so the length of the sidereal day remains practically invariable.—On the research on the number of roots common to several simultaneous equations, by M. Émile Picard.—On the law of intensity of light emitted by phosphorescent bodies, by M. Henri Becquerel. A correction to a formula given in a previous paper.—The heat of formation of hydrazine and of hydrazoic acid, by MM. Berthelot and Matignon. *Hydrazine*—(1) Heat of solution of hydrazine sulphate at 10°·6 is − 8·70 cal. per molecule. (2) Heat of neutralization: (*a*) by sulphuric acid + 5·55 cal. per equivalent; (*b*) by hydrochloric acid + 5·2 cal. per equivalent. Hydrazine is therefore a weak base comparable to ferric oxide. (3) Heat of combustion of 1 mol. crystallized hydrazine sulphate = + 127·7 cal. (4) Heat of formation $\frac{1}{2}N_2H_4$ = − 4·75 cal. *Hydrazoic acid*—(1) Heat of solution of ammonium salt, = − 7·08 cal. per mol. (2) Heat of neutralization: (*a*) by baryta water + 10·0 cal.; (*b*) by ammonia + 8·2 cal. Hydrazoic acid, dilute, is comparable to amidobenzoic acid, superior to hyponitrous acid. (3) Heat of combustion of the am. salt + 163·8 cal. per mol. at constant volume, and + 163·3 cal. at constant pressure, by explosion in compressed oxygen. (4) Heat of formation of am. salt: (*a*) crystallized − 25·3 cal.; (*b*) in solution − 32·3 cal. Heat of formation of the free acid in dilute solution = − 61·6 cal.—Oxidation of nickel carbonyl, by M. Berthelot. (See Notes.)—Tables of Vesta, by M. G. Leveau. A comparison of the meridian observations made of the minor planet Vesta, from January to April 1890, with positions given in the *Nautical Almanac*, and in an ephemeris computed by means of M. Leveau's tables of this planet. The tables are founded on 5000 meridian observations made between 1807 and 1888, and the masses taken for Jupiter and Mars, respectively, are 1/1045·63 and 1/3,648,000. The mean differences of position are greater in the *Nautical Almanac* than in M. Leveau's ephemeris, both in right ascension and declination.—On secular variations of eccentricities and inclinations, by M. J. Perchot.—On linear differential equations, by M. André Markoff.—On the dielectric power, by M. Julien Lefebvre. From experiments described, the following mean dielectric constants have been derived: sulphur (flower and roll), 2·6; sulphur, cast in rolls six months previously, 3·9; ice, different specimens, 3·45 and 2·4; ebonite, 2·3; paraffin, brown, 2·1, white, 2·0; petroleum, 1·9; carbon bisulphide, 1·7; spirits of turpentine, 1·5. The results agree fairly well with those obtained by Gordon. M. Lefebvre also finds that the dielectric constant of sulphur increases with the time.—On an application of photography to the polariscope, by MM. Chauvin and Charles Fabre.—Action of light on ruthenium peroxide, by M. A. Joly.—Salts formed by oxygen compounds of ruthenium inferior to ruthenic and heptaruthenic acids, by M. A. Joly.—On the iodonitroso and bromonitroso compounds of platinum, by M. M. Vèzes.—On the coloration of solutions of cobalt, and the state of its salts in solution, by M. A. Étard.—The nitration of silk, by MM. Léo Vignon and P. Sisley.—On the implanting of large pieces of decalcified bone to fill up losses of the substance of the skeleton, by M. Le Dentu. It has been found that pieces of decalcified bone substituted for a portion or the whole of a diseased bone plays the part of a temporary support which, before disappearing, allows the periosteum or the osseous tissue sufficient time to reconstruct a new bone.—On some phenomena of reproduction of Cirrhipedes, by M. A. Gruvel.—On the age of the fauna of Samos, by Mr. Forsyth Major.—On a Neolithic flint working (*exploitation*) of a new type, by M. Armand Viré.

BERLIN.

Physiological Society, October 30.—Prof. du Bois-Reymond, President, in the chair.—Prof. Gad reported on experiments, conducted under his direction by Dr. Schtscherbak, on the alteration of the movements of eating in rabbits which result from removal of certain parts of the cerebrum.

Meteorological Society, November 3.—Prof. Schwalbe, President, in the chair.—Dr. Zenker spoke on the relationship of solar radiation, as it would really occur if the sun were directly overhead and there were no atmosphere, to the actually existing and observed temperatures of stations, taking into account their proximity to oceans and continents.—Prof. Hellmann made a short communication on the recent experiments in America on the artificial production of rain.

Physical Society, November 6.—Prof. du Bois-Reymond, President, in the chair.—Dr. Raps explained certain modifications which he had introduced into his automatic mercurial air-pumps, and demonstrated the action of the pump on a Geissler tube, which he rapidly exhausted so completely that a phosphorescent light made its appearance in it.—The President made some remarks on photographs of the human retina.—Prof. Kundt described Dr. Zehnder's new and simple differential-refractor, an instrument by means of which the two rays destined to produce interference may be kept some 50 to 100 cm. apart, and be subjected separately to varying experimental conditions.

DIARY OF SOCIETIES.

LONDON.

THURSDAY, NOVEMBER 26.

ROYAL SOCIETY, at 4.30.—On Instability of Periodic Motion: Sir William Thomson, Pres.R.S.—A New Mode of Respiration in the Myriapoda: F. G. Sinclair.—Further Observations on the Gestation of Indian Rays: J. Wood-Mason and A. Alcock.—On some Variations observed in the Rabbit's Liver under certain Physiological and Pathological Circumstances: Dr. Brunton, F.R.S., and Dr. Delépine.—On the Electromotive Phenomena of the Mammalian Heart: W. M. Bayliss and Dr. E. H. Starling.

INSTITUTION OF ELECTRICAL ENGINEERS, at 8.—Description of the Standard Volt and Ampere Meter used at the Ferry Works, Thames Ditton: Captain H. R. Sankey (late R.E.) and F. V. Andersen.

LONDON INSTITUTION, at 6. On the Spread of Commerce in Europe in Prehistoric Times: Prof. Boyd Dawkins, F.R.S.

CAMERA CLUB, at 8.30.—Some Analogous Aspects of Painting, Music, and Poetry (Musical and Pictorial Illustrations): Rev. F. C. Lambert.

FRIDAY, NOVEMBER 27.

INSTITUTION OF CIVIL ENGINEERS, at 7.30.—Modern Railway Carriages: Walter Clemence.

CAMERA CLUB, at 8.—Retouching: Redmond Barrett.

SATURDAY, NOVEMBER 28.

ROYAL BOTANIC SOCIETY, at 3.45.

ESSEX FIELD CLUB, at 6.30 (at Loughton).—On some Ancient Lake Remains at Felstead, with Notes on other similar Remains in the District: J. French.—The Life-History of the Hessian Fly: F. Enock.

SUNDAY, NOVEMBER 29.

SUNDAY LECTURE SOCIETY, at 4.—How came the Great Ice Age? (with Oxyhydrogen Lantern Illustrations): Sir Robert S. Ball, F.R.S.

MONDAY, NOVEMBER 30.

ROYAL SOCIETY, at 4.—Anniversary Meeting.

SOCIETY OF ARTS, at 8.—The Pigments and Vehicles of the Old Masters: A. P. Laurie.

ROYAL MICROSCOPICAL SOCIETY, at 8.—*Conversazione.*

ARISTOTELIAN SOCIETY, at 8.—Croll's Philosophical Basis of Evolution: Arthur Boutwood.

LONDON INSTITUTION, at 5.—Recent Progress in Astronomy (Illustrated): Sir Robert Ball, F.R.S.

CAMERA CLUB, at 8.30.—Lantern Evening.

TUESDAY, DECEMBER 1.

ZOOLOGICAL SOCIETY, at 8.30.—Notes on Transcaspian Reptiles: G. A. Boulenger.—Further Descriptions of New Butterflies from British East Africa, collected by Mr. F. J. Jackson during his Recent Expedition, Part II.: Miss E. M. Sharpe.—On the Association of Gamasids with Ants: A. D. Michael.—Notes on the Bornean Rhinoceros: Edward Bartlett.

INSTITUTION OF CIVIL ENGINEERS, at 8.—Monthly Ballot for Members.—Renewed Discussion on Portland Cement and Portland-Cement Concrete: Messrs. Bamber, Carey, and Smith.

WEDNESDAY, DECEMBER 2.

SOCIETY OF ARTS, at 8.—Secondary Batteries: G. H. Robertson.

ENTOMOLOGICAL SOCIETY, at 7.—Notes on Lycæna (*recte* Thecla) Rhymnus, Tengstræmii, and Pretiosa: George T. Baker.—The Effects of Artificial Temperature on the Colouring of Vanessa urticæ and certain other Species of Lepidoptera: Frederic Merrifield.—On the Variation in the Colour of the Cocoons of Eriogaster lanestris and Saturnia carpini: W. Bateson (communicated by Dr. D. Sharp, F.R.S.).

THURSDAY, DECEMBER 3.

CHEMICAL SOCIETY, at 8.—Ballot for the Election of Fellows.—Phosphorus Oxide, Part II.: Prof Thorpe, F.R.S., and A. E. Tutton.—On Frangulin, Part II.: Prof. Thorpe and Dr A. K. Miller.—The Structure and Character of Flames: A. Smithels and H. Tingle.—The Composition of Cooked Vegetables: Miss K. J. Williams.—On the Occurrence of a Mydicatic Alkaloid in Lettuce: T. S. Dymond.—On some Metallic Hydrosulphides: S. E. Linder and H. Picton.—On the Physical Constitution of some Solutions of Insoluble Sulphides: Harold Picton.—Solution and Pseudo-Solution: H. Picton and S. E. Linder.

LINNEAN SOCIETY, at 8.—A Contribution to the Freshwater Algæ of the West of Ireland: W. West.—The Tick Pest in Jamaica: Dr. W. H. W. Strachan.

LONDON INSTITUTION, at 6.—The Tower of Babel and Confusion of Tongues (Illustrated): Theo. G. Pinches.

CAMERA CLUB, at 8.30.—On Toning Bromide Paper and Transparencies (with Demonstration and Illustrations): A. R. Dresser.

FRIDAY, DECEMBER 4.

PHYSICAL SOCIETY, at 5.—On a Permanent Magnetic Field: W. Hibbert.—Note on the Production of Rotatory Currents: Prof. Ayrton, F.R.S.

GEOLOGISTS' ASSOCIATION, at 8.—Supplementary Observations on some Fossil Fishes from the English Lower Oolites: Arthur Smith Woodward.—Organic Matter as a Geological Agent: Rev. A. Irving.

INSTITUTION OF CIVIL ENGINEERS, at 2.—Students' Visit to inspect the New Refrigerating Plant at Nelson's Wharf, Commercial Road, Lambeth.

CAMERA CLUB, at 8.—Retouching: Redmond Barrett.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

BOOKS.—Leçons sur les Métaux, 2nd fasc.: A. Ditte (Paris, Dunod).—Hand-book to the Geology of Derbyshire, 2nd edition: J. M. Mello (Bemrose).—Annals of British Geology, 1890: J. F. Blake (Dulau).—The Ouse: A. J. Foster (S.P.C.K.).—Hand-book of Psychology—Feeling and Will; J. M. Baldwin (Macmillan).—Index-Catalogue of the Library of the Surgeon-General's Office, U.S. Army, vol. xii. (Washington).—Electricity tested Experimentally, 3rd edition: L. Cumming (Longmans).—Problems in Chemical Arithmetic: E. J. Cox (Percival).—An Account of British Flies, vol. i. Part 2: F. W. Theobald (Stock).—A Treatise on the Geometry of the Circle: W. J. McClelland (Macmillan).—Beast and Man in India: J. L. Kipling (Macmillan).—Principles of Agriculture: edited by R. P. Wright (Blackie).—Elementary Inorganic Chemistry, new edition: A. H. Sexton (Blackie).—Euclid, Book XI.: A. E. Layng (Blackie).

PAMPHLETS.—Water and Water-Supply: J. Hopkinson (Hereford).—History of Liberia: J. H. T. McPherson (Baltimore).—The Nuptial Number of Plato: J. Adam (Clay).

SERIALS.—Zeitschrift für Wissenschaftliche Zoologie, 53 Band, 1 Heft (Williams and Norgate).—Cyclone Memoirs, Part 4; W. L. Dallas (Calcutta).—Journal of the Anthropological Institute, November (K. Paul).—Government of India Meteorological Department, Monthly Weather Review—March and April 1891 (Calcutta).—Indian Meteorological Memoirs, vol. iv., Part 7 (Calcutta).

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