THURSDAY, JANUARY 3, 1895.

BIOLOGICAL LECTURES AND ADDRESSES.

Biological Lectures and Addresses delivered by the late Arthur Milnes Marshall, M.A., M.D, D.Sc., F.R.S. Edited by C. F. Marshall, M.D., B.Sc., F.R.C.S. (London: Nutt, 1894.)

MANY of us remember the pleasure experienced in listening to the vigorous addresses of Prof. Milnes Marshall, whose sudden death, about a year ago, was such a blow to biologists and to all interested in the spread of scientific education. In reading them now, the energy and humour of the speaker are ever present in the memory, giving life to the apt illustrations and clearly expressed thoughts.

The choice of appropriate subjects for the occasion of these addresses shows remarkable discrimination. It is quite clear that the late Prof. Milnes Marshall believed that a single address—if heard at the right point in his career, and delivered with the confidence of this belief—might change the whole attitude of mind with which a student approached his subject.

Lecturing to the Owens College Medical Students' Debating Society, he chose as his subject "Embryology as an aid to Anatomy." In preparing to address a class of students expected to deal with, and to remember, in the course of their daily work, innumerable details which are as yet far from receiving a scientific interpretation, he selected embryology, which "offers an explanation of many otherwise completely unintelligible anatomical facts"; and by the example of the development of the nerves supplying the muscles of the eye, the thoughts of many young anatomists may well have been turned into a direction which was of the most inestimable benefit to their study.

Equal wisdom and foresight were shown in the selection of subjects for addresses to the Members of the Manchester Microscopical Society, viz. "Inheritance" (1888), "The Shapes and Sizes of Animals" (1889), "Some Recent Developments of the Cell Theory" (1890), and "Death" (1893). In the choice of these subjects, and in their treatment, the Members of the Society are shown that the most profound biological problems are to be approached and, perhaps, solved by the study of the most minute detail and the lowest forms of life. The student thus made fully aware of the dignity and possibilities of his subject, will not be likely to forget, in the patient investigation of histological or biological detail, the wide issues which are at stake.

The subjects chosen for presentation to other audiences are just as happy. Sometimes, as in "The Theory of Change of Function," a difficulty in evolution by natural selection is taken as the subject, and the final solution given in the clearest and simplest manner. It would be well if this address were widely known, for the difficulty it deals with is still frequently raised, just as if no explanation had ever been forthcoming. In other cases, wide questions, such as "The Influence of Environment" or "Animal Pedigrees," form the subjects of the addresses.

The addresses abound in humorous and apt illustrations. Thus, on p. 39, in order "to show that, even in our worldly transactions, changes of environment often produce not only direct and immediate changes and re-adjustments, but also definite and calculable ones," he gives as an example the following:—"Let *a* be a merchant, and *b* his purse: the combination *ab* will at once strike you as a natural and stable one. . . . Now let *c* be a highwayman, and *d* his pistol: the combination *cd* is again recognised as a natural and stable one. Now, bring the compound *ab* into the presence of the compound *cd*, and mark how the stability of the former is shaken. . . . The several elements become rearranged in a manner that finds perfect expression in the formula—

$$ab + cd = a + bcd."$$

Again, in order to illustrate the tendency towards reversion to an ancestral condition, the card house is selected—

"The resulting structure is a far more imposing one than the pack of cards when laid flat on the table, but it is also an eminently unstable one, its instability being directly proportional to the extent to which it departs from its initial condition." (p. 104.)

"The influence of food yolk on development" is compared to "that of capital in human undertakings" (p. 224), the metaphor being worked out in an interesting and amusing manner. Prof. Weismann's views on the absence of death in Protozoa are illustrated on p. 273 as follows:—

"If the original Amœba be called Tom, and the products of fission Dick and Harry, the upshot of the process may be expressed by saying that Tom has disappeared without having died, while Dick and Harry have come into existence without having been born. Nothing has died, there is no corpse to bury, and our ordinary ideas with regard to individuality and identity fail altogether to afford answer to the question—Where is Tom at the end of the process?"

There are a few sentences in the addresses which are perhaps capable of misconstruction. Prof. Milnes Marshall appears to have hesitated to accept the belief in the hereditary transmission of acquired characters, and yet there are some statements which seem to imply this belief. This is the case with the statements that the white man and the negro have been evolved "through the long-continued action of selection and environment" (pp. 247 and 358), that modifications of development have occurred "due chiefly to mechanical causes" (p. 316), that the "larger size of the eggs of fresh-water forms appears to be dependent on the nature of the environment" (p. 313), although in this case it is clearly shown on the following page that environment is believed to act selectively and not directly.

The term "acquired character" is made to bear still further burdens in the way of special interpretations. It was unfortunate so late as 1890 to continue to speak of the distinction in development "between those characters which are really historical and inherited, and those which are *acquired* or spurious additions to the record" (p. 307), or to speak of the view that Amphioxus and the Cyclostomes are "degenerate animals—whose simplicity is *acquired* and deceptive rather than real and ancestral." (p. 335.)

The book is well and clearly printed, and has been extremely well edited. The only printer's error noted was in the title of Mr. Oldfield Thomas' paper on p. 318.

E. B. P.

TEXT-BOOK OF AGRICULTURE.

Advanced Agriculture. By H. J. Webb, Ph.D., B.Sc. (Lond.). 8vo., pp. vi. and 672. (London: Longmans, Green, and Co., 1894.)

TEXT-BOOKS of agriculture are rapidly increasing in number. The name of the author on the title-page of this is that of the late Principal of the Agricultural College at Aspatria, Cumberland, who, most unfortunately, was unable to complete the work. He was struck down at a comparatively early age, and in the midst of hard work and a successful career. He is most highly spoken of, and deservedly so, in the preface by the editor of the book, Mr. J. Lister, of Aspatria. From such hands we anticipated much and good work, though the task might appear heavy. We regret to say that we have been greatly disappointed.

We are told that—

"This work, though primarily intended for the advanced stage of the Science and Art Departments' Examination in Principles of Agriculture, will also cover the greater part of the syllabus of the Honours stage. Care has, however, been taken not to adhere too rigidly to the syllabus in question, and we trust it may be found equally serviceable for the examination for the diploma in Agriculture of the Highland and Agricultural Society in Scotland, and the senior examination of the Royal Agricultural Society of England."

Even the fact that the book has been prepared mainly to assist students to pass examinations, can hardly excuse some of the statements we meet with. In agricultural geology we are told, among other scraps of information that zeolites are "hydrated silicates of alumina or lime." This is scarcely true. "Diorite consists of plagioclase and hornblende." This is true, but as the meaning of plagioclase is nowhere explained, it does not help the student much. In agricultural physics we learn that "a heavy soil might contain absolutely double the amount of phosphoric that a light soil did, although it would show only the same percentage if it were twice as heavy." We have tried to believe this, but till now we do not understand it. In engineering, a definition of horse-power is given without any mention or suggestion of *time* as a possible factor.

In chemistry, Rendonda [Redonda] and Alta Vela phosphates are said to contain a large quantity of *alum;* leather to contain 4½ to 9 per cent. of *ammonia*, and meat-meal and meat-guano to contain much *ammonia.* This is the more confusing, as in other manures, mentioned on the same page, the percentage of *nitrogen* is correctly given; but this confusion of ammonia with the ammonia equivalent of the contained nitrogen is common throughout. Ammonium chloride is said to contain about 18½ per cent. of water and 32½ per cent. of ammonia. The formula of monocalcic phosphate is correctly given as $CaH_4P_2O_8$, and tricalcic phosphate as $Ca_3P_2O_8$, and, yet, in one analysis 18·01 per cent. of the former is said to be equal to 28·1 per cent. of the latter, and in another case 19·2 per cent. of monocalcic phosphate is said to be equal to 30·07 per cent. of tricalcic phosphate made soluble: of course the so-called monocalcic phosphate in these analyses is calcium metaphosphate, formerly known as "biphosphate"; but how can the student know this? Cellulose is described as a white amorphous powder.

In agricultural botany, we are told that "protoplasm generally presents itself as a granular semi-fluid substance with or without a cell-wall." "The corolla, when present, usually consists of green leaves or sepals, sometimes scarcely noticeable." This later statement is, of course, only a misprint; but it is also only an example of several similar serious misprints.

The root residues of crops remaining in the soil are said to "consist very largely of protoplasm." The section on farm crops, however, in which this occurs, is written in a curious style, somewhat difficult to follow.

On looking up information regarding anthrax, we find that "although a disease of the blood, the writer [in this case Mr. H. Thompson] considers it more of a dietetic nature, having seen it produced from steeped brewers' grains allowed to stand till they had reached the acetous stage of fermentation. It is also produced by the hay bacillus, obtained from the fermentation of chopped hay and from mouldy cotton-cakes, more particularly the undecorticated variety. He has also seen it arise in certain undrained lands. Although very fatal to other animals, such as dogs, cats, and poultry, that may have eaten the flesh or blood, yet he considers it neither infectious nor contagious, having never known it to extend beyond the buildings in which it originated. Again, the disease was always traceable to some peculiarity of the feeding, and the writer thinks that it is analogous to an aggravated form of *red water*." And this stuff is set forth as *advanced* agriculture. There are several very startling statements in the chapter on veterinary science; *e.g.* in retention of the fœtus, "different bones belonging to the fœtus, such as jaw, scapula, ribs, humerus, and several others, are passed at times through the rectum."

In the chapter on agricultural entomology, we hardly suppose the author is serious when he recommends the use of rape-cake, at the rate of two or three tons per acre, to clear the field of wire-worms.

In part ii. of the book the misstatements are perhaps not quite so numerous or so serious, but they are not absent. In the chapter on permanent pastures, after a notice of the power which leguminous plants, with the aid of the low organisms present in the nodules on their roots, possess of appropriating free nitrogen, we are told that "this fact of the nitrogen-storing power of the leguminous plants explains the action of heavy dressings of nitrogenous manures on permanent pastures encouraging the growth of grasses, especially the coarser ones, at the expense of the clovers." We fancy that this *explanation* of the fact will not satisfy many readers.

The work of Rothamsted is frequently referred to and fully appreciated; but why should Rothamsted be spelt in three different ways in the book? Probably, however, every would-be advanced agriculturist knows that there are not three Rothamsteds.

But we have given enough instances of what are very serious flaws in the book. Much of the matter is exceedingly good and useful; but does not this really add to the danger of the work in the hands of the learner who is not in a position to sift the wheat from the chaff and the weeds?

We do not think that this book will serve to advance the reputation of its author, nor to advance agriculture. We submit that "advanced agriculture" is not agriculture *plus* a smattering of chemistry, a dip into geology, a pinch of botany, a skim of entomology, a sniff at meteorology, and so on; even if the sciences be correctly expounded. The text-book of agriculture, like that of other subjects, has no doubt to pass through stages of evolution, and we trust it will not long remain at the stage indicated by this book.

EWING ON THE STEAM ENGINE.

The Steam Engine and other Heat Engines. By Prof. J. A. Ewing, M.A., B.Sc., F.R.S., M.Inst.C.E. (Cambridge: The University Press, 1894.)

ENGINEERING students and others will welcome the present volume as one likely to increase their knowledge of an important branch of engineering, from the pen of an acknowledged master of the science; any work by Prof. Ewing is sure to be read by engineers generally, and treated as a book for constant reference.

As is well known, Prof. Ewing wrote some valuable articles for the "Encyclopædia Britannica" on this subject, and it is an expansion of these articles which constitutes the basis of this work. As a University text-book this volume will fill a great want, treating as it does, from the theoretical side, a subject only descriptively dealt with in the majority of such text-books. As the author remarks: "The endeavour throughout has been to make evident the bearing of theory on practical issues."

The first six chapters may be said to contain the early history of the steam engine, and a scientific treatment of the general behaviour of steam in the cylinder, as well as the general theory of heat engines. The information thus brought together is of a valuable nature, and the references which are made, add considerably to the usefulness of the work. To the thoughtful practical engineer, this portion of the book will form a perfect mine of matter for careful consideration.

The testing of steam engines has of late years become a common occurrence, thanks to Profs. Kennedy, Osborne Reynolds, and many others. Designers and manufacturers of such engines have everything to gain by such experiments; few being, like the late Mr. Willans, capable of carrying out scientifically accurate trials of their own engines. Chapter vi. deals with this important question. Many useful hints are given, and sources of error carefully pointed out. As indicator diagrams play such an important part in the trials of steam engines, it is interesting to note that the Crosby Company's modified form of Richards' Indicator is considered by Prof. Ewing to be one of the best.

Compound expansion comes in for very full and accurate treatment; many sets of indicator diagrams are explained and illustrated, and the difficult matter involved in the combination of such diagrams is lucidly dealt with. Of the advantages of compound expansion in the use of high-pressure steam, we are told much, and also of the mechanical advantages of such an arrangement. We cannot help pointing out that, in this matter, the practical men on the Clyde were singing the praises of compound engines before the theoretical men would admit of their utility or economy. On valves and valve gears our author has much to say, although we notice nothing remarkable in the chapter. Locomotive engineers do not trust entirely to the drawing-board or calculation in the design of valve gears. It is now the practice to try the proposed gear as a full-size model on the valve gear testing machine, and so to obtain the best results. The latest machine of this kind has been erected in the drawing-office of Messrs. Sharp, Stewart, and Co., the eminent locomotive engineers, of Glasgow. The saving of time is great, and very accurate results are obtained. In fact, in such drawing-offices, the old valve diagrams, with their many curves, are things of the past, and a simple table of leads, cut-offs, suppressions, &c., for the different degrees of expansions, has taken their places.

Chapter ix. treats of the many forms of governors used for regulating the work done in the steam engine, commencing with Watt's simple arrangement, and finishing with the differential or dynamometric governors invented by the late Sir W. Siemens; and further on in the book we find much useful information concerning the work on the crank shaft. Diagrams of crank efforts are given, and the effect of friction and of the inertia of the reciprocating pieces are duly discussed. The balancing of machinery is an all-important subject, in fact the life of any machine depends upon the balancing of its moving parts; for this reason, we are sorry to see that Prof. Ewing has so little to say on this subject generally, and particularly on the balancing of locomotives. If theory is of any help to the locomotive engineer at all, surely it could be best applied in balancing; some engineers balance the whole reciprocating weight, others none; the majority about 30 per cent.: which is right Prof. Ewing does not help us, but observes that "the final adjustment of the balancing masses is usually a matter of experiment, the locomotive being hung in chains to allow its oscillation to be observed"; this, to say the least of it, is never done in this or any other country.

The production of steam is the subject treated in the following chapter; the illustrations of the different boilers, with the descriptions, are excellent. The only specimens of water-tube boilers illustrated are the Thornycroft and the Babcock and Wilcox, the latter used principally for stationary engines. The Thornycroft boiler is the forerunner of similar types; for instance, the Yarrow and the Clyde among others, all of which are considerably lighter than the ordinary marine boiler. As an example of the locomotive boiler, one of the London and North-Western Railway Company is taken as typical of British practice. As an example of an injector for feeding boilers, an old-fashioned type of Giffard injector is illustrated, but more recent types are described. Mechanical stoking and the use of liquid fuel are also mentioned.

The following chapters conclude the work, occupying some sixty pages with descriptions of forms of steam engines, air, gas, and oil engines. The Willan's central valve engine is well illustrated, and the description does credit to what is probably the most economical steam engine ever designed. A description is given of rotary engines, but none are illustrated. The "Rota" engine, designed and made by MacEwan Ross, of Glasgow, might have been included with advantage. The locomotive is outlined, and the compound type described; but no information as to tests is given, probably because no trustworthy data can be obtained; and as no British Railway Company, with one possible exception, is likely to build any more compound engines, it seems probable they are not the unqualified success they were originally claimed to be, although the Vauclain system, with four outside cylinders, appears to be a success in the States. But it must not be forgotten that the American rival is a very uneconomical engine when compared with our own.

N. J. L.

OUR BOOK SHELF.

Das Verhältniss der Philosophie zu der empirischen Wissenschaft von der Natur. By David Wetterhan. (Leipzig: W. Engelmann, 1894.)

THIS is the essay which gained the prize of 1000 marks offered, in 1891, by the Philosophical Society of Berlin. It consists of 110 pp., of which about twenty are occupied by notes and abstracts from various writers, in small print.

Naturally, in giving forth his own views, some of which possess considerable originality, the author makes continual and extensive use of the theories of Kant, Schopenhauer, Wundt, Bunge, and others; and one noticeable feature about the work is the full share of recognition accorded to English philosophers and scientists, such as Faraday, Herbert Spencer, Darwin, Romanes, and Huxley. The writer well remarks that the limits of scientific knowledge are everywhere and nowhere.

In the earlier pages the author discusses the relation between the physical and the psychical sides of nature. The theory of the conservation of energy has nothing to do with mental processes: it governs the quantitative relations of all processes of nature, but does not explain their qualitative differences. Sensation, consciousness, motor impulse, are not forms of energy, and do not correspond to them, but to the causes of qualitative changes in forms of energy.

The world of psychics cannot be separated from that of physics, and we must look forward to the future progress in the latter science to bring the qualitative changes into connection with the theory of the conservation of energy. The author shows by a very simple example—"Shall I kill that spider, or leave it alone?"—the effect of his will on surrounding nature; and the divergent effects thereon which would result from each of the wo alternative modes of procedure.

Memory he believes to be caused by an impulse of a certain kind, producing in the particular arrangement of the smallest particles in the ganglion cells and nerve fibres a modification in the same direction as was produced by the original impulse, and resulting in corresponding physical phenomena. But he acknowledges that, at present, we cannot explain "brain oscillations."

The principle of evolution sheds a light upon the psycho-physical problem: physical development is not the cause but the effect of psychical development, and the modifications in the brain and nervous system throughout the animal kingdom are intelligible as resulting from psychical causes, whereas the physical causes, if they exist, remain hidden. He considers that even in palæontology we can detect traces of this psycho-physical process by the examination and comparison of the cranial capacity of the skulls of extinct reptiles and mammals. As man is the culminating point in mental development amongst mammals, so is the ant amongst insects; but clearly this position has in each case been attained independently, and is independent of the structure of the nervous system. The inheritance of acquired characters is discussed, and the old difficulties presented by a disbelief in it are once more brought forward; and especially the difficulty in the adaptation of terrestrial mammals to a life in water, such as must have occurred in the ancestors of the Cetacea. The author endeavours to show that the principle of progressive psycho-physical development may admit of a vital-mechanical explanation, if the transference of acquired characters, as a consequence of changed functions, is possible for "keimplasma."

The author is apparently a practical man of science, and not a mere arm-chair philosopher; he fully recognises that philosophy must be based upon scientific experiments, and quotes Huxley's words, "The Laboratory is the fore-court of the temple of Philosophy."

Meteorology, Practical and Applied. By John William Moore, B.A., M.D., M.Ch., F.R.C.P.I. (London: F. I. Rebman, 1894.)

IT is to be hoped that this little book may meet with the popularity it deserves. Well written and well illustrated, it ought to recommend itself to that numerous class of whom some knowledge of meteorology is now required. The author, a medical practitioner, has evidently, first of all, but by no means exclusively, sought to interest medical officers of health and those who seek a qualification in preventive medicine and its allied branches. Writing for such students, the author has prudently not burdened his work with technical terms, or attempted to discuss with any completeness the general motions of the atmosphere depending upon the application of thermodynamics. Neither does he fall entirely into the popular and pleasing style of writing; though he does seek legitimate interest by exhibiting the many points in which meteorological inquiry bears on social and sanitary science, how it may benefit the agriculturist, protect the traveller, or instruct the physician.

The book is divided into four sections. In the first we find a very full and, considering the source from which it is drawn, probably accurate account of the history and development of the United States Weather Bureau. It seems to have occurred to the author, that if he shows to the reader at an early stage the interest and devotion which the shrewd American gives to this subject, he will convince him that there is something in meteorology after all, beyond the dreary and wearisome accumulation of barometer and thermometer readings. Then we have, of course, the description of the necessary instruments in use, with their corrections. We are glad to see in this section due prominence given to Mr. Aitken's interesting work on atmospheric dust; and in the chapter on evaporation we notice that Mr. Apjohn's formulæ are given correctly, which is not the case in some other well-known elementary works. The third section of the book treats of climate and weather, a section that might with advantage have been made fuller; but in reviewing the whole subject of meteorology within moderate compass, it is necessary to curtail somewhere. The last section considers the influence of season and weather on disease. Here the author is apparently on very familiar ground, and the small space devoted to this topic is full of interest and suggestion. There are one or two slips in the text, as, for instance, on page 10, where the oft-repeated error is once again seen, of mistaking the axis of rotation of the earth for the plane of the equator; but such oversights are easily excused in presence of the collection of a large number of facts, well arranged and tersely expressed. W. E. P.

The Province of South Australia. By J. D. Woods, J.P. With a Sketch of the Northern Territory, by H. D. Wilson. Pp. 446. (Adelaide: C. E. Bristow, 1894.)

THIS account of the province of South Australia, from its discovery to the end of 1892 was, the preface informs us, written under the authority of the Government of the Colony. It may therefore be taken as an authoritative work of quite a different and a better kind than the many descriptions of Australia that have appeared during the past few years. The physical features, fauna, flora, climate and meteorology are fully described, and the story of the explorations of the interior of the continent is full of interest. There is a chapter on the agriculture of South Australia, and one on the minerals in which the province is so wonderfully rich. Those familiar with the history of education in South Australia will remember that prior to 1874 the colony did not possess a university. It was in 1872 that an endowment of £20,000, given by Sir W. W. Hughes, was applied to the founding of two professorships—one for classics and comparative philology and literature, and the second for English language and literature and mental and moral philosophy. Science was benefited shortly afterwards by a like donation from Sir Thomas Elder, to found a professorship for mathematics and another of natural science. The same benefactor gave £10,000 for the establishment of a medical chair in 1883, and £1000 for evening classes; and the Hon. J. H. Angas gave £6000 for the creation of a chair of chemistry, and £4000 for the establishment of scholarships and exhibitions. Though the Adelaide University was incorporated in 1874, the present University buildings were not opened until 1882. The School of Mines and Industries, as it is officially designated, was opened in 1889, and has steadily increased in influence and usefulness since then.

The chapter on the aborigines of South Australia is perhaps the best in the book, and as the author has had more than forty years' experience with the blacks, he writes upon what he is well qualified to describe. Altogether the volume includes much that has not hitherto appeared in print in a collected form, and therefore deserves to rank with the best books on Australia, its people, and its resources.

Measurement Conversion Diagrams. By Robert H. Smith, Professor of Engineering, Mason College, Birmingham. (London: Charles Griffin and Co., Limited, 1895.)

THE scope of this work is described on the title-page as follows:—"Forty-three graphic tables or diagrams for the conversion of measurements of different units, comprising conversions of length, area, volume, weight, stress, density, work; energy in mechanical, thermal, and electrical units; horse-power, and temperature." Only those who are familiar with graphic statics know what can be done by diagrams, but even they will be astonished at the wide range of conversions covered by Prof. Smith's graphic equivalence plates. The diagrams will principally aid the conversion of English and metric measures, and *vice versâ*, but they also represent the relations between different systems of English, and of French, measurement. We have always been attracted by the method of expressing equivalents by means of squared paper, and Prof. Smith's graphic tables have greatly increased our admiration of it.

LETTERS TO THE EDITOR.

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

The Kinetic Theory of Gases.

I DO not feel as if those who heard me ask some questions at the British Association at Oxford, about the kinetic theory of gases, exactly understood my difficulties. They are those of an onlooker, and so they may be of general interest. As several of them have been fairly satisfactorily answered, it may be worth while stating the present position of such an onlooker as myself.

In the first place, consider the difficulty as to reversibility and as to the number of possible ways in which a system could be started on a reverse path so as to obtain a *given* initial state. This is, I think, completely answered in the way Mr. Larmor gives in his letter on p. 152. As well as I can recollect, Mr. Culverwell and I had been mutually satisfied by this kind of explanation previous to the meeting at Oxford, and it was not then referred to.

The question of reversibility lately started, as I understand it, has reference to the introduction of the postulate of chance in the deduction of the theorem about H. Mr. Burbury, in his recent letter, has indicated a proof of this theorem, in which he explicitly postulates chances, and so far justifies the possibility of proof on these lines. I understand that Mr. Culverwell is so far satisfied, and only asks for more, *i.e.* an extension of this form of proof to other cases than the simple one of colliding spheres.

Secondly, as regards the solar system, &c., I am not yet quite clear why a finite number of particles moving about for an indefinitely long time does not satisfy the conditions of the problem as usually stated, just as well as a large number of bodies for a short time. As to the necessity for *collisions* among the parts of a system, I cannot see why the earth, moon, Jupiter, and sun are not to all intents and purposes of the generalised coordinates in collision at present and always; and I desired to know why any other kind of collision is required for the application of the investigation. I think I now see, through conversations with Mr. Culverwell, where the existing investigations may fail to apply to solar systems. I may explain my position as follows. It was always, I knew, postulated that more than two particles should not be in collision at once, and I therefore asked how this could be an essential part of the investigation when applied to the case of air near the earth subject to gravitation. I did not see why the *earth* was not (so far as the generalised coordinates investigation was concerned) a particle in collision with every particle of the air during every one of their collisions with one another, and consequently violating the postulate requiring only *two* particles to be in collision simultaneously. I now understand that when dealing with gravitation and such like forces, these are supposed to be directed to *fixed* centres, and that in the case of a large particle like the earth this is very nearly true, but that it could not be even approximately true if we had three fairly equal particles acting upon one another simultaneously. This may also explain why the equal partition of energy does not hold in the solar system where the bodies do not act upon one another in pairs, but are all always subject to one another's action. This, as I understand, is also the reason why the direct distance law is not an exception to the equal partition of energy theorem. It also may explain how we can have water and steam in equilibrium with one another, notwithstanding the apparent *uniqueness* of the Boltzmann-Maxwell solution. From experience it would seem that when we can extend the investigation to the case of several bodies in simultaneous collision, we shall find that there are *three* solutions corresponding to the solid, liquid, and gaseous states. At the same time, some of the very general investigations that seem to me, as a physicist, as if they were intended to apply to complex molecules in collision with one another, and with a partition of energy amongst the atoms, appear to violate the postulate of collisions in pairs; for I find it hard to conceive of these molecular systems of atoms as other than systems, the various parts of which are held together by mutual actions, and which must consequently be considered to be in simultaneous collision with one another. This may be where the spectrum crux fails. Perhaps somebody would be so very kind as to point out where exactly in these generalised coordinate investigations the postulate of collisions in pairs is used, and so save lazy people like me the trouble of hunting it up.

This raises the third point as to how this difficulty about the spectral lines is to be surmounted. I cannot follow either Mr. Bryan's, or what I understood to be Mr. Larmor's view, that any help can be got by supposing spectral lines to be due to electromagnetic vibrations. The example Mr. Bryan gives of smooth solids of revolution is quite beside the point. In this case there is *no interchange of energy* between rotation round the axis of revolution and the other degrees of freedom. This is quite contrary to what we know to be the case in respect of ethereal and molecular energy. We know that radiations cause bodies to become cooler, and therefore there *is* interchange of energy. This could not be otherwise, as is evident from what we know of the mechanical forces—electric, magnetic, and electromagnetic—that interact between matter and ether. It is rather hard for Mr. Bryan to say that the *onus probandi* lies with physicists to explain exactly *how* transference takes place; surely the fact that transference does take place is sufficient to prove that a complete theory should take in both sets of coordinates—ethereal as well as material—and I should have thought that those formidable arrays of $dp_1\ dp_2\ .\ .\ .\ dp_n$ &c., of coordinates and momenta to the *n*th, with dots between to signify their indefinite number, should include everything of this kind that could possibly be required. Here, however, the postulate of collisions in pairs entirely breaks down, and thus shows a way out of this spectral difficulty. A second way was suggested to me, by whom I forget, at Oxford, namely, that the complicated systems of lines that we see in the spectrum—of iron, for instance—are so connected as to their amplitudes, periods, and phases, as to represent only a single coordinate. Anybody who has tried to expand a simple function in Fourier series will easily understand how a very simple motion might produce a fearfully complicated spectrum.

It seems, then, to me that the questions which need solving in our study of the dynamical foundations of thermodynamics at present are (1) how to explain spectra, and (2) how to deal with several bodies in simultaneous collision?

GEO. FRAS. FITZGERALD.

Trinity College, Dublin, December 19, 1894.

THE difficulties in the way of harmonising the spectroscopic phenomena of heated gases with the conclusions of the Boltzmann-Maxwell theorem of distribution of kinetic energy, appear to me to have been exaggerated.

This theorem asserts, as a purely dynamical proposition, that a very large number of, say, billiard balls, perfect as to sphericity and elasticity, having been set in haphazard motion in a space bounded or interrupted by perfectly elastic rigid surfaces, if the volume of free space be sufficiently large as compared with the total volume of the balls[1] will, if left to itself, tend to a certain state of density and velocity distribution, and having reached this state, will remain in it permanently. The billiard balls may be replaced by rigid bodies of any form, or indeed by material systems capable of any changes of shape or motion of the parts among themselves, and a corresponding proposition still prevails—one general property holding good in all cases, viz. that if u, v, w be the motions of translation of each system as a whole, m its mass, T its total kinetic energy, and n the total number of its degrees of freedom inclusive of the 3 of translation, then in the permanent state

$$\frac{m\overline{u^2}}{2} = \frac{m\overline{v}}{2} = \frac{m\overline{w^2}}{2} = \overline{T}/n,$$

where – denotes mean value throughout the whole medium.

Primâ facie there is a discontinuity in this abstract proposition unfitting it for the basis of physical investigation, *e.g.* the smallest possible want of perfect sphericity in the billard balls would appear to effect as complete a change in the physical properties of the medium as if each ball became an ellipsoid or tetrahedron, the mean $\frac{m\overline{u^2}}{2}$ being changed *per saltum* from $\overline{T}/3$ to $\overline{T}/6$, and the difficulties introduced by the spectroscope are founded upon this discontinuity. Doubtless we cannot make n fractional, but it should be remembered that the dynamical proposition only speaks of an *ultimate* state, and ignores the rate of approach to that state. The continuity of physical properties is maintained by attending to the continuity of change in this rate. This rate I have estimated for a particular case in the new edition of my short treatise on the subject, showing that there *may* be a *sensible* permanence long before the law of equal partition is established.

And, again, there is yet another point to be considered. The passage from the thermal to the optical properties resembles the passage from mere noise to music. Dynamically it is the passage from irregular, haphazard motion, *heat*, to regular periodic motion, *light*; the former must be decomposed into its equivalent harmonic motions, and the most important terms retained, but there would be no necessary relation between the number of these terms (sensible bright lines) and the number of degrees of freedom of the molecule. H. W. WATSON.

[1] So large that the average number of cases in which three or more spheres are at any instant in collision is infinitely smaller than the number of cases in which two only are in collision.

The Horn Expedition to Central Australia.

In your issue of September 27, 1894, occurs a short notice of the work of the Horn scientific expedition to Central Australia. Reference is made therein to the discovery of a "new type of marsupial" by Dr. Stirling. The animal in question was found by Mr. South, a mounted trooper (or rather by his cat, who brought it into the house), at Alice Springs. By him it was presented on our arrival to Dr. Stirling, who had charge of the anthropological work of the expedition, by whom it was kindly handed on to me as officer in charge of the zoological department. The specimen was a male, and being desirous of securing more, I stayed behind the party, and by aid of the blacks procured two more specimens, both of them females. The animal, which lives in holes amongst the rocks and stones, is by no means common, as I had to offer considerable quantities of flour and tobacco to the blacks as a reward for its capture. After a number of them had been out hunting for several days the total result was two more specimens, though as these were females they formed a welcome addition to my zoological collections. As the expression, "a new type of marsupial," gives rise to too great expectations, it may be as well to state that it is merely a new species of the genus Phascologale, distinguished, amongst other points, by its remarkably fat tail and by the nature of the striated pads on the soles of its feet. I was able to make drawings of the animal alive, and on showing these to the blacks at Charlotte Waters, some 250 miles to the south of Alice Springs, they were at once greeted with cries of "Amperta," the native name for the animal which they took it to represent. Through the kindness of Mr. Byrne, the head of Charlotte Waters Telegraph Station, I have since been provided with specimens of the "Amperta," which on examination turns out to be the rare form—only as yet, I believe, known from a single specimen—described by Krefft under the name of *Chætocercus cristicanda*, and subsequently placed by Mr. Oldfield Thomas in the genus Phascologale.

These two species, and a new one of the genus Sminthopsis, which we secured amongst the sand-hills near Lake Amadeus, are the most important finds amongst the marsupials which, owing to the country traversed by us being in the main of a desert description, were by no means plentiful. In this region animals can only be secured in numbers after rain, an experience which, during three and a half months' wandering, did not fall to our lot. However, as one result of Mr. Horn's generous action in equipping the expedition, I hope to be able to give a fairly good general account of the fauna of the central desert region of Australia. Towards the close of the notice referred to, it is said that there is some doubt as to the manner of publication of our results. Mr. Horn's intention is, I believe, to issue a separate volume, the various parts of which will deal with the branches of science represented by the members of the scientific staff as follows:—

Prof. R. Tate (chairman of the scientific staff), geology and botany; Dr. E. C. Stirling, anthropology; Mr. C. Winnecke (leader of the expedition), surveying and meteorology; Prof. Baldwin Spencer, zoology; Mr. J. A. Watt, geology and mineralogy.

The volume will contain an accurate map, compiled by Mr. Winnecke, of the central district drained by the Finke river, and will, by means of the reproduction of photographs, illustrate some of the more important physiographic features of Central Australia. BALDWIN SPENCER.

Palæontology at the Royal School of Mines.

IN reading the excellent review of the biography of Sir R. Owen, which appeared in last week's NATURE, I observe an error which, though small, requires correction. It is stated that when Owen surrendered his appointments at the College of Surgeons he was "enabled to accept the lectureship on Palæontology at the Royal School of Mines, in 1857." The records of that Institution will show that Owen never held a lectureship there, nor was he in any way connected with the School.

The large theatre of the Museum of Practical Geology was frequently employed for other purposes than those of the School, by permission of the Director-General; and it was in virtue of such permission from Sir Roderick Murchison that Prof. Owen used the theatre for the delivery of his lectures on Palæontology to the public, in 1857 and subsequently. T. H. HUXLEY.

Eastbourne, December 27, 1894.

Eocene Fossils at Murren.

I HAVE read with surprise the extracts from the letter which you have received from Dr. Fellenberg on this subject. Of course it has long been known that there are Eocene strata at Murren. But below them lie calcareous rocks coloured on the Swiss map as Malm. These are so described on p. 211 of the "Livret Guide," published by the International Geological Congress which met this autumn at Zürich. During the subsequent excursion, under the able guidance of Prof. Renevier, Prof. Golliez, and M. Lugeon, we were taken to Murren and shown these rocks, and Prof. Golliez gave us the reasons which had led some geologists to regard them as Trias rather than Malm. It was in these calcareous beds that the layer containing nummulites was met with. The train was just starting, and we had to leave, but the find excited so much interest that M. Lugeon returned to Murren the next day, with some of the party, and verified the exact locality. JOHN LUBBOCK.

High Elms, Farnborough.

The Use of the Globe in Crystallography.

IN your issue of December 20, Mr. Buchanan revives a graphic method of crystallographic calculation which seems to have been used in the early part of the century (*vide* "Zur physischen Krystallonomie, &c.," Grassmann, 1829, p. 37), and claims that by use of the globe and *métrosphère* "every problem in the geometry of crystals can be solved with ease and accuracy."

Crystallographic angular measurements are said to be accurate if subject to a probable error of less than two or three minutes, and descriptions of inorganic substances are nowadays habitually published in which the probable errors are of this order. Although no details of Mr. Buchanan's method are given, it seems inconceivable that any graphic process of crystallographic calculation, involving triangulation on a sphere, could be accurately performed without the use of numberless tedious precautions and large and cumbrous apparatus.

It is usually more easy to grasp a good plane diagram of any solid figure than to understand and follow up explanations on the solid figure itself; the use of the latter is liable to lead to inaccuracy of expression and confusion of thought. Thus, when the sphere is used, the real meaning of the points which Mr. Buchanan describes as cataloguing the edges occurring on crystals, is not at once seen; using the plane projection, it is immediately apparent that these points are characteristic, not merely of the edges, but in a much wider sense, of the zones: they are merely the poles of the zone circles.

Further, the positions of the points representing the corners are dependent on the *sizes* of the faces concerned, which, as we teachers of crystallography are at infinite pains to impress on our students, have no crystallographic signification; these points, then, are not *characteristic* of the corners.

Similarly, the rather complicated piece of reasoning respecting "reciprocal inversion forms" simply yields the well-known result that in the cubic system, the octahedron truncates the corners of the cube. WILLIAM J. POPE.

Central Technical College, South Kensington.

"The Zoological Record."

IT has long been a matter of regret that the *Zoological Record* is not sold in separate parts; a specialist requiring any one part being required to pay for all the others, though they may be of no more use to him than so much waste-paper.

In order to remedy, to some extent, this unfortunate state of things, I am proposing (if sufficient support is forthcoming) to purchase the *Zoological Record* as published, and to issue the separated parts to subscribers. With this view, I would ask all those desiring any part (of the volume just issued, or of past volumes) of the *Zoological Record*, to communicate with me *as soon as possible*, stating *which part* they would be willing to subscribe for.

Although it is, of course, not intended to make a profit out of this scheme, it will nevertheless be necessary to charge slightly more for the separated parts than their proportionate value, as some parts are almost certain to remain unsold.

I am confident that this scheme will not in any way injure the Zoological Society; in fact, although they maintain the contrary, I am sure it would be to their advantage to issue the parts separately, if necessary at a slightly higher rate. At present subscribers are, I believe, mostly libraries and societies, requiring the whole volume. These would, of course, continue to subscribe for all parts, even if they were obtainable separately; while, on the other hand, all specialists who do not subscribe under the present arrangements would be practically certain to purchase those parts dealing with their own subjects, if obtainable at a moderate cost. If the proposed scheme meets with support, it will go a long way towards proving the justice of the foregoing contention, and in that case it will probably be possible to induce the Zoological Society to grant the concession for which many zoologists have for long been agitating.

Royal College of Science, London. S. PACE.

Gravitation.

IN answer to Dr. Lodge's letter, I may state that Newton in his "Opticks" (Query 21) asks if an increase of density of the ether outwards from bodies will not account for gravitation, every body endeavouring to go from the denser parts of the medium towards the rarer; and if such increase of density may not, even at great distances, be effective, provided the elastic force of the medium be sufficiently great.

I do not think a tensile ether is contemplated in this theory of gravitation. Prof. Worthington's effect manifests itself only in a tensile liquid, and this constitutes its suggestiveness in connection with the hypothesis of a tensile ether. I have no such definite ideas to advance as are put forward in the "Opticks." J. JOLY.

Trinity College, Dublin.

The Feigning of Death.

THE curious condition of apparent death, assumed by the English grass snake, which Mr. G. E. Hadow describes (NATURE, December 6, p. 127), is one that I have frequently observed, but have always been puzzled to account for I hardly think that it has anything to do with simulation, or that it is voluntary, since I have seen snakes so affected when quite undisturbed in their cases. I have also observed precisely the same state in the common Italian snake. In my experience the condition only occurs in fairly hot weather, and when the snake has not fed for some time. This seems to point to a species of fainting fit, and I imagine that it is immediately induced by a disturbance of the cerebral circulation. R. HARRY VINCENT.

Leytonstone, December 30, 1894.

Peculiarities of Psychical Research.

IN reply to Prof. Pearson: (1) His remark about "scientific acumen" was not made *à propos* of M. Richet's experiments, but of those of the S.P.R.; and hardly any stress is laid on M. Richet's results, either by Mr. Gurney or Mr. Podmore. Mr. Gurney, on the contrary, expressly says: "Clearly no definite conclusion could be based on such figures." But if Prof. Pearson has made experiments which are equally striking in the opposite sense, I wish he would publish them, or communicate them to the S.P.R. (2) There was nothing in my letter to indicate that I under-estimated the importance of "abnormal distributions"; but I asked Prof. Pearson to say whether he had any reason to suppose such distributions might have occurred in the case in dispute. This he has failed to do—he has evaded the point. (3) Prof. Pearson descends to vague generalities except in regard to Dr. Oliver Lodge, who may be left to defend himself.

With the last paragraph of the letter, however, I heartily concur. There is nothing the S.P.R. would welcome more than intelligent and independent criticism. Only the critic would have to study the evidence first, and the Professor apparently has the "scientific acumen" to see that by doing so he would cut his own throat; for he would, *ipso facto*, become a psychical researcher! EDWARD T. DIXON.

Cambridge, December 29, 1894.

ON THE AGE OF THE EARTH.

IT has been thought advisable to publish the following documents. On October 12 I put my views before Prof. Fitzgerald and Dr. Larmor. The first paper is a copy of my letter to Dr. Larmor. It has now been edited a little, as originally it was rather hurriedly written. Some long mathematical notes, added on November 1, to prove the legitimacy of my approximate method of calculation, are now omitted, as Mr. Heaviside has given exact solutions, and has found that there is practically no difference between mine and the exact numerical answers. That Mr. Heaviside should have been able, in his letters to me during eleven days, to work out so many problems, all seemingly beyond the highest mathematical analysis, is surely a triumph for his new methods of working. Only for Prof. Fitzgerald's encouragement and sympathy, it is very probable that this document would never have been published.

I have sometimes been asked by friends interested in geology to criticise Lord Kelvin's calculation of the probable age of the earth. I have usually said that it is hopeless to expect that Lord Kelvin should have made an error in calculation. Besides, in every class in mathematical physics in the whole world since 1862 the problem has been put before students, and, as the subject is of enormous interest, if there had been any error it certainly would have been discovered before now.

I dislike very much to consider any quantitative problem set by a geologist. In nearly every case the conditions given are much too vague for the matter to be in any sense satisfactory, and a geologist does not seem to mind a few millions of years in matters relating to time. Therefore I never till about three weeks ago seriously considered the problem of the cooling of the earth except as a mere mathematical problem, as to which definite conditions were given. But the best authorities in geology and palæontology are satisfied with evidences in their sciences of a much greater age than the one hundred million years stated by Lord Kelvin; and if they are right, there must be something wrong in Lord Kelvin's conditions. On the other hand, his calculation is just now being used to discredit the direct evidence of geologists and biologists, and it is on this account that I have considered it my duty to question Lord Kelvin's conditions.

The original object of Lord Kelvin's investigation is usually forgotten. He sought to prove, and proved, that the earth is losing energy at a calculable rate. He said that the loss might be the loss of potential or chemical energy instead of sensible heat, or as well as heat, although he thought that a large proportion of potential or chemical energy was improbable; and it is only on the assumption that the earth is a cooling body losing energy originally only of the sensible-heat form, that his calculation of the age of the earth is based. Not only so, but also his earth is a homogeneous mass of rock such as we have on the surface, with the same conductivity and other heat properties. He starts with the knowledge that there is an average increase of temperature downwards in the earth of one Fahrenheit degree for every 50 feet. Assuming that the earth, a solid, was once at the uniform temperature of 7000° F., that its surface was suddenly brought to and kept at the temperature 0, and taking k/c (k being conductivity and c capacity for heat of unit volume, in year foot units) as 400, he finds that 10^8 years have sufficed to cause the temperature-gradient at the surface to be what it is now. He stated that the conditions were sufficiently represented by an infinite uniform mass of matter at 7000° F. with an infinite plane face kept at 0.

At first I preferred to consider a *globe* of 4000 miles radius of constant surface-emissivity to be cooling as if in an enclosure, kept at constant temperature. I made the emissivity infinite, and obtained Lord Kelvin's answer for temperature-gradient near the surface. When the emissivity is taken of a finite value, the time taken to produce the present temperature-gradient is less than Lord Kelvin's answer.

It is interesting to notice that if we take our enclosure to be at a zero of temperature which we can choose as we please, we have a method of using Fourier's expression in certain cases in which the emissivity is not constant. By no method of working does it seem probable that we shall greatly alter Lord Kelvin's answer.

Modification of Lord Kelvin's Conditions.

But, when we depart from homogeneity, when we assume that the interior of the earth may be of better conducting material than the surface rock in which the temperature-gradient is alone measured, we find a very different state of things from that considered by Lord Kelvin. The cooling from a constant temperature of an infinite mass bounded by a cold plane face, a slice of which near the surface is of material different from the rest of the infinite block, is a problem difficult to attack mathematically. But if the slice is thin, or if much time has elapsed, the following artifice leads to a solution.

Imagine an infinite homogeneous block, originally at temperature V_1, whose surface is kept at 0. If x_1 is sufficiently small and t great, we may neglect the exponential term, and (v being temperature and t time, and x the distance from the cold face)

$$\frac{dv}{dx} \text{ at } x_1 = V_1 \div \sqrt{\pi\kappa_1 t}; \; v_1 \text{ at } x_1 = V_1 x_1 \div \sqrt{\pi\kappa_1 t}.$$

Rate of flow of heat across unit area at $x_1 = k_1 V_1 \div \sqrt{\pi\kappa_1 t}$. I take k as conductivity, and κ as conductivity divided by capacity for heat of unit volume.

Now take another such homogeneous infinite block of different material, and use the letters with affix 2 instead of 1. Let the time be the same in both. Let the surface slice from x_1 to 0 in the first, and from x_2 to 0 in the second be considered. We can, by taking proper values of V_1 and V_2 and x_1 and x_2, make the rates of flow of heat equal and the temperatures equal at x_1 and x_2:

$$k_1 V_1 / \sqrt{\kappa_1} = k_2 V_2 / \sqrt{\kappa_2} \text{ and } V_1 x_1 / \sqrt{\kappa_1} = V_2 x_2 / \sqrt{\kappa_2}$$

Hence $k_1 \div x_1 = k_2 \div x_2$. Thus if $nk_2 = k_1$, we take $nx_2 = x_1$.

Now we can take the slice x_2 to 0 from the second block and let it take the place of the slice x_1 to 0 on the first block. The artificial block so produced will go on cooling, its outside face being kept at 0_1. But we shall have at the point of junction a sudden multiplication of dv/dx. In fact, dv/dx will be what it used to be towards the interior, but will be n times as great towards the surface. It is of no consequence what the value of κ_2 is, if times are great and slices thin, the only important thing is that k_1 shall be n times k_2. The application of the result is obvious:—

Let the interior of the earth be a uniform sphere, uniformly heated to 7000° F. Take its κ as m times what Lord Kelvin took it, then an increase of temperature downwards from the surface of 1 F. degree for every 50 n feet would be produced in $10^8 n^2/m$ years. Take its k as n times what Lord Kelvin takes. Now if we imagine a skin removed and replaced by one of $1/n$th of the thickness and $1/n$th of the conductivity, that is, take it of Lord Kelvin's conductivity of rock, the surface slope will be 1 in 50, what it is now, and Lord Kelvin's time will be increased in the proportion n^2/m.

Considering the great differences in conductivity of such bodies as we know, it is quite conceivable in our knowledge and ignorance of the interior of the earth that n^2/m may be considerable even now, and probably was very considerable in past times. Roughly we may say that Lord Kelvin's age of the earth, 10^8 years, ought to be multiplied by two to six times the ratio of the internal conductivity to the conductivity of the skin.

I am not in a position to criticise the arguments from tide phenomena which Lord Kelvin or Mr. Darwin would now put forward on the subject of much internal fluidity of the earth. The argument from precession has been given up. Of course much internal fluidity would practically mean infinite conductivity for our purpose. But there is no doubt of a certain amount of fluidity inside even now, and taking it that the inside of the earth is a honeycomb mass of great rigidity, partly solid and partly fluid, we have reason to believe in very much greater quasi-conductivity inside than of true conductivity in the surface rocks, and if there is even only ten times the conductivity inside, it would practically mean that Lord Kelvin's age of the earth must be multiplied by 56.[1]

If we imagine the earth perfectly conducting inside with a thin covering, say 60 miles thick, of rock, such as we know it on the surface, we must leave Lord Kelvin's infinite mass and study the sphere. Indeed, if we take it that we have now an infinite mass at 7000° F. of infinite conductivity, cooling through rock of from 60 to 70 miles thick with a constant gradient of 1° for every 50 feet, we can imagine that this state of things has existed for an infinite time, and any original distribution of temperature in the rock would settle down to such a state.

Taking, then, an internal sphere of infinite conductivity[2] (and working in C.G.S. Centigrade units), its specific heat 0·2, and the conductivity of the rock 0·002, I find that if at the beginning of time there was an increase of 1° Centigrade in 45 feet, and now there is an increase of 1 Centigrade degree in 90 feet, the lapse of time is 28,930 million years, or 290 times Lord Kelvin's age, and the core has cooled from 8000 to 4000 degrees. Or, again, in the last 10^8 years the gradient has only diminished by 1/400th of its present value, and the core has only changed from 4010 to 4000 degrees.

I do not know that this speculation is worth much, except to illustrate in another way the augmented answer when we have higher conductivity inside. It would evidently lengthen the time if I assumed that the temperature-gradient was not uniform in the shell, but the exact mathematical calculation is so troublesome that I have not attempted it.[3]

JOHN PERRY.

31 Brunswick Square, London, W.C., October 14.

October 22, 1894.

The reasoning in my paper was applied either to infinite blocks of cooling material or to a sphere with an internal core which has infinite conductivity. At the time of writing I did not see my way to the consideration of a sphere with a core of finite conductivity and a shell of rock as a covering, but the case is really easy to work when the shell is only a few miles in thickness, as will be seen below.

PROBLEM.—A sphere of radius $R = 6·38 \times 10^8$ centim. of conductivity $k = 0·47$ (or 79 times that of surface-rock) and $k/c = 0·16464$ (or 14 times that of surface-rock), has upon it a shell of rock of thickness 4×10^5 centim. (about $2\frac{1}{2}$ miles). The whole mass was once at a temperature $V = 4000°$ C., and suddenly the outside of the shell was put to 0° C. and kept at that. Find the time of cooling until the temperature-gradient in the shell has become 1 Centigrade degree in 2743 centim. (or 1° F. in 50 feet).

Now, if we are allowed to assume that the shell very rapidly acquired and retained a uniform temperature-gradient throughout its thickness, and it is easy to show that this assumption is allowable (or if not, then the discrepance is in favour of a greater age for the earth), the problem is exactly the same as this:—The above-mentioned sphere has no shell of rock round it, but emits heat to an enclosure of 0° C., the constant emissivity of its surface[4] being $E = 1·475 \times 10^{-8}$; find the time in which the surface-temperature v' becomes 146° C.

This problem is solved by Fourier, who gives for the temperature at the distance r from the centre

$$v = \frac{2VER}{k} \Sigma \frac{\sin er/R}{er/R} \frac{\epsilon^{-ke^2t/cR^2}}{e \operatorname{cosec} e - \cos e},$$

where in the successive terms the values of e to be taken are the successive roots of the equation

$$e/\tan e = 1 - ER/k.$$

In the present case $ER/k = 20$, and e_1, e_2, e_3, &c., are nearly π, 2π, 3π, &c. I have, however, taken the actual values of e_1 and e_2—two exponential terms, only, being of importance, and I find that, if $t = 96 \times 10^8$ years,

$$v' = \underset{\text{1st term}}{142·7} + \underset{\text{2nd term}}{5·65} = 148·4;$$

so that the age of cooling to the present temperature-gradient is more than 96×10^8 years.

[1] Observe that, even if we assume that there is the same conductivity inside and outside, inasmuch as the density is greater, c is greater, say 3 times as great, and even without the assistance of increased conductivity inside, we have 3 times Lord Kelvin's age. I admit that all such speculation as to the value of c is too vague to be of much importance.

[2] If θ_0 and θ were the internal temperatures at the times t_0 and t, if b is the thickness of the crust and R the radius of the internal sphere, if s is its specific heat and ρ its density and k its conductivity,

$$t - t_0 = \frac{Rbs\rho}{3k} \log \frac{\theta_0}{\theta}.$$

[3] If ·006 be taken as the conductivity of rock, the times are only a third of what I have given.

In connection with this matter I notice that in Lord Kelvin's very short paper, entitled "The 'Doctrine of Uniformity' in Geology briefly Refuted," read before the Royal Society of Edinburgh in 1865, he finds:—"But the heat which we know, by observation, to be now conducted out of the Earth yearly is so great that if this action had been going on with any approach to uniformity for 20,000 million years, the amount of heat lost out of the Earth would have been about as much as would heat, by 100° Cent., a quantity of ordinary surface-rock 100 times the Earth's *bulk*." (The italics are mine.) In his address on "Geological Dynamics," Part II., published in 1869 (p. 126, vol. ii. "Popular Lectures and Addresses"), he calculates the total amount of energy which *may* once have been possessed by the Earth mass, partly gravitational and partly chemical, as 'being about 700 times as much heat as would raise the temperature of an equal *mass* of surface-rock from 0° to 100° Cent.' (The italics are mine.) I do not think that these two statements have ever before been put in juxtaposition. Comparing them, we may say that, according to Lord Kelvin's own figures, if the present action had been going on with any approach to uniformity for 10^8 years the amount of heat lost by the Earth would have been the 1/7800th part of the whole energy which the whole Earth may once have possessed, or 1/2230th part of what Lord Kelvin gives as an estimate, an over-estimate he calls it (but he says that it is not possible to make one much less vague), of the whole amount of heat at present in the Earth. I mention this because some mathematical physicists believe that Lord Kelvin based his age of the Earth upon a calculation of this total loss. He only used it in opposition to the extreme doctrine of uniformity for the past 20,000 million years (a doctrine which is not now believed in by any geologist), but it lends no support to his calculated age of the Earth).

All through this paper I give 10^8 years as Lord Kelvin's age of the Earth. His own words (*Trans. R.S. Edin.*, 1862 (j) are:—"We must, therefore, allow very wide limits in such an estimate as I have attempted to make; but I think we may with much probability say that the consolidation cannot have taken place less than 20,000,000 years ago, or we should have more underground heat than we actually have [he means a more rapid increase of temperature downwards], nor more than 400,000,000 years ago, or we should not have so much as the least observed underground increment of temperature." Taking the average diffusivity for heat of the Edinburgh experiments, he finds (r) that the present temperature-gradient of 1 Fahr. degree for every 50 feet gives a life of 10^8 years.

[4] Because if v' is the surface-temperature of the sphere and b the thickness of the shell of rock, v'/b was the surface-gradient in the shell and v'/b multiplied by conductivity of rock is equal to Ev'.

If we take k as 195 times that of the surface-rock, and k/c as 35 times that of the surface-rock, and if the shell has a depth of $3{\cdot}272 \times 10^6$ centimetres (about 20 miles), the time of cooling until the temperature-gradient is 1 Cent. degree in 2743 centim. is *more* than 127×10^8 years."[1]

I kept no copy of the letter which I sent to Prof. Tait with the foregoing document. In it I explained my difficulty in getting Lord Kelvin to re-consider the internal heat question, and I asked for his advice.

Extract from Letter of Prof. Tait, November 22, 1894.

. . . my entire failure to catch the *object* of your paper. For I seem to gather that you don't object to Lord Kelvin's mathematics. Why, then, drag in mathematics at all, since it is absolutely obvious that the better conductor the interior in comparison with the skin, the longer ago must it have been when the whole was at 7000° F. : the state of the skin being as at present?

I don't suppose Lord Kelvin would care to be troubled with a demonstration of *that*.

As to the validity, or more properly the *plausibility* of his or your assumptions, I don't suppose anyone will ever be in a position to judge. He took the simple and apparently possible case of uniform conductivity all through—having no data whatever. What if he had assumed, as he was quite entitled to do, that the conductivity diminishes *very fast* with rise of temperature? But I need not say any more, as I seem to have entirely missed your point.

———

Letter to Prof. Tait, November 26, 1894.

DEAR PROF. TAIT,—I should have been on the whole better satisfied if you had opposed my conclusions. You say I am right, and you ask my object. Surely Lord Kelvin's case is lost, as soon as one shows that there are *possible* conditions as to the internal state of the earth which will give many times the age which is your and his limit. . . . What troubles me is that I cannot see one bit that you have reason on your side, and yet I have been so accustomed to look up to you and Lord Kelvin, that I think I must be more or less of an idiot to doubt when you and he were so "cocksure." The argument from the sun's heat seems to me quite weak. Even a geologist without mathematics can see that the time given by Lord Kelvin will be increased if we assume that in past times the sun radiated energy at a smaller rate than at present, much of its mass being possibly cold and in the meteor form, and the rate may have greatly varied from time to time. This is not only possible but probable, and it is for you and Lord Kelvin to prove a negative.

Then the Tidal Retardation argument! Even if your rate of retardation is correct, the real basis of your calculation is your assumption that a solid earth cannot alter its shape (diminishing its equatorial radius by a few miles) even in 1000 million years, under the action of forces constantly tending to alter its shape, and yet we see the gradual closing up of passages in a mine, and we know that wrinkling and faults and other changes of shape are always going on in the solid earth under the action of long-continued forces. I know that solid rock is not like cobbler's wax, but 10^9 years is a very long time, and the forces are great!

I had thought these two arguments to be mere supporters of the internal heat one which I took to be the only important one, like a diamond whose pure sparkle was brought into relief by two rubies.

If I were alone in my opinion, I should still have the courage, I think, to write as I do; but as I have already told you, I did not venture to write and speak to Lord Kelvin, or write to you until I found that so many of my friends agreed with me—Fitzgerald, O. Reynolds, Larmor, Henrici, Lodge, Heaviside, and many others. Fitzgerald is the only man to whom I have mentioned my notion about the sun's heat, but he quite agrees with me. I have not put before him my notion about the Tidal Retardation argument. . . .

———

November 27.

DEAR PROF. PERRY.—I should like to have your answers to *two* questions :—

(1) What grounds have you for supposing the inner materials of the earth to be better conductors than the skin?

(2) Do you fancy that any of the *advanced* geologists would thank you for 10^{10} years instead of 10^8? Their least demand is for 10^{12}:—for *part* of the mere secondary period!

Yours truly,
P. G. TAIT.

———

November 29, 1894.

DEAR PROF. TAIT,—It is for Lord Kelvin to prove that there is *not* greater conductivity inside. Nevertheless I will state my grounds :—

1 (*a*). In page 6 of the paper sent you I say "I am not in a position to criticise the argument from tide phenomena which Lord Kelvin or Mr. Darwin would now put forward on the subject of much internal fluidity of the earth. The argument from precession has been given up. Of course, much internal fluidity would practically mean infinite conductivity for our purpose. But there is no doubt of a certain amount of fluidity inside, even now, and taking it that the inside of the earth is a honeycomb mass of great rigidity, partly solid and partly fluid, we have reason to believe in very much greater quasi-conductivity inside than of true conductivity in the surface rocks."

1 (β). Even if we assume perfect solidity, and even neglecting our knowledge of much iron—surely there can be no doubt of the conductivity of rock *increasing* with the temperature. From the analogies with electric conduction, one would say, without any experimenting, that as a metal diminishes in conductivity with increase of temperature, so a salt, a mixture of salts, a rock, may be expected to increase in conductivity with increase of temperature. I presume that Everett's book is recognised now as giving the most exact information on these subjects. He nowhere suggests that rock diminishes in conductivity with temperature. Every case he gives shows an increase. I have made out the following table from the only quotations which Everett gives from Dr. Robert Weber; only five cases, but probably representative.

Percentage increase for a rise of 100° *Centigrade.*

	In conductivity.	In specific heat.
Micaceous gneiss ...	48·0	23·6
Mica schist	136·4	24·4
Eurite	1856	35·7
Gneiss	21·4	61·5
Micaceous schist ...	94·5	35·4
Average ...	431	36·1
Average, leaving out Eurite	75	36

Even if the conductivity and specific heat did not alter, inasmuch as the internal density is greater, the volumetric capacity is greater; and if it is three times as great, we have three times Lord Kelvin's age. In fact, the rule given at page 4 of my paper is the same as this :—If the conductivity inside is

[1] The general expression for any case is this :—A sphere of radius R of conductivity nk and capacity per unit volume mc/n surrounded by a shell of thickness b, conductivity k, and capacity for heat per unit volume c; take $E = k/b$: when is v', the temperature at the surface, equal to $b/2743$?

$$\frac{e}{\tan e} = 1 - R/bn.$$

Then

$$\frac{b}{2743} = \frac{2VR}{bn} \Sigma \frac{\sin e}{e} \quad \frac{\sin e}{e - \frac{1}{2}\sin 2e} \epsilon^{-ke^2t/cR^2}$$

enables t to be calculated. It would no doubt be possible, but it would hardly be worth while, to find the values of n and b which would give a maximum value for t. In one of the above cases I took e nearly π, and in the other $\pi/2$.

I am quite unable to attack the problem of the cooling of a sphere from an arbitrary initial condition, in which the diffusivity for heat is an arbitrary function of r.

There is some distribution of k/c which would give a greater age to the Earth than any other, but, again, it would hardly be worth whilo to spend much time on the problem. My purpose has not been to fix a higher limit to the age of the Earth; it has only been to show that such a higher limit must be greater than some hundred of times one hundred million years.

Some of my friends have blamed me severely for not publishing the above document sooner. I was Lord Kelvin's pupil, and am still his affectionate pupil. My B.A. lecture on Spinning Tops was stolen from him, as I duly acknowledged when it was published. He has been uniformly kind to me, and there have been times when he must have found this difficult. One thing has not yet happened; I have not yet received the thirty pieces of silver.

n times the conductivity outside; if the specific heat inside is *s* times the specific heat outside; if the density inside is *d* times the density outside; then Kelvin's age of the earth is increased *n s d* times. . . It is not likely that Dr. Weber's rate of increase would be constant to such a temperature as 4000° C.; but the electric analogue allows us to imagine a greater and greater rate of increase at higher temperatures; therefore it is in Lord Kelvin's interest to take Weber's rate. Now at 4000° C. the conductivity would be [leaving out eurite, which seems abnormal and too much in my favour], thirty times as great as it is at the surface; the specific heat would be $14\frac{1}{2}$ times as great, and taking the density as three times, we have, even for a *perfectly solid* earth an age 1300 times the age given by Lord Kelvin.

2. In answer to your second question, Lord Kelvin completely destroyed the uniformitarian geologists, and not one now exists. It was an excellent thing to do. They are as extinct as the dodo or the great auk.

I have met many advanced geologists, and not one of them demands more than 1,000,000,000 years. Probably Sir Archibald Geikie is the most representative of the geologists who have studied this question, and he never (in recent years) seems to have desired even as much as 1,000,000,000 years. (See his address as President of the British Association.) The biologists have no independent scale of time; they go by geological time. According to Huxley, less than 1,000,000,000 years is enough as the age of life on the earth.

But surely the real question now is not so much what the geologists care about, as—Had Lord Kelvin a right to fix 10^8 years, or even 4×10^8 years, as the greatest possible age of the earth?

Yours truly,

JOHN PERRY.

December 6, 1894.

DEAR PROF. TAIT,—Prof. Fitzgerald has pointed out to me that the five rocks given by Everett are not to be found in his 1891 edition. I quoted from his 1886 edition. I therefore wrote to Everett, asking why he had left them out—was there a mistake? He writes to say: "I copied Weber's data from a copy of his paper which was, and may be still, in my possession, having been sent me through the post, probably by the author, or possibly by Dr. Stapff, the geologist of the St. Gothard Tunnel, with whom I had much correspondence in underground temperature. You seem to assume, in writing to Tait, that I picked out samples of Weber's results; but my recollection is that I gave everything without reservation.

"I did not reproduce his results in the 1891 edition, and I cannot remember all my reasons for dropping them. On comparing them with other people's, which I give, they appear to be much too small. There is such a mass of conduction results in my book, that I was on the look-out for something that might be omitted.

"I have just referred to the foreign translations of my book. The German edition, published in 1888, gives only a page of conductivities of solids, and includes among them one of R. Weber's, namely Glimmerschiefer $\cdot 000733 + \cdot 000010\, t$. The Russian edition, brought out by editors who took tremendous pains in verifying and correcting references, gives my list of Weber's results exactly as it stands in my book, the sign of the temperature coefficient being positive in every case. I do not know of any direct evidence as to the variation of rock conductivity with temperature except R. Weber's, but there is something approaching to direct evidence in the comparison of George Forbes' results with Herschel and Dunn's (see my 1891 edition, pp. 126, 129). Forbes found at − 10° C. the conductivity of white marble to be ·00115, black marble ·00177. Dunn and Herschel found at the temperature of hot water, marbles, &c., ·0047 to ·0056 (see Forbes' remark, quoted at p. 129).

"You have built a very lofty edifice on the basis of Weber's results, and extrapolation is proverbially a risky process, but I consider you have established a strong presumption in favour of the increase of rock conductivity with temperature."

I did not know, when writing to you on November 26, that the Rev. M. H. Close, M.A., had (R. Dublin Soc., Feb. 1878) put forward in great detail the reasons which I gave you shortly, against the tidal retardation argument. I thought they were my own. I notice that this gentleman assumes that increased conductivity inside would help Lord Kelvin, and indeed I cannot help thinking that, without mathematics, almost anybody would be of the same opinion—in spite of what you say in your first letter. I know that Lord Kelvin himself did not seem to think me right when—after I had sent him the documents—I talked to him at Cambridge.

I remain, yours truly,

JOHN PERRY.

Copy of a Letter from Lord Kelvin.

The University, Glasgow, December 13, 1894.

DEAR PERRY,—Many thanks for sending me the printed copy of your letter to Larmor and the other papers, which I found waiting my arrival here on Saturday evening. I have been much interested in them and in the whole question that you raise, as to the effect of greater conductivity and greater thermal capacity in the interior. Your $n^2 \div m$ theorem is clearly right, and not limited to the case of the upper stratum being infinitely thin. Twenty or thirty kilometres may be as good as infinitely thin for our purposes. But your solution on the supposition of an upper stratum of *constant* thickness, having smaller conductivity and smaller thermal capacity than the strata below it, is very far from being applicable to the true case in which the qualities depend on the temperature. This is a subject for mathematical investigation which is exceedingly interesting in itself, quite irrespectively of its application to the natural problem of underground heat.

For the natural problem, we must try and find how far Robert Weber's results can be accepted as trustworthy, and I have written to Everett to ask him if he can send me the separate copy of Weber's paper, which it seems was sent to him some time before 1886; but in any case it will be worth while to make farther experiments on the subject, and I see quite a simple way, which I think I must try, to find what deviation from uniformity of conductivity there is in slate, or granite, or marble between ordinary temperatures and a red heat.

For all we know at present, however, I feel that we cannot assume as in any way probable the enormous differences of conductivity and thermal capacity at different depths which you take for your calculations. If you look at Section 11 of "Secular Cooling" ("Math. and Phys. Papers," vol. iii. p. 300), you will see that I refer to the question of thermal conductivities and specific heats at high temperatures. I thought my range from 20 millions to 400 millions was probably wide enough, but it is quite possible that I should have put the superior limit a good deal higher, perhaps 4000 instead of 400.

The subject is intensely interesting; in fact, I would rather know the date of the *Consistentior Status* than of the Norman Conquest; but it can bring no comfort in respect to demand for time in Palæontological Geology. Helmholtz, Newcomb, and another, are inexorable in refusing sunlight for more than a score or a very few scores of million years of past time (see "Popular Lectures and Addresses," vol. i. p. 397).

So far as underground heat alone is concerned you are quite right that my estimate was 100 millions, and please remark ("P. L. and A.," vol. ii. p. 87) that that is all Geikie wants; but I should be exceedingly frightened to meet him now with only 20 million in my mouth.

And, lastly, don't despise secular diminution of the earth's moment of momentum. The thing is too obvious to every one who understands dynamics.

Yours always truly,

KELVIN.

JUPITER.

JUPITER being now near opposition, and having an apparent diameter of 47″·7, is displayed as a very brilliant object in the heavens, and his northerly declination of 23 degrees enables him to remain above the horizon for a period of $16\frac{1}{2}$ hours.

During the few ensuing months, the observation of his belts and spots will enlist a large amount of attention, for there is probably no other planetary object which exhibits a more diversified and variable aspect. One feature of the present observations will be important as enabling comparisons to be made as to the rates of motion of the various white and dark spots in this and preceding oppositions. No doubt many of the surface markings now existing are identical with those observed some years ago. From 1878 to 1882 the prominent apparition of the red spot incited observers to fully investigate the phenomena of the different formations, and they were found to be very discordant in their rates of velocity. The red spot and equatorial white spots were evidently subject to a marked retardation, causing their rotation periods to increase with the time.

As to the red spot, the slackening rate of motion it exhibited in the earlier years of its presentation, appears not to have been maintained since 1886, for Mr. Marth's adopted period of 9h. 55m. 40·63s. (equivalent to a daily rate of 870°·27) has correctly represented its mean motion during the last eight years. There have been, it is true, some marked deviations from the mean rate, for in the years from 1886 to 1890 the motion became accelerated and corresponded to a rotation period of about 9h. 55m. 40·2s., but in the three following years it slackened again, and since 1891 the period has been about 9h. 55m. 41·5s.

At the present epoch the spot is extremely feeble in its visible outlines, but on a really good night its elliptical form can be distinctly traced, and it does not appear to have materially changed either in its shape or dimensions since 1879. Its following end is decidedly the plainest, and its southern borders have lately been conjoined with a grey belt in about latitude 30° south. The commingling of the spot and belt has been noticed here on previous occasions, and it is certain that on its southern side the spot exerts very little of the repellent influence so often ascribed to it. On the contrary, the belts on the equatorial side of the spot always run clear, and abruptly bend north to allow of a clear white interval between the spot and belt. The S. border of the spot and S. belt were apparently in touch early in the past autumn, for Mr. Barnard, observing the red spot with the 36-inch refractor of the Lick Observatory, says: "The belt south of it seems to be in contact, if it does not actually overlap it slightly." The same thing was noticed as long ago as October 31, 1893, by the aid of the 16-inch refractor of the Goodsell Observatory, Minnesota, when the observers wrote "the great red spot was seen by us, and the colour was exactly the same as that of the belt just to the south of it, and the two objects merged into one another without the slightest change in intensity of colour."

The spot now arrives at mid-transit two or three minutes after Mr. Marth's zero meridian, as the following observations made here will indicate:—

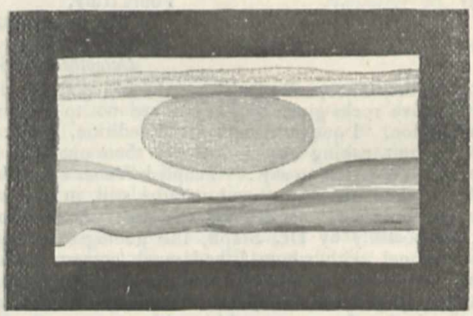
1894	Mid-transit of spot. h. m.	Follows Marth's zero meridian. m.	Long. of spot. °
November 25 ...	... 10 50 ...	... 4·2 ...	... 2·9
December 12 ...	... 9 46 ...	... 1·2 ...	... 0·7
19 ...	... 10 33 ...	... 3·5 ...	... 2·1

It is well known that the perception of a delicate planetary marking depends in a very great measure on the quality of the definition. On an indifferent night the red spot is practically invisible, but with steady air and a sharp image the familiar pinkish ellipse, now representing that object, shows up distinctly in the remarkable concavity of the great southern equatorial belt. The latter configuration always offers an excellent guide to the position of the spot, but its appearance is not symmetrical, the following side of it being much more strongly developed than the side preceding the spot.

The impending disappearance of the latter has been suggested by several observers from its loss of tone during the last few years, but there seems to be no tangible reason to suppose that we are on the point of losing this truly singular object. It is indeed utterly impossible to predicate anything definite as to its future history. The facts gleaned during past observations are not of a character to guide us to any reliable conclusion. Its present faintness may be but the prelude to a more marked manifestation, for variation of tint is certainly one of its leading peculiarities. It is doubtless the same object as the the ellipse seen by Gledhill and Mayer in 1869 and 1870, and quite possibly the same formation was figured by Dawes in 1857 and in subsequent years. At any rate the resemblance of the objects is eminently suggestive, and altogether too striking to be disregarded.

About fifteen years ago the equatorial zone of Jupiter exhibited phenomena proving it to be in a state of considerable activity. There were white and dark spots, wisps of dark material, veins of bright matter, and other irregularities all in condition of rapid change, and imparting a very broken and variegated aspect to the northern side of the great southern equatorial belt. To-day the indications are essentially different. The light tint of the equator seems pretty even, and exhibits few noticeable irregularities. Precisely on the equator there is a dark belt like a narrow pencil line, but it is not now continuous right round the disc. The chief seat of energy appears to have been transferred to the northern side of the great northern equatorial belt.

Dark spots (some of them almost black, like satellite shadows) and white patches, with other details of structure, are plentifully arrayed in the northern equatorial belt. The white spots I observed here in 1885 and 1886, and they have persistently maintained themselves ever since. Their rotation period in 1886 was about eight



1894 November 25, 10h. 9m., 10-inch reflector, power 252.
Region of Red Spot.

seconds less than that of the red spot, but in 1890–91 I found it only 2·6 seconds less. At present the period is still a little less, and there are a number of black spots in the same latitude, and subject to the same drift. One of the most conspicuous of the black spots precedes the red spot about 3h. 20m., and one of the chief white spots precedes the red about 2h. I have secured the following observations of this pair of objects:—

Black spot.			White spot.		
1894.	Mid transit. h. m.	Long. °	1893.	Mid transit. h. m.	Long °
Nov. 5 ...	10 57 ...	239·1	Nov. 23 ...	10 0 ...	303·4
15 ...	9 10 ...	238·2	1894.		
17 ...	10 47 ...	237·6	Nov. 5 ...	12 16 ...	286·9
29 ...	10 39 ...	237·7	15 ...	10 31 ...	287·2
Dec. 4 ...	9 42 ...	235·1	25 ...	8 45 ...	287·0
18 ...	11 14 ...	236·5	Dec. 19 ...	8 24 ...	284·2

The northern temperate belt on which in 1880 and 1891 many rapidly moving black spots appeared, is now in close contiguity with the great northern belt, and blends with it in certain places. A few years ago the two belts were separated by a bright zone.

There is a narrow spotted belt in about latitude 35° north. In 1892 and on subsequent occasions I noticed this belt beaded with numerous dark spots, but I have not made a sufficient number of observations to determine the rotation period. This belt forms a delicate object; it is very narrow, and the spots extremely small. In bad air it is quite obliterated. Only a few observers appear to have noticed this northerly belt and its beaded aspect, but it has been an interesting feature of the planet during the last two years.

In the southern hemisphere and further south than the red spot, there are some dark condensations and white spots, with a rotation period of about 9h. 55m. 20s.

The S. region of the great N. belt exhibits interruptions in the form of very bright streams of material dividing the belt in an oblique direction relatively to the equator. These appearances show rapid changes from day to day, and I have not yet computed the rotation period satisfactorily, but it is obviously very different to that of the red spot. Five observations of these luminous encroachments on the belt have been secured here, as follow :—

	Mid-transit. h. m.	Long. °
November 8	11 0	332·0
15	12 31	359·7
29	9 47	206·1
December 4	8 1	174·1
19	9 33	325·8

It is intended to make a closer study of the variable aspect and rate of velocity of these singular formations.

We may depend upon it, from the number of interesting markings displayed on the planet, that there will be no abatement of useful observations during the present apparition. There is always something new to record, and objects previously known require attentive watching with reference to the differences in their rotation periods. Whether the theorem that the longer an object remains visible the slower becomes its velocity is true or not, cannot be definitely said, but the behaviour of the red spot has not altogether supported the idea. A vast amount of new evidence is required as to the phenomena affecting the surface markings of Jupiter; he presents an ample field both for the observer and the theorist.

W. F. DENNING.

NOTES.

SCIENCE is represented in the list of New Year honours by Mr. W. H. White, C.B., F.R.S., the Director of Naval Construction, who has been promoted to a Knight Commandership of the Bath. Two other Fellows of the Royal Society, Dr. J. Russell Reynolds, President of the Royal College of Physicians, and Dr. Eric Erichsen, formerly President of the Royal College of Surgeons, have each had the honour of a Baronetcy conferred upon them.

THE cross of Commander of the Order of the Ernestine House of Saxony, second class, has been conferred upon Dr. Edward S. Holden, Director of the Lick Observatory, for his services to science. This Order was founded in 1690, reorganised in 1833, and is conferred upon persons holding high official positions, either military or civil. At present there are eighteen Commanders of this class in Germany.

THE fund for the proposed statue to the late Dr. Charcot will probably soon be closed; as it is announced that thirty-five thousand francs have been collected, and this is sufficient to warrant the ordering of the statue. In all probability, a statue will be erected to Helmholtz, for it is reported that, at the recent celebration in Berlin, the German Emperor offered to head a subscription list for that purpose with a donation of ten thousand marks.

THE Paris Chemical Society has (says the *Chemist and Druggist*) recently come into possession of a legacy from M. Rigout, formerly of the École des Mines, who bequeathed to the Society an income of twelve hundred francs per annum, of which a portion is to be set aside for an annual prize to be awarded in connection with inorganic chemistry. M. Rigout declines to allow his name to be attached to this legacy, but asks to have it named the Edouard Rivot prize, in memory of his former Professor. It is worthy of mention that a short time ago M. Silva, late Professor at the School of Arts and Manufactures, left his library and modest fortune to the same Society "out of gratitude for the kindness met with in the French scientific world."

THE death is announced of M. Frederik Johnstrup, Professor of Mineralogy in Copenhagen University, at seventy years of age.

DR. F. KOHLRAUSCH, Professor of Physics in Strassburg University, has been appointed Director of the Imperial Physico-Technical Institute at Berlin, in succession to Helmholtz.

INCREASED facilities for instructing in the science and art of agriculture are gradually being given to the Technical Education Committees in various parts of the country. We are glad to see it announced that Earl Cowper, chairman of the Hertfordshire County Council, has offered to place a farm of nearly 300 acres, with residence and buildings, at the disposal of the Council, rent free, for the purpose of providing practical instruction in agriculture, on condition that the County Council stock the farm and work it. Lord Cowper has also undertaken to erect a laboratory and the necessary dormitories. A sub-committee of practical agriculturists has been appointed to consider his lordship's offer and report to the Council upon it.

A NEW society has just been formed in Paris for the purpose of subterranean exploration, to be called "La Société de Spélæologie." Its projector is M. E. A. Martel, whose works, "Les Abîmes" and "Les Cévennes," may be familiar to some readers of NATURE. In his papers read before the French Association for the Advancement of Science at Besançon in 1893, M. Martel warmly set forth the claims of underground investigation, and the light that might thereby be thrown upon geology, palæontology, and kindred subjects. Indeed, as readers of the works just mentioned are aware, very interesting discoveries have already been made, notably that of the underground rivers of Bramabiau (Gard) and of Padirac (Lot). Among those who have joined the Société de Spélæologie are several well-known men of science. All information is to be obtained of M. Martel, 8 rue Ménars, Paris.

THE idea of holding International Mathematical Congresses is crystallising into shape. Prof. Vassilief, of Kazan, has suggested an assembly of mathematicians in 1896, in order to definitely decide the organisation of such congresses. The matter was pushed a little further at the Vienna meeting of the Deutsche Mathematiker Vereinigung, in September last, when it was unanimously resolved that the committee of the Mathematical Union should take part in framing the necessary arrangements; and the Mathematical section of the French Association for the Advancement of Science have also expressed their support of the scheme. A circular now informs us that the Editors of the *Intermédiare* will be glad to receive the names of mathematicians who are in favour of international meetings of the kind suggested. M. C. A. Laisant's address is 162 Avenue Victor-Hugo, Paris; and that of M. E. Lemoine, 5 rue Littré.

THE *Johns Hopkins University Circular* states that it is proposed to collect the physiological papers and addresses of Prof. Martin, and publish them as one of the memoirs of the Biological Laboratory of the Johns Hopkins University. This plan has been adopted in response to suggestions from a number of his former pupils, that the long and valuable services of Prof. Martin to the University, and his brilliant contributions to American physiology and biology, should be commemorated by a memorial of some kind. After consultation with a number of his friends, it has been decided that the most appropriate form of memorial will be the publication in a handsome volume of his scientific papers and addresses. It is hoped to raise a fund among his friends sufficiently large to meet the cost of an edition of about 300 copies. One copy will be sent to each person or institution subscribing ten dollars, and, if desired, an extra copy for every additional ten dollars subscribed. As the work will be placed at once in the hands of the printer, it is requested that subscriptions be sent promptly to Prof. W. H. Howell, Johns Hopkins University, Baltimore.

THERE are often hidden meanings in the humorous answers given by schoolboys in the examination room. From a collection of such answers in the *University Correspondent*, we cull a few authenticated specimens. "Parallel straight lines," said one boy, "are those which meet at the far end of infinity." And another sagely remarked that "Things which are impossible are equal to one another." The boy who wrote "A point is that which will not appear any bigger, even if you get a magnifying glass," would have no difficulty in understanding that a star, being but a lucid point, cannot be magnified. Every examiner is familiar with the non-committal answers frequently received, and with which may be classified the cautious statement that "Two straight lines cannot enclose a space, unless they are crooked." But even these words of wisdom are eclipsed by the definitions of kinetic and of potential energy once received. "Kinetic energy," ran the definitions, "is the power of doing work. Potential energy is the power of doing without work." This truth, which has a monetary application, is well worth adding to our contemporary's collection.

A SWARM of bees in December is remarkable enough to be put on record. According to a Long Sutton (Lincolnshire) correspondent of the *Daily Telegraph*, a few days ago there was a swarm of bees on a farm at Spalding, Sutton Crosses.

THOUGH the present age is not very favourable to the development of pure science, applied science flourishes like a green bay tree. An instance of this growth is afforded by a note from the Institution of Civil Engineers, with reference to the present membership. The Institution, which was established in January 1818, and incorporated by Royal Charter in June 1878, for the general advancement of Mechanical Science, now consists of as many as 6660 members.

IMMEDIATELY following the recent very high barometric pressure, which reached 30·93 inches on the west coast of Ireland on December 27, a large disturbance advanced over the northern parts of our islands from the Atlantic, and during the night of the 28th, destructive gales from W. and N.W. were experienced over the whole country, accompanied by snow, thunder, and lightning. In some districts the storms lasted for several days, and were followed by sharp frosts, generally. At Greenwich a wind pressure of 28 lb. on the square foot was registered on Saturday morning, this pressure being one pound less than in the severe storm of the previous week.

IN *Naturwissenschaftlich Wochenschrift* for December 9, Dr. C. Hess discusses the hailstorms of Switzerland for the years 1883–1893, on the basis of the observations regularly published by the Meteorological Office at Zürich. He finds that hail is more frequent in the valleys than on the mountains, the latter at times transforming the hail into sleet, or rain. Near marshes, and in the valleys of the lakes, hail occurs more frequently than over wooded country, while the river valleys which lie in the direction of the paths of the thunderstorms favour the formation of hail. On passing over a cultivated district or a hilly forest, there is, however, a tendency to a decrease in the intensity, and, at times, an entire cessation of the hailstorms.

THE prize offered by the Hon. Ralph Abercromby for the best essay on "Southerly Bursters" (NATURE, vol. xlviii. p. 77) has been awarded by the Royal Society, N.S.W., to H. A. Hunt, of the Sydney Observatory, and the paper has been published in vol. xxviii. of the *Journal* of the Society. The essay, which extends to forty-seven large octavo pages, and is illustrated by four photographic plates, contains a short note on "bursters" in New South Wales and other colonies, and gives tabular statements of all that have taken place at Sydney between September 1863 and March 1894. These storms occur very suddenly, and mostly between November and February; a fresh north-easterly wind may change in ten minutes to a gale from the south, doing much damage to vessels that may be unprepared. The storms are always accompanied or preceded with great electrical excitement, and cause a considerable drop in the temperature. The wind velocity used to reach from 60 to 80 miles an hour, and on one occasion attained the rate of over 150 miles in the hour, in a gust. Latterly, however, the wind seldom exceeds 50 miles, and generally ranges between 20 and 40 miles an hour. This result possibly arises either from obstruction to atmospheric disturbance by increased number of buildings, or from less absorption or radiation of heat, owing to greater cultivation of the land. The average annual number of storms is thirty-two. The investigation is the result of much patient research, facilitated by reference to unpublished documents to which access was allowed by the Government Astronomer of the Sydney Observatory.

WE are informed that one of the points in the Report of the Upsala meeting of the International Meteorological Committee, contributed by Mr. Lawrence Rotch to the *American Meteorological Journal*, and reprinted in NATURE of December 20, is misleading. It was stated that "a proposition of the Russian Admiral Makaroff, on the necessity of an international convention to arrange for the discussion of the data contained in ships' logs, was not approved." This is erroneous, for we understand that Admiral Makaroff did not ask the Committee to express an opinion upon his scheme, so the subject was not discussed at all.

THE significant name "*Pithecanthropus erectus*" is proposed by Dr. Eug. Dubois, of the Netherland-Indies Army Service, for some fossil remains recently discovered in the andesitic tuffs of Java, as indicating the former existence in that island of an intermediate form between man and the anthropoid apes. The bones, which consist of the upper part of a skull, a very perfect femur, and an upper molar tooth, are elaborately described and figured in a quarto memoir recently published at Batavia.

THE announcement that the Surinam water-toads, recently received by the Zoological Society, have commenced to show the curious phenomena of their reproduction, in the Reptile House, has created much interest amongst zoologists. There can be no doubt that one of these living specimens now carries a layer of ova placed in cells in the skin of its back, and that about eighteen days after the deposition of the ova, young tadpoles were visible in some of the cells. Thus the extraordinary facts recorded by Madame Merian, concerning the reproduction of this abnormal Batrachian in the latter part of the last century, and not, we believe, subsequently noticed, have been already partially verified.

DR. ANTON REICHENOW, of the Royal Zoological Museum of Berlin, has just completed a memoir on the birds of German East Africa, which will form a portion of the third volume of Stuhlmann's work, "Mit Emin Pascha in's Herz von Afrika." The memoir gives an account of 728 species of birds which have been recorded up to the present time, by German naturalists and other explorers, as met with within the limits of the German Protectorate. The remaining portions of the same volume, which are in progress under the editorship of Dr. Möbius, will complete the account of the vertebrates. The fourth volume, under the same editorship, will be devoted to the invertebrates, and the fifth, edited by Dr. Engler, to the botany of the German East-African Protectorate. Vol. vi. will give an account of the geography and meteorology, and vol. vii. of the geology and mineralogy of the same country. It may be asked how long we shall have to wait for a similarly complete account of British East Africa?

THE "Zoological Record" for 1893, which has just been published by Messrs. Gurney and Jackson, for the Zoological Society of London, has appeared rather later than usual. Its bulk shows us at once that the quantity of work accomplished by zoologists in 1893 is not inferior in amount to that of the immediately preceding years. Some of the recorders, we may be allowed to point out to Dr. Sharp, commence their "Records" with an interesting account of the principal events that have taken place in their special branches of the subject during the year of record. Others altogether neglect this very desirable piece of information. It would be well to insist that a page or two of introduction, containing matter of this description, for the general information of zoologists, should be prefaced to every section of the "Record." Many readers who do not care to study the special portion, would like to get a general notion of what has been going on.

AN exhaustive bacteriological investigation of the Altona water-supply has been recently published by Dr. Reinsch. Although the results obtained are not specially novel, yet they are of importance as confirming the researches of other investigators. It is pointed out that whilst the layer of slime which forms on the surface of sand-filters plays an important part in the retention of microbes, yet another factor must be most carefully watched if a satisfactory filtrate is to be obtained. This factor is the depth of the column of fine sand through which the water is made to pass. It was first called attention to by Dr. Percy Frankland in 1886, in the course of his examinations of the London water supply; and since that time investigations made at Zurich and elsewhere, have shown that it is not advisable to reduce the layer of sand below 30 c.m. Reinsch states that it should never be less than from 40-60 c.m. high. This investigator also states that to encourage the formation of surface-slime, the filters, after the water is first run on, should be left undisturbed for twelve hours, and that the neglect of this simple precaution exercised an important influence on the filtrate.

SOME vexed points in the developmental history of medusæ have been attacked in a masterly manner by Miss (?) Ida H. Hyde, who publishes a paper on this subject in the *Zeitschrift f. wiss. Zoologie* (Bd. lviii. iv.). The material for her investigations consisted of embryos of three different species of *Aurelia* and *Cyanea*. The formation of the endoderm is shown to vary considerably in different species and even in the same species. In *A. marginalis* the endoderm is formed by multipolar delamination, in *C. arctica* by delamination at the pole of the blastopore. In *A. flavidula* the endoderm sometimes arises by embolic gastrulation, with or without the participation of a few immigrant or delaminated cells, but in other cases arises almost exclusively from immigrant and delaminated cells, the invaginated element being relatively insignificant. These differences of origin in the same species seem to be determined by local conditions of temperature and salinity of the water. The author regards multipolar delamination as the most primitive of the various processes by which the endodermal digestive chamber is brought about. The development of the *Scyphula* larva is then traced, and the author is able to confirm Goette's account in all essential points. There is a true stomadæum, and the invaginated ectoderm also takes part in the formation of the four septa and of the gastral filaments of the *Scyphostoma*. The points of affinity which have been raised between the Scyphomedusæ and the Anthozoa thus acquire new and substantial support from Miss (?) Hyde's researches.

A NEW periodical, *Archiv für Entwickelungsmechanik der Organismen* (Leipzig: Engelmann), of which the first number lies before us, is intended by the editor, Dr. W. Roux, to provide a medium for the publication of researches upon the causes of the phenomena of organic development, as distinct from the mere normal order of the phenomena themselves. The journal is to furnish an organ for that branch of biology which has been variously termed the "mechanics of development" and "causal or experimental morphology." In the hands of Roux, Driesch, Hertwig, Chabry, Wilson, and others, the methods of this school, which are above all things experimental, have been applied to various problems in development with much success and with the most interesting results. It is therefore to be hoped that the publication of a journal specially devoted to experimental morphology will lead to a considerable increase in the number of investigations in this promising field. In the present number the editor gives an introduction on the objects, methods, and scope of the branch of biology the new *Archiv* represents, and a number of interesting researches are published on the "cytotropism" of isolated blastomeres, on compensatory hypertrophy of organs and regeneration, and on the origin of the forms of joints.

THE young cocoa-trees in one part of the island of Jamaica are suffering from the attacks of a caterpillar. A correspondent calls our attention to a letter in the *Jamaica Gleaner*, in which Mr. W. Fawcett, the Director of Public Gardens and Plantations, regrets that the museum is at present without the services of a curator, who might be able to give some information as to the best means of dealing with the caterpillars, and the remedy he suggests for keeping down the plague is by hand-picking. In this connection, a summary of a report on a plague of caterpillars at Hong Kong, in the current *Kew Bulletin*, is of interest. These caterpillars appeared on pine-trees, and belonged to a large moth (*Metanastria punctata*, Walker). The trees attacked were those of *Pinus sinensis*, Lamb, very largely planted in the island for re-foresting purposes. Active steps were taken by the Government to destroy the pests, by establishing stations where the caterpillars could be received and paid for by weight. The caterpillars were caught by shaking the trees and picking them off the ground. From the report summarised in the *Kew Bulletin*, it appears that the plague lasted two months. The quantity of caterpillars collected weighed nearly thirty-six tons, and a large number of cocoons were also destroyed. Altogether it is estimated that thirty-five million insects were destroyed. Mr. W. J. Tutcher, who drew up the report, says that the methods employed for the extirpation of the scourge were decidedly successful; so that if similar measures are promptly taken at Jamaica, the caterpillars may be kept under.

IN measuring the volume of a solid by immersion in a liquid, it is usual, after withdrawing the solid, to find what volume of liquid is required to re-establish the former level. The well-known difficulty of identifying the former level is overcome by Sgr. Guglielmo in a manner described in the current *Atti dei Lincei*. He grinds the edges of a beaker, and mounts inside it a pointer of glass or platinum so that its fine point lies just in the plane of the edge. This is accomplished while the mounting cement is still soft, by inverting the beaker on a plane surface, and thus pressing the point into its place. A burette with an india-rubber tube enables the observer to add or withdraw a known volume of water. In order to measure the volume of a solid, the plane horizontal edge of the beaker is lightly spread with grease or paraffin, and the beaker is filled up until the unevenness in the liquid surface produced by the pointer has just disappeared. A volume of water larger than the solid is then withdrawn, and the latter is immersed. The liquid is then brought up to the point again, and the difference of level in the burette is the volume required. The reflection of the bar of a window or other straight line in the liquid is useful for discovering any deformation of the surface. If the pointer should not be easily wetted, a few drops of soap solution are recommended, or the substitution of petroleum for water.

The comparison of condensers of small capacity and the measurement of the specific inductive capacity of liquids is of considerable importance. An interesting paper on this subject was read by Prof. Nernst, of Göttingen, before the German Electro-Chemical Society (see also *Zeitschrift für Physikalische Chemie*, vol. xiv. p. 622), in which he describes a method for measuring the specific inductive capacity and specific resistance of liquids. The author uses a modification of De Santy's method, in which a telephone is used in place of a galvanometer, alternating currents supplied by a small induction coil being used. The resistances employed in the bridge must necessarily be non-inductive, and also, since the capacities to be compared are very small, of practically evanescent electrostatic capacity. Hence the author uses liquid resistances contained in fine capillary tubes, the electrode at one end consisting of a fine platinum wire, which can be moved along the bore of the tube by means of a micrometer screw, and thus the resistance of the liquid column be varied at pleasure. An air condenser is used as a standard with which to compare the condenser containing the liquid under observation, and when this liquid is not a perfect insulator the telephone cannot be brought to complete silence by altering the resistances. To overcome this difficulty the author places a high resistance shunt on the air condenser, which is adjusted so that complete silence is obtained.

A system of two pendulums joined by an elastic thread has been studied by M. Lucien de la Rive, and the results of his experiments are given in the *Journal de Physique*. The masses and the lengths of the two pendulums were equal, and the elastic threads consisted of pure unvulcanised caoutchouc, long enough to make the amount of stretching small in comparison with their length. It was found that there was a periodical transmission of energy from one pendulum to another. On starting one pendulum, and arranging the thread so that it always remained stretched, the other pendulum started swinging with increasing amplitude, until the first was for an instant reduced to rest. After that the second pendulum was gradually reduced to its minimum, which did not, however, attain zero, while the other reached its maximum. This transmission of energy exhibited a fixed period, which could be derived from the ordinary period of each pendulum by multiplying with a number proportional to the length, and inversely proportional to the sectional area of the thread. In the end, the pendulums tend to oscillate like a rigid system, with a constant tension of thread. After ten or twelve oscillations, the alternation of periods is hardly perceptible. When one pendulum only was attached to a fixed point by an elastic thread, the decrement was observed in order to determine the internal friction of the thread. This internal friction gives rise to an elevation of temperature which has been exhibited by Warburg, who placed india-rubber tubes round glass tubes containing sounding columns of air, and observed the rise of temperature in the caoutchouc. It shows an analogy to that in wires due to electric currents.

On October 27, as we have recorded in a previous number (p. 18), a disastrous earthquake occurred in Argentina. The exact time is as yet unknown, and it is therefore uncertain whether any connection exists between this earthquake and a remarkable series of pulsations that were registered on the same day in several European observatories. At Rome, a very slight and comparatively rapid movement of the great seismometrograph (length, 16m., mass, 200 kg.) commenced at 9h. 7m. 35s., p.m. (Greenwich mean time). This lasted until 9h. 40m., when the slow pulsations began to be visible. The first maximum occurred at 9h. 49m. 50s., and the principal one at 9h. 55m. 40s. The amplitude then rapidly diminished, with occasional maxima, the disturbance lasting until about 11 p.m. Pulsations were also recorded at about the same time at the geodynamic observatories of Siena, Ischia, Pavia, and Rocca di Papa. The period of the complete oscillations at the time of their maximum amplitude was found to be 16·7 seconds at Rome, and about 18 seconds at Siena and Rocca di Papa. In the south of Russia, at Charkow and Nicolaiew, horizontal pendulums were strongly disturbed. At the former place, the movement began at 9h. 8m. 36s., p.m., and, for more than an hour, was so great that all traces of the photographic curve disappeared. The large oscillations lasted until 11h. 9m. 48s., and the small ones until 0h. 10m. 54s. a.m., of the following day. At Nicolaiew, the disturbance was first perceptible at 9h. 12m. 6s., p.m., and the curve disappeared almost completely between 9h. 24m. 6s. and 10h. 2m. 6s., the pendulum continuing in oscillation until 0h. 37m. 6s., a.m., of the next day. The magnetographs at Utrecht and Wilhelmshaven also showed traces of the pulsations, at the former place beginning at 9h. 45m. 28s., and reaching a maximum at 9h. 56m. 28s., at the latter beginning at 9h. 55m. 25s. and ending at 10h. 11m. 55s. No disturbances are perceptible on the magnetograms at Lisbon, Perpignan, Paris, Kew, Greenwich, Stonyhurst, Vienna, and St. Petersburg (*Boll. Meteor.*, Suppl. 112).

The last issue of *Studies from the Yale Psychological Laboratory* contains an interesting record of Dr. Gilbert's researches on the mental and physical development of school-children between the ages of six and seventeen years. On the physical side, statistics are given of increase in (*a*) weight, (*b*) height, and (*c*) lung-capacity. On the psychological side, there are observations on the development of the power of perception and discrimination as exercised upon (*a*) weights poised in the hand, (*b*) colour-differences in a series of shades of red, and (*c*) time-intervals. To these are added observations on the "force of suggestion," in which, by means of a series of loaded blocks (1) of different sizes but the same weight, and (2) of different sizes and different weights (the two being nowise correlated), it is shown that visual perception of size very markedly affects weight-discriminations. Reaction time, with and without discrimination and choice, was tested; and observations were made on the influence of fatigue on "voluntary motor ability" as measured by the rapidity of tapping with the key of a specially devised piece of apparatus. In all cases the results, both for boys and girls, are summarised in curves on the graphic method. One of the interesting features to be noticed in these curves is the influence of puberty on mental development, there being a marked depression in the curves at the age of from thirteen to fourteen years. Another noteworthy piece of work, recorded in the same number, is that by Dr. Scripture and Mr. H. F. Smith, which deals with the highest audible tones. The general result is that the pitch of the highest note varies directly and almost proportionally with the intensity. It is also shown that the limit of audibility is much higher when reached by descent from more rapid inaudible vibrations, *i.e.* proceding from silence to sound, than it is when reached by ascending from less rapid audible vibrations, *i.e.* proceding from sound to silence.

THE ninth part of "The Natural History of Plants"—the English edition of Prof. Kerner's admirable "Pflanzenleben"—has been published by Messrs. Blackie and Son.

MESSRS. A. AND C. BLACK have published the hird part of "A Dictionary of Birds," by Prof. Alfred Newton, F.R.S. The part extends from "Moa" to "Sheathbill." We propose to review the complete work when the fourth (and last) part has been issued.

"WEBSTER'S PRACTICAL FORESTRY," published by Messrs. Rider and Son, has reached a second edition. We reviewed the work when it first appeared (NATURE, vol. xlix. p. 526), and it is only necessary to add to the remarks then made that the author has enlarged the volume, thus increasing its value as a practical and popular handbook on the rearing and growth of trees.

THE second volume of Mr. Boulenger's "Catalogue of the Snakes in the British Museum," which has just been issued, concludes the account of the Aglyphodont Colubrine Snakes, and gives descriptions of 427 species, which are represented in the collection by 2528 specimens. We understand that the MS. of the third and concluding volume of this important work is nearly ready for press.

THE December number of the *Journal* of the Royal Microscopical Society has just been received. It contains a paper by Mr. F. Chapman, on the Foraminifera of the Gault of Folkestone (part vii.), and a description of a simple method of measuring the refractive indices of mounting and immersion media, by Mr. E. M. Nelson. The invaluable summary of current researches in zoology, botany, microscopy, &c., takes up the chief part of the journal.

WE note the commencement of vol. xxx. of the meteorological publications of the Manila Observatory. The *Bulletin* for the year 1894 is in a large quarto form, which is much more convenient in size than its predecessors. In addition to a good summary of the weather over the Philippine Islands, the work contains hourly observations for Manila, and observations taken twice daily at a number of secondary stations, together with rainfall and other maps.

MESSRS. WHITTAKER AND CO. will issue the first number of a new science weekly on January 5. The new journal will embody the combined features of *The Technical World* and *Science and Art*, two periodicals which have hitherto appeared as separate organs. It will appeal to all persons interested in the progress of art, science, and technology from an educational point of view. In addition to current news, the journal will contain articles in all the departments of pure and applied science and art.

A FINE geological map of Alabama, drawn on a scale of an inch to ten miles, has been prepared and issued by the Geological Survey of that State. The map is accompanied by a very useful explanatory chart, showing, in four parallel columns, (1) the names, synonyms, classification, and common fossils of the formations; (2) the thickness, lithological and topographical characters, area and distribution; (3) useful products; (4) soils, characteristic timber growth, agricultural features. By means of this chart and the coloured map, the geological record of Alabama can be read by any one.

WE have received from Dr. A. von Danckelman, an excerpt paper from vol. vii. of "Mittheilungen aus den deutschen Schutzgebieten," containing some valuable meteorological observations made by Dr. Steinbach, during the year 1893, at Jaluit, an island in the North Pacific. The station is situated in 5° 55′ N. lat. and 169° 40′ E. long., and is only five feet above mean sea-level. A Richard barograph was sent out by the Deutsche Seewarte in 1892, but the vessel was wrecked on approaching the island; another was supplied, and records were commenced on January 1 last. The observations for 1893 show that the air temperature is exceedingly uniform, the yearly range only amounting to 22°; the absolute maximum was 92°·8 in November, and 70°·7 in September. Cloud is very prevalent, there only being six clear days during the whole year. Rain falls almost daily, there being 343 wet days during 1893, the total fall being 182 inches; the greatest fall in twenty-four hours was 4·5 inches. Thunderstorms are not very frequent; there were only thirty-two in the year in question, but it is noteworthy that the majority occur in the forenoon, to an extent which has, we believe, not been observed at any other station.

THE additions to the Zoological Society's Gardens during the past week include a Bonnet Monkey (*Macacus sinicus*) from India, presented by Mr. J. Hussey; two Red-eared Bulbuls (*Pycnonotus jocosus*) from India, presented by Brigade-Surgeon Lieut.-Colonel E. F. Drake Brockman; a Cape Bucephalus (*Bucephalus capensis*), a Rhomb-marked Snake (*Psammophylax rhombeatus*) from South Africa, presented by Mr. J. E. Matcham.

OUR ASTRONOMICAL COLUMN.

A NEW SHORT PERIOD VARIABLE.—The possible variability of *d* Serpentis was suggested some time ago by the Potsdam photometric work, and the variation has now been confirmed by Mr. Yendell (*Astronomical Journal*, No. 331). Fifty observations made by him between August 5 and November 4 of last year indicate a variation from magnitude 5·0 to 5·7 in a period of not far from 8·7 days. The form of the light-curve appears to resemble that of β Lyræ, though this is not yet certainly established. There appear to be two maxima, one at 2·2 days, and the other at 6·2 days from the principal minimum, while there is a secondary minimum of about magnitude 5·5 at 4·3 days from principal minimum. Mr. Yendell suggests the provisional elements

1894, August 7·6 (G.M.T.) + 8d.·7 E.

The new variable is chiefly of interest in the probable resemblance of its light-curve to that of β Lyræ. Notwithstanding the very detailed investigations of the spectrum changes in the latter variable, which have been recently made at Harvard, Potsdam, Pulkowa, Stonyhurst, and Kensington, the cause of the variability is by no means completely understood. In this case the spectrum is a complicated one, consisting of bright lines as well as dark ones, and the Kensington photographs have shown that there are two distinct sets of the latter. According to the Draper catalogue, however, the spectrum of *d* Piscium is of the Sirian type, and no mention is made of bright lines. It may be that a complete study of the simpler spectrum of this star will throw some light on the origin of the variability in β Lyræ, and possibly also on the causes which produce other kinds of continuous variability in short periods.

The position of the star for 1900 is

R.A. 18h. 22m., Decl. + 0° 8′.

DOPPLER'S PRINCIPLE.—The verification of Doppler's principle has hitherto depended upon comparisons of the spectroscopically measured velocities in the line of sight of Venus, and of the sun's limb with their known velocities, and upon the consistency of the measured velocities of stars when due allowance is made for the varying effect of the earth's movement. Dr. Belopolsky, however, has recently suggested a method (*Astr. Nach.* No. 3267) of demonstrating the principle by a laboratory experiment. Imagine two cylinders with axes parallel, on which are arranged vertically two sets of small mirrors, *a*, *b*, *c* . . ., *m*, *n*, *o* . . . The mirrors are inclined so that a ray of light falling on *a* is reflected to *m*, back again to *b*, then to *n*, and so on, finally being directed to the slit of the spectroscope. If now the cylinders be set in rapid rotation, in opposite directions, the effects of the mirrors will be to accelerate or retard the velocity of the ray of light according to the directions of movement. Figures are given which indicate that measurable displacements may be obtained by employing cylinders of from 0·25 to 0·7 m. diameter, with ten or twenty mirrors, making from eighty to twenty revolutions per second. With cylinders half a metre in diameter, making eighty revolutions per second, and having twenty mirrors, the equivalent of a velocity of five kilometres per second in the line of sight is obtained, and with modern instruments there would be no difficulty in observing the effects of such a movement. It is pointed out that dynamos already make from seventy to eighty revolutions per second, and if sufficient care be taken in the construction of the cylinders, the experiment does not seem to present insuperable difficulties.

"THE ASTROPHYSICAL JOURNAL." This is the title under which *Astronomy and Astro-Physics* will appear in future. The University of Chicago has purchased that journal, and arrangements have been made to carry it on under the editorship of Profs. Hale and Keeler, assisted by Profs. Ames, Campbell, Crew, Frost, and Wadsworth, and a number of associate editors, five of whom represent Germany, Great Britain, France, Italy, and Sweden. The scope of the new publication will be quite as broad as that of *Astronomy and Astro-Physics*, for all problems and investigations of modern physical astronomy will be dealt with. The journal will, in fact, represent the common ground of physics and astronomy, and it should therefore be appreciated by the physicist as much as by the astronomer. Workers in astronomical physics should be gratified that their science has progressed sufficiently to need a special organ to be devoted to its interests. *Popular Astronomy* will be continued as heretofore, and will appeal to all amateurs, teachers and students of astronomy, and others interested in celestial science. This journal will be practically the old *Sidereal Messenger*, and the *Astrophysical Journal* will be the realisation of Prof. Hale's original plan of a periodical for workers in the domain of the new astronomy.

ELLIPTICAL ORBITS.—A little pamphlet on elliptical orbits, by Mr. H. Larkin, has been published by Mr. Fisher Unwin. If it should induce any student of astronomy to construct for himself the curves in which the apparent orbit of a binary star, as we see it, may be resolved, it will serve one useful purpose, and probably the only one. For it is to be hoped that the same student will not embrace the theory of formation of binary stars as given here, wherein they are made to result from the explosion of one larger star. There are many other strange things taught, of which Newton never dreamed, and which no one capable of reading Newton would ever have stated. Perhaps the most singular is the suggestion of "precarious equilibrium" that would accompany motion in a circular orbit. "Precarious equilibrium" is a curious term, but the context seems to suggest that some catastrophe or disaster would result from such a condition. The author does not seem to have realised that there is no sudden and violent change between an ellipse of small eccentricity and a circle, and that in the solar system there are instances in which the eccentricity (if existing) is so slight that the motion can be conveniently considered as circular.

FORESTRY IN NATAL.[1]

NATAL lies between latitude 28° and 31° S. The climate of the coast is almost tropical, owing to a warm current from the equator. Mangrove trees grow along the coast, and sugar-cane and tropical Indian fruit trees are cultivated there. The land ascends rapidly inland, and the capital, Pietermaritzburg—or Maritzburg, as it is usually called, at fifty miles from the coast—is at 2275 feet above sea-level, and possesses an appreciably mild climate.

The colony is bordered on the west by the Kathlamba or Drakensberg, a mountain chain attaining altitudes which exceed 9600 feet, and separating Natal from the Transvaal, the Orange Free State, and Basuto-land. These mountains form the eastern boundary of the high South African plateau, which is drained by the Orange River and its tributaries.

Natal is scantily populated, containing 18,755 square miles, with 532,000 inhabitants, of whom 38,000 only are Europeans. Most of the latter are English who came by sea and founded Port Durban, but a few are descended from the Dutch Boers who came from the west in 1838–42 and founded Maritzburg. Natal has been an English colony since 1843, when the territory only included 3000 native inhabitants, but their numbers rose rapidly to 100,000 in 1845, and to 400,000 in 1883. They are mostly Zulus in the north, and Kaffirs in the south of the colony.

Much greater progress could have been made in Natal, in trade, agriculture, and manufactures, if it had been connected by roads and railways with the Transvaal and Orange States. The Cape railway, 650 miles long, from Cape Town to Kimberley, with express trains doing the distance in thirty-six hours, has long been constructed, and in 1893 this railway was extended to the Transvaal gold mines at Johannesberg.

In 1880 a railway was constructed from Durban to Maritzburg, but only recently has it been pushed further inland, and it now reaches the confines of the colony. Its further extension to Johannesburg is most important for the future prosperity of Natal. About one and a half years ago a railway was made from Ladysmith in Natal to Harrismith in the Orange State. Natal is at present short of funds, and this may partly explain why, having made a good start in forest conservancy, the Colonial Government has not had the resolution to persevere in it.

The Cape Colony has had for some time a good forest administration which was organised by a French forest officer, Count Vasselot de Regné, Conservator of Forests in Algiers, and Mr. D. E. Hutchins is now Chief Conservator of Forests at Cape Town. He was trained at Nancy for the Indian forest service, and left it for service at the Cape in 1883. Mr. Fourcade, of the Cape forest service, was employed in Natal for nine months in 1889, and has written a very valuable paper on the Natal forests, but he declined to quit the Cape service permanently for that of Natal, and was succeeded in 1891 as chief forest officer there by Mr. Schöpflin, a Baden forest officer.

The work he undertook of organising a forest department in Natal was full of difficulty, especially as the forest revenues were not expected to cover the expenditure for a number of years. Irrespectively of the continual clearance of forests for the extension of agriculture, forest fires, unregulated grazing, and wasteful timber felling have so exhausted the Natal forests that the areas still covered with brushwood and forest are widely scattered over the country, and only a small percentage of them is still State property.

From the coast to altitudes of about 975 feet, with an average annual temperature of 67°–71°, the forest consists of numerous species belonging to the tropical flora. The woods are not more than 30–60 feet high, but something might be made of them, as several species yield valuable timber. Unfortunately nearly all the coast forests are now private property.

In a central zone ranging in altitude between 980 and 3450 feet with an average annual temperature of 59°–67° F., extensive tracts are covered with so-called mimosa scrub, formed of several species of *Acacia*; these woods are very thinly stocked, and contain a tall grass undergrowth. The acacias bear plenty of seed, and young growth exists, but is continually being destroyed by the annual grass fires. If only protection could be afforded to these forests against fire, as has been done for the last thirty years in British India, they could be worked profitably with short rotations—twenty-four years, according to Mr. Fourcade; it is, however, probable that most of this area will be cleared for agriculture.

The present area of the coast and acacia forests is estimated at 196,000 acres of State forest, and 1,645,000 acres in private hands.

In the higher zone, from 3450 feet up to 9600 feet, with a temperate climate, and an average annual temperature of 52°–59°, the most valuable forests are situated, but they are

[1] Translation of a paper by Sir Dietrich Brandis, K.C.I.E., F.R.S., in the *Forst und Jagd Zeitung*.

scattered over a difficult mountainous region. Of these forests, the State possesses 54,000 acres, and 27,000 acres are in a territory assigned to the indigenous inhabitants of the country. The Government has decided that in the mountains the action of the Forest Department will be restricted to the State forests. They contain many species; *Podocarpus Thunbergii* and *P. elongata*, both known as yellow-wood, are the commonest, and their wood resembles that of the European spruce. At present the great cost of transport prevents the profitable working of yellow-wood. Amongst the remaining species, the two most valuable trees are stink-wood (*Ocotea bullata*), so named on account of the bad odour of freshly-sawn wood, an evergreen lauraceous species with a beautiful brown heart-wood, which is hard and tough; and sneeze-wood, *Pteroxylon utile*, an ally of the horse-chestnut. These woods are also highly esteemed in the Cape Colony, especially for cart and wagon making, and can be worked at a profit even from these remote mountain forests. Unfortunately these two species are only found here and there in the forests, and there is no large supply of them.

In the year 1891-92, the sale of wood by the Natal Forest Department yielded £725, while the expenditure was £1942, partly for establishment and partly for the survey of the forests. Owing to the small area of forests available, and the remote position of the State forests, Mr. Fourcade strongly recommended that plantations should be started near the towns and railways. Past experience with the blue gum (*Eucalyptus globulus*) is favourable for the success of this tree in Natal. At Arambi, near Ootacamund in the Nilgeri Hills in India, this tree attains a height of 107 feet in nineteen years, and yields 8696 cubic feet per acre. This enormous production of 457 cubic feet per acre annually was attained in latitude 11° N. at an altitude of 7426 feet above sea-level.

In the higher latitude of Natal, a similar climate to that of Arambi is found at 2275 feet above sea-level, and, according to Mr. Fourcade, mixtures of *Eucalyptus globulus*, *longifolia* and *rostrata* give an even higher yield near Maritzburg than at Arambi. Mr. Schöpflin doubts whether this will be the case; but, at any rate, the gum-trees will give a large yield, and if the wood were only fit for fuel a considerable pecuniary return would be obtained. Several of the gum-trees, however, yield splendid timber, and especially *E. rostrata*, the red gum of Southern Australia.

Timber imports into Natal average in value £180,000 a year, so that, as the indigenous forests are small, much subdivided, and unfavourably situated, the State is clearly called upon to plant up a sufficient area of the State lands. Mr. Fourcade states that the land necessary for these plantations can now be purchased cheaply, and Mr. Schöpflin commenced planting operations. This useful measure is now abandoned, owing to want of funds, and the plants in the State nurseries will be sold.

Besides *Eucalypti*, several Australian acacias succeed admirably in Natal, especially *A. decurrens* and *mollissima*; their bark is rich in tannin, and a plantation of 1200-1500 acres of these trees has been started by a private company. Near the neighbouring Transvaal gold-fields, Australian trees are being planted on a large scale to supply mine-props.

The length of rails in Natal is about 625 miles, and the mountain forests will yield a portion of the necessary railway sleepers. Yellow-wood must be kyanised, as has been done in the Cape Colony, and kyanising works can easily be established in Natal, and wood from gum-tree plantations ought to supply the balance of the sleepers required.

It is evident that Natal cannot possibly prosper without a Forest Department, and the colony will have cause to regret having abandoned the attempt to form one, after such an excellent beginning has been made. The Government wished to retain Mr. Schöpflin's services up to March 31, 1894, but would not undertake to employ him after that date. Under these circumstances, he was obliged to resign his appointment last September, in order to return to the Baden Forest Service.

W. R. FISHER.

THE FERTILISATION OF "LORANTHUS KRAUSSIANUS" AND "L. DREGEI."

THE parasite *Loranthus Kraussianus* grows on the coast here on the tree *Chætacme Meyeri*, and as three of these trees grow within a short distance of my house, I have this season had a good opportunity of observing the rather curious mode of its fertilisation. In the flower bud the corolla segments adhere along their whole length, forming an upright cylinder, of about an inch long, of red and white, thus getting the not inappropriate colonial name of "lighted candles." The flowers grow in close umbels, so close together as to give quite a reddish tinge to the host tree. After a little time five slits appear about half-way up the upright cylindrical corolla, and these slits are about one quarter the length of the cylinder. The anthers occupy *almost* the extreme tip of the cylinder, and are pressed against each other by the closed tube of the corolla (the cylinder aforesaid), the actual tip being occupied by the capitate stigma. If a needle be inserted into one of the slits of the corolla with a downward movement, as if to seek the nectar at the base, it causes the tube to split with some force, and at the same time the anthers are quickly and forcibly released from their pressure one against the other, and fly downwards violently, scattering practically all the pollen they contain by the one movement; and at the same time the stigma, from being upright, springs to an angle of, say, 40 degrees on one side quite clear of the now split corolla tube. I found by microscopic observations of a number of stigmas just at this stage, that only in a small proportion of cases (I only found one) did any of the triangular pollen actually reach the stigma by the act of explosion, although the style was fairly thickly peppered. These flowers are constantly being visited by large numbers of the commonest coast sunbird (*Cinnyris olivaceous*), a very active and hard-working, though not very brightly coloured, member of the sunbirds. A little quiet watching will show the birds at these flowers splitting open flower after flower, and getting head and bill covered with pollen in moving about, undoubtedly fertilising the capitate receptive stigmas (in the receptive stage protruding free from the corolla tube) of other and older flowers. After seeing them thus at work, the question arose whether without their aid the bursting of the flower happened. The negative evidence was that although I had observed for many hours, I never saw a simple flower voluntarily explode; but to check this, I put a net-bag over a small branch containing, say, 80 to 100 healthy flowers. I found that when thus protected hardly a single flower got to a further stage than having the splits on the corolla tube ready for the outside aid of the sunbird to enable them to perform the next function, viz. explosion. Actually none exploded, and, as a consequence, not a single flower within the bag set seed. They seem to be quite sterile without outside help, the anthers dehisce, but at a level below the capitate stigma, and as the corolla tube is generally upright the pollen is lost even as a self-fertilising agent. After careful watching, I feel sure sunbirds are the only effective agents in the fertilisation of this plant. At first I never observed bees visiting it, and actually made a note to the effect that they did not do so; but at a later date they came in good numbers. They seemed simply to follow the birds, and take any nectar left by them in the exploded flowers, and very seldom, and then, I think, only by a happy chance themselves caused the explosion. I did not observe any other insect visitors, so that it would appear this plant is dependent on *Cinnyris*; and there is an element of irony in it, for from the berries of this plant the boys make bird-lime, and the energetic efforts of these lovely little birds are towards the perpetuation of the means by which they are often made captive. It would be interesting to know how far the different individuals of *Loranthus* on the one tree are in the position of independent individuals of terrestrial species (pollen from an *independent individual* being necessary for the most perfect results of cross-fertilisation), or whether the fact of having a common host approximates them in this respect to the position of one plant, and whether to get the best results of cross-fertilisation pollen should be brought not from flowers of a different individual on the same host, but from plants growing on a different tree altogether. To carry on the life-history of this plant, my friend Mr. Harry Millar, of Durban, informs me that the berries when ripe are taken by the little tinker bird (*Barbetula pusilla*), who eats the *covering* of the berry, and rejects the seeds and viscid matter around them, and to clear away the latter bangs the berry with his bill against a tree, where the seeds adhere with the viscid substance and germinate. I may say that Mr. Millar states that in shooting these birds, as specimens, he often finds the head and bill covered with pollen. I am informed that another sunbird (*Cinnyris Verreauxi*) visits this plant, but as it is of the same habits as *C. olivaceous*, the results of its visits, as far as the plant is concerned, would be the same.

Loranthus Dregei grows on the coastlands of Natal upon various hosts, most commonly perhaps upon the introduced Syringa, *Melia Azedarach*, and never, so far as I have observed, upon the tree (*Chætacme Meyerii*) infested by *L. Kraussianus*. While in the bud the petals form a cylindrical tube, and the anthers are pressed against the closed petals, the tips being just below the stigma. Subsequently slits appear on the still closed cylinder. My observations show that the plant is abundantly visited by *Cinnyris olivaceus* and *C. Verreauxi* and that both birds insert their long bills into the slit to get at nectar secreted at base of tube, exactly as in *L. Kraussianus*. In this species, however, instead of the anthers remaining still attached to the filaments when the flower jerks open, they are all broken sharp off, and fly off into space with great violence, parting with their pollen as they go. I find that although the pollen is thrown so far upwards as to reach the base of the stigma, the force appears so nicely adjusted that none actually reaches it, the great bulk of the cloud of pollen being thrown downwards so as to reach the head and beak of the visiting bird. Apparently after this dissemination of its pollen (and anthers) the flower still has attractions for the sunbirds, for I have seen them distinctly visiting the burst flowers, and this would of course be necessary if cross-fertilisation or, indeed, fertilisation of any kind took place. And on opening the burst flowers I found in most cases a quantity of nectar, so that probably secretion goes on after the flower is open and its anthers gone. I observed this plant repeatedly and at all hours of the day, and never saw it visited by a single insect of any kind; and although aware that negative evidence of this kind cannot be relied upon, my observations were so frequent that I feel sure any insect visitants, at all events diurnal visitors, must be exceedingly rare. I noticed one flowering upon *Acacia* sp. which was also in flower and visited by bees, but the bees took not the slightest notice of the flowers of the *L. Dregei*. I should judge from the length of the corolla tube, that if any insect visits this plant, it must be furnished with a long proboscis, for the flower tube from stigma to base is fully two inches long. As in *L. Kraussianus* the flowers need outside aid to burst at all, for I have watched them for long periods and in all kinds of weather, and never seen a single flower burst by its own volition. Although apparently entirely dependent on the sunbirds for its propagation, this mode of fertilisation must be very successful, for the plant is very common indeed. In addition to the fact of the flying pollen never reaching the stigma, and self-fertilisation being thus prevented, the flower seems to be proterandrous, for at a stage of development when a slight touch in the right place bursts the flower, the stigma seemed dry and unreceptive. After bursting, the stigma, instead of being in line with the corolla tube, inclines to one side, though not to such an extreme angle as in *L. Kraussianus*, and this deviation from the upright will help pollination to some extent. I have often watched the birds on these flowers bursting them, and each time causing quite a little cloud of pollen and anthers to fly, and the force is so great, the anthers are jerked to quite a considerable distance, and in no single instance did the force fail to detach the whole of them. It is a very pretty and interesting sight. In the case of this species, I believe it is absolutely dependent on the sunbirds for its sexual propagation.

Durban, Natal. MAURICE S. EVANS.

SCIENTIFIC INVESTIGATION IN CANADA.[1]

IN a Society formed to include as far as possible representatives of all branches of literature and of science, it appears to be most appropriate that the president for the time being should devote the address which it is his privilege to deliver, to some specific topic, or to the consideration of such matters of interest or importance as may lie particularly in his own line of work or thought. The literary, artistic, and political development of the country have already been dealt with. It may therefore be of some interest and service to give a very general and very brief review of what has been accomplished, and what remains to be accomplished in Canada, by various scientific agencies working in the investigation of the natural features, and towards the development of the natural resources, of the country.

Science is but another and a convenient name for organised knowledge, and as such it has entered so largely into every branch of human effort, that when, at the present time, any one attempts to pose as a "practical," in contradistinction to a scientific worker, he may be known to be a relic of the past age, in which much was done by rule of thumb and without any real knowledge of the principles involved. Neither can any division be made between what is sometimes called "practical" or "applied" science and science in general, for the knowledge must be gained before it can be applied, and it is scarcely yet possible to bar any avenue of research with a placard of "no thoroughfare," as an assurance that it cannot lead to any material useful end.

At the same time, there are certain directions in which investigation is very closely wedded to results of immediate and tangible value. But the line should not be too rigorously drawn, for should the investigator for a time stray into some by-path of research, because of his individual interest in his work, it is not improbable that he may return from his excursion with some unexpected discovery, which may prove to have important bearings on the problems of every-day life. Take, for example, the study of palæontology, which, relating as it does, to extinct forms of life, might appear to be a branch of science wholly removed from any practical object, however interesting it may be to disinter and to reconstruct these remarkable forms. But we all know that this study has become an indispensable one as an aid to the classification of the rock formations, and thus to the search for the useful minerals which some of these contain. This is more particularly the case, perhaps, in the instance of coal beds, which are usually confined in each region to some set of strata, which may be defined with precision only by the aid of the evidence afforded by fossil remains.

THE GEOLOGICAL SURVEY.

In the first united Parliament of Upper and Lower Canada, in 1841, the Natural History Society of Montreal and the Historical Society of Quebec joined in urging the establishment of a Geological Survey upon the Government, with the result that the modest sum of £1,500 sterling was granted for the purpose of beginning such a survey.

Mr. Logan, afterwards so well known as Sir William Logan, was the first geologist appointed. He entered upon his duties, in 1843, with the greatest possible zeal, and for more than twenty-five years the history of the Survey and that of its director were the same.

The field work of the Geological Survey necessarily began with exploratory trips in which the main features to be dealt with, in a country almost entirely unknown geologically, were ascertained. In many parts, even of the older provinces, such explorations are still requisite, but in most of these provinces it became possible after a time to proceed with the more systematic mapping of definite areas, the map-sheets produced forming parts of a connected whole. When the great western regions were added to the field, these could only be attacked by extended exploratory journeys in which geology and geography went hand in hand. As it is now, the field work of the Survey may be divided under three classes: (1) Reconnaissance surveys; (2) the approximate mapping of large areas on a small scale; (3) finished map-sheets on a larger scale, and forming continuous series. All these three classes of work are in progress concurrently in different districts, while the auxiliary chemical, palæontological, and lithological investigations in the office are kept in touch with the field work, and render it possible to bring this together in a homogeneous form. Were there in existence any complete topographical maps of Canada, approaching in accuracy to those which have been made in older countries, much more geological work could be accomplished with a given amount of money and in a given time, and thus the construction of such maps must be stated yet to be, as it has been from the beginning of the Survey, one of the principal desiderata. There is, however, one other matter which at the present moment must be regarded as even more urgent, and one which might be attained within a short time and at a relatively small cost. This is the construction of a suitable and safe museum building for the preservation and display of the important collection which has grown up as the result of so many years of investigation. This collection is not merely a matter of record, closely connected with all the publications of the Survey, but it is fitted to become also a great educational medium in regard to the mineral resources of the country. With proper accommodation its utility could be vastly increased for all purposes.

[1] Abstract of an address delivered before the Royal Society of Canada, by Dr. G. M. Dawson, C.M.G., F.R.S.

Nothing can be adduced which is more creditable to the system of government in Canada, than the quietly persistent and uninterrupted support accorded to the Geological Survey by every political party; but it remains to provide such a museum building and centre for the work as that referred to, and it may be confidently asserted that nothing would be more favourably received by the general public. This museum should be of a national character, and there is every reason to hope that when it is undertaken, its plan will include provision for all the valuable collections which have been or may be made by the several Government departments, so that it may form in effect a representation of the sources, the history and the various lines of activity of the whole country.

METEOROLOGICAL SERVICE AND MAGNETIC OBSERVATORY.

Although the first scientific branch of the Government service established by Canada, it must be noted that several years previous to its inception the Magnetic Observatory had been founded at Toronto. It was established as the result of representations made by the British Association at its meeting in Newcastle in 1838, acting in conjunction with the Royal Society of England, and as a part of a system of magnetic research on sea and in the colonial possessions of Great Britain. The observations were actually begun in 1839. Meteorological observations had been made concurrently with those relating to magnetism, from the time of the establishment of the observatory; but it was not until 1871 that the Canadian Government first made a grant of 5000 dols. for a meteorological service.

In 1876, the issue of daily weather forecasts and storm warnings was begun, and since that time these have become so much a part of the every-day life of the country, that it is unnecessary to enter into any explanation of their character, or to present any plea in their favour. They are equally important and necessary to the farmer as to the navigator, and are, in addition, of value in a hundred other ways.

There are at the present time over four hundred stations in Canada reporting to the central office, of which twenty-nine make daily telegraphic reports, useful primarily in affording data for the weather forecasts. The meteorological service thus developed naturally from the Magnetic Observatory, and both have become merged in a common organisation, the growth of the meteorological work now perhaps overshadowing the original magnetic purpose of the observatory in its immediate interest, though the importance of the magnetic observations has never been lost sight of.

Respecting magnetic charts of the Dominion, much also remains to be done, for though scattered observations of precision have been made, particularly in the west, no systematic attempt at a magnetic survey has been undertaken since that accomplished in an extended journey through the northern parts of the country in 1842 and 1843, by Sir J. H. Lefroy. It is well to remember that the magnetic pole itself is situated within the limits of Canada, and that problems of the greatest importance, both from a purely scientific and from a practical point of view, call for solution by a systematic study of its secular movement, as well as of any changes in intensity and dip by which this may be accompanied. These are all strictly domestic problems, and they should not be left for solution to enterprise from abroad.

EXPERIMENTAL FARMS.

This branch of the public service was established as the result of the recommendation of a select committee of the House of Commons appointed in 1884 to inquire into the best means of encouraging and developing the agricultural resources of Canada. The "Experimental Farm System Act" was passed in 1886, and the organisation of the work began in the same year.

It is thus only about eight years since the initial steps in this new scientific enterprise of the Government were taken, but in that time, thanks to the energy and ability of the director and staff of the farms, great progress has been made, and the way has been opened in many directions for still further usefulness. Besides the central farm at Ottawa, which was first undertaken, branch farms have been established for the maritime provinces, Manitoba and the north-west territories, and British Columbia.

If any line can be drawn between that which may be described as strictly practical and that which may be called purely scientific work, it will be found to run through the centre of the field of operations of the experimental farms. An inspection of the reports already published will show that the work consists largely of submitting actual observations in the field to scientific tests, and in the application in turn of the best results of scientific knowledge to matters of every-day importance on every farm throughout the land.

The following are among the many lines of work undertaken in this service:—

One is the origination of new crosses or hybrids of cereals, fruits, and other useful plants to meet the requirements of the varied climates and conditions of different parts of Canada.

Other branches of the work involving much original research are: the investigation, by chemical analysis, of soils, in their relation to fertilisers, and of grains, grasses, fodder plants and other products of the farm, by which a fundamental knowledge of their respective value and of the best and most profitable methods of their treatment may be arrived at, and the study of insects and parasitic plants injurious or beneficial to vegetation and to stock, such as to enable the pests of the agriculturist to be combated either by methods which may be classed as direct, or by means which are indirect. The latter implies a study of the life-history of the forms to be dealt with, including not only those which are native to the country, but those also which may be from time to time introduced, such as the Colorado potato beetle, the horn fly, and many others. It includes also the study of the best means of counteracting the attacks by all those lower forms of vegetation, known as rust, smut, mould or mildew, that prey upon the plants which are the special care of the farmer.

Even in connection with the familiar and almost world-old operations of butter and cheese making, the results of purely scientific investigations are now being proved to have a great importance. The best mechanical methods of dealing with the milk from which these are made, are not here referred to, but the fact that the nature of the vegetable ferments which act upon this milk and upon the cheese, after it has been produced, are now known to give character to the product; that is to say, the effect of inoculation of the mass with some particular species of ferments is favourable, while the presence of others is deleterious. Thus the results obtained in the whole field of bacteriology are being made contributory to the success of the dairy. Already in Denmark "pure cultures" of certain kinds of ferments are beginning to be regarded as necessary to the success of the butter-maker, and essays of a similar kind are actually in progress here.

It is not possible to refer in detail to the numerous experiments and tests, completed or in progress, of varieties of plants and animals which may be already well known, but of which it is desirable to ascertain those best suited to the actual circumstances of the country. Nor is it possible to enter into questions such as the tests of fertilisers, the testing of the vitality of seeds, or the propagation of trees suited for planting on the plains of the north-west. Though a part of the useful work of the farms, these do not imply original research in the same measure with those subjects already alluded to. Neither is this the time to dwell upon the methods adopted of making the information gained available to the public, such as the publication of special bulletins and reports of progress, the distribution of samples of seed grain (which in 1892 reached the number of 30,000) and of young trees for plantations.

Before concluding this brief review of the several branches of scientific research or work carried on by the Government, allusion must be made to several comparatively late undertakings of this nature begun under the auspices of the Department of Marine and Fisheries.

Under the name of the "Georgian Bay Survey," a hydrographic survey of the Canadian portion of the Great Lakes was begun in 1883, and several excellent charts of the northern part of Lake Huron have already been published.

When the British Association met in Montreal in 1884, a committee of that body, which had for many years been engaged on tidal determinations, interested itself in the extension of such observations to Canadian waters, and a joint committee of the Association and of the Royal Society of Canada was formed, by which the importance of such observations, made systematically and with modern appliances of accuracy, was urged upon the Government. In 1890 a beginning was made in this work, and provision has since been made for its continuation and extension.

Another promising departure is the initiation of a scientific study of that most important element in the welfare of the country, the fisheries. Much has already been done in Canada in the matter of the propagation of food fishes, but much yet remains to be done in investigating the conditions of the fisheries of both salt and fresh waters, and it may now be anticipated that before many years an important basis of fact will have been built up upon this subject.

One important line of inquiry must yet be mentioned in which no systematic beginning has been made, either under the auspices of the Government or by any society or institution especially devoted to it. This is the field of ethnology, which in Canada is a very extensive one, and which calls for immediate effort, inasmuch as the native races, with which this study is concerned, are either rapidly passing away or are changing from their primitive condition.

Ten years ago, the Council of the British Association was so much impressed with the urgency of investigations of this kind, that it not only appointed a committee to deal with the subject, but has since given each year a substantial grant from its own funds in aid of this work. The Canadian Government for several years supplemented this grant, and eight reports, filled with valuable observations on the western tribes, have so far, as a result of this action, been published in the annual reports of the Association. It has been decided, however, that the functions of the committee, with the grant accorded by the Association, shall cease this year, so that if further progress is to be made, the matter must now be taken up by the Canadian Government. It is earnestly to be desired that the Government may at least contemplate the attachment, either to the Indian Department or to some other department, of a properly qualified ethnologist, by whom these investigations may be continued.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, December 6, 1894.—"Experimental Researches on Vegetable Assimilation and Respiration. No. I. On a New Method for Investigating the Carbonic Acid Exchanges of Plants." By F. F. Blackman, B.Sc., Demonstrator of Botany in the University of Cambridge.

This paper consists of a description of a complicated apparatus for the estimation of very small amounts of CO_2, which is especially adapted for biological research.

By its aid the evolution of CO_2 by a single germinating seed, or by a small area of a foliage leaf, can be accurately estimated from hour to hour without a break, for any desired time, while for the same area of leaf the more active absorption of CO_2 in assimilation can be easily determined for such short periods of time as fifteen minutes, and that at the same time separately for the two surfaces of one and the same leaf area. Further, for the purposes of this assimilation, air containing any proportion of CO_2, however small, can be supplied continuously to the tissue under investigation. The apparatus is practically in duplicate throughout, and strictly comparative double experiments can be performed.

The experiments are carried out in a continuous current of air at atmospheric pressure; the actual estimation of the CO_2 is accomplished by leading this through baryta solution, of which only a small quantity is used in each case, and the whole of it afterwards titrated *in situ* in the absorption tube, to which only air freed from CO_2 is otherwise admitted.

The following communication illustrates the applicability of this apparatus to the investigation of minute quantities of carbon dioxide:

No. II. "On the Paths of Gaseous Exchange between Aerial Leaves and the Atmosphere."

Conclusions.—(1) That if the amounts of CO_2 evolved in respiration by the two surfaces of any leaf area be determined, it will be found that there is a very close relation between these amounts and the distribution of the stomata. In those leaves with no stomata on the upper surface, practically no CO_2 is exhaled from that surface, and all escapes from the lower surface. When stomata occur equally on the two surfaces the amounts of CO_2 exhaled are equal on the two surfaces, and so on.

(2) Similarly with assimilation, no CO_2 is absorbed by an astomatiferous leaf surface, and when stomata occur on both surfaces, then the amounts absorbed follow the ratios of stomatic distribution.

(3) Boussingault's experiments on the assimilation of leaves with blocked stomata, which have hitherto formed the mainstay of the "cuticular" absorption theory, are completely vitiated by having been performed in super-optimal percentages of CO_2. Their interpretation is exactly the reverse of that usually accepted.

(4) The exhalation of CO_2 in bright light by a leafy shoot in Garreau's well-known experiment, is not the expression of any physiological truth for the leaf, but is only due to the presence of immature parts, or of tissues not sufficiently green, or not fully illuminated. Mature isolated green leaves fully illuminated assimilate the whole of their respiratory CO_2, and allow none to escape from them.

December 13, 1894.—"The Influence of the Force of Gravity on the Circulation." By Prof. Leonard Hill, M.B.

The chief results of the research are contained in the following conclusions:—

(1) That the force of gravity must be regarded as a cardinal factor in dealing with the circulation of the blood.

(2) That the important duty of compensating for the simple hydrostatic effects of gravity in changes of position must be ascribed to the splanchnic vaso-motor mechanism.

(3) That the effects of changing the position afford a most delicate test of the condition of the vaso-motor mechanism.

(4) That the amount of compensation depends largely on individual differences.

(5) That the compensation is far more complete in upright animals such as the monkey, than in rabbits, cats, or dogs, and, therefore, is probably far more complete in man.

(6) That in some normal monkeys over-compensation for the hydrostatic effect occurs.

(7) That in the normal monkey and man gravity exerts but little disturbing influence, owing to the perfection of the compensatory mechanism.

(8) That when the power of compensation is damaged by paralysis of the splanchnic vaso-constrictors, induced by severe operative procedures or by injuries to the spinal cord, by asphyxia, or by some poison such as chloroform or curare, then the influence of gravity becomes of vital importance.

(9) That the feet-down position is of far greater moment than the feet-up position, because when the power of compensation is destroyed the blood drains into the abdominal veins, the heart empties, and the cerebral circulation ceases.

(10) That, generally speaking, the feet-up position occasions no ill consequence.

(11) That the horizontal and feet-up positions at once abolish the syncope induced by the feet-down position by causing the force of gravity to act in the same sense as the heart, and thus the cerebral circulation is renewed.

(12) That firmly bandaging the abdomen has the same effect. While the heart remains normal, and so long as the mechanical pressure is applied to the abdominal veins, the blood pressure cannot possibly fall.

(13) That if the heart is affected, as by chloroform or curare poisoning, the restoration of pressure is incomplete, and it is possible that the heart may be stopped altogether by the inrush of a large quantity of blood, caused by too rapid an application of pressure on the abdomen. More work would be thrown upon the heart than, in its impoverished condition, it could perform.

(14) That vagus inhibition and cardiac acceleration are subsidiary compensatory mechanisms in the feet-up and feet-down positions respectively.

(15) That chloroform rapidly paralyses the compensatory vaso-motor mechanism, and damages the heart.

(16) That ether, on the other hand, only paralyses the compensatory vaso-motor mechanism very slowly and when pushed in enormous amounts.

(17) That the vaso-motor paralysis induced by these anæsthetics lasts for some considerable time after the removal of the anæsthetics.

(18) That chloroform can, by destroying the compensation for gravity, kill the animal, if it be placed with the abdomen on a lower level than the heart.

(19) That elevation or compression of the abdomen immediately compensates for the vaso-motor paralysis produced by chloroform.

(20) That compression or elevation of the abdomen, coupled with artificial respiration and with squeezing of the heart through the thoracic walls, is the best means of restoring an

animal from the condition of chloroform collapse. That these results agree entirely with McWilliams', and are opposed to those of the Hyderabad Commission.

(21) That the feet-down position inhibits respiration, and the feet-up position accelerates it.

(22) That these respiratory results probably depend upon the stimulation of sensory nerve endings by changes of tension brought about by the alterations of position, because the results are abolished by dividing the vagi.

(23) That in the feet-down position the respiration is thoracic in type, and the abdomen is retracted; in the feet-up position the respiration is diaphragmatic and the abdomen freely expanded.

(24) That these types of respiration tend to compensate for the effects of gravity on the circulation, for the retraction of the abdomen in the feet-down position mechanically supports the abdominal veins, whilst the thoracic inspirations aspirate blood into the heart. In the feet-up position the full and free expansion of the abdomen withdraws all obstacles to the compensatory dilatation of the abdominal veins.

In the last part of the paper the medical aspects of this research are discussed. It is suggested that emotional syncope is due to paralysis of the splanchnic area, and a case is quoted where compression of the abdomen immediately removed the syncopal condition. The same treatment, or that of elevation of the abdomen, is suggested for conditions of shock, chloroform collapse, and after severe hæmorrhage.

Finally, a parallel is drawn between some of the results of this research in reference to monkeys and those obtained by Dr. George Oliver on man, by measuring the diameter of the radial artery with his ingenious instrument, the arteriometer.

Physical Society, December 14, 1894.—Prof. W. E. Ayrton and Mr. H. C. Haycraft communicated a paper on a students' simple apparatus for determining the mechanical equivalent of heat. Mr. Haycraft, who read the paper, explained that the object at which the authors had aimed was the construction of an apparatus which could be placed in the hands of junior students, and by means of which a result correct within one per cent. could be obtained, without the introduction of troublesome corrections. The method employed is the electrical one, and the measurements to be made are (1) the value of the constant current passed through the resistance, as given by a direct-reading ammeter; (2) the average value of the P.D. between the terminals of the resistance, as given by a direct-reading voltmeter; (3) the mass of water heated *plus* the water-equivalent of the containing vessel, resistance-coil, and stirrer; (4) the rise of temperature of the water; (5) the time during which the current is passed. Of these the measurements (1) (2) (3) can be effected without the introduction of an error anything like as great as one per cent. The case of (4) and (5) is different. The rise of temperature, to be measured with accuracy, should be fairly considerable, and the same remark applies to the time of heating as measured by an ordinary stop-watch. At the same time, if these two quantities are made unduly great, there will be too great a ratio of heat lost to heat generated during the experiment. The authors consider that, with a given amount of electrical power available, the best conditions will be obtained by making the percentage accuracy of the temperature measurement, the percentage accuracy of the time measurement, and the percentage of generated heat lost by surface cooling equal. Hence they determine the mass of water to be used and the time of heating which may be expected to give the best results. The immersed conductor is a strip of manganin about 0·25 inch wide, 0·03 inch thick, and 5 feet long, which is bent into a series of zig-zags, so as to form a kind of circular gridiron, the successive portions of strip lying all in one plane, and the whole being held rigid by a strip of vulcanised fibre, to which each portion of the strip is screwed. Another precisely similar grid is placed 3 inches below the first, and the two are joined in series, and are mechanically connected together by thin vulcanite pillars. The water is contained in a glass beaker of just sufficient diameter to take the framework of manganin strip. This latter exposes a considerable surface (about 400 sq. cm.) to the water, and is moved bodily up and down during the experiment, thus constituting an efficient stirrer. To allow sufficient freedom of movement, electrical connection is made by means of very flexible leads, each made up of about 210 thin copper wires. The results obtained by students for the heat equivalent of the watt-second have an average deviation from the mean, if several experiments are made, of less than one-half per cent.; and they agree with the best standard determinations within one per cent. Mr. Griffiths thought it inadvisable to provide junior students with apparatus from which every source of error had been eliminated; they were thus led to underrate the difficulty of an experiment, and the care required to obtain reasonable accuracy. Prof. Carey Foster agreed, generally, with Mr. Griffiths, and deprecated the use of direct-reading ammeters and voltmeters in experiments of this kind. He thought it preferable that a student should learn to reduce instrumental readings to absolute measure for himself. Prof. S. P. Thompson dissented from the opinions expressed by the two previous speakers, and thought it was an advantage to students to have the use of direct-reading instruments. Dr. Sumpner described a simple method which he had employed for measuring the mechanical equivalent of heat, and which depended on the heating of a stream of water, as it flowed through a pipe containing the current-conductor. Prof. Rücker was inclined to take an intermediate view of the questions that had been raised. He thought that students should take for granted as little as possible concerning their instruments; but to verify every point, even if practicable, would occupy a great deal of time which might otherwise be more profitably employed. Prof. Ayrton replied, and explained that the calibration of ammeters and voltmeters would be part of the work of a student at another part of his course.—A paper by Prof. Ayrton and Mr. E. A. Medley, entitled "Tests of glow-lamps, and description of the measuring instruments employed," was commenced by Mr. Medley, the latter part of the paper being held over till next meeting. The object of the investigation was to find at what E.M.F. glow-lamps could be most economically run. Too low an E.M.F. gives a low efficiency, and too high an E.M.F. renders the lamps short-lived; so that there must be (for a given lamp) a certain E.M.F. which is more economical to work at than any other. It was also pointed out that, as glow-lamps deteriorate and become less efficient with use, it may be an economy to discard a lamp before the filament actually breaks. The lamp is then said to have reached the "smashing point." Accumulators were used to drive the lamps, automatic apparatus being used to keep the E.M.F. constant, and when a lamp-filament broke, the fact was automatically recorded on a tell-tale.

Geological Society, December 19, 1894.—Dr. Henry Woodward, F.R.S., President, in the chair.—The Lower Greensand above the Atherfield Clay of East Surrey, by Thomas Leighton. This paper embodies the results of the author's examination of the Lower Greensand of East Surrey during the three years 1892–94. The area discussed in the paper extends from Leith Hill in the west to Tilburstow Hill in the east; and the divisions of the Lower Greensand chiefly referred to are those hitherto known as the Bargate, Sandgate, and Hythe beds. The author stated that the Lower Greensand of East Surrey shows that formation to consist of beds deposited in a marine estuary or narrow sea, not far from land and within the influence of strong currents, extending generally from N.W. to S.E., so that, without palæontological evidence, no correlation of beds here with those exposed at Sandgate and at Hythe is possible. He arrived at this conclusion by following the outcrop of the various chert-beds, which, after Dr. G. J. Hinde (*Phil. Trans. Roy. Soc.* vol. clxxvi. 1885), are accepted as of sponge origin (deep-water deposits), and further by following the outcrop of the pebble-beds, described by Mr. C. J. A. Meyer (*Geol. Mag.* for 1866, p. 15).—On the eastern limits of the Yorkshire and Derbyshire or Midland coalfield, by W. S. Gresley. The author attempted to throw light on the question of the easterly extension of the Yorkshire, Derbyshire, and Nottinghamshire coalfield beneath the newer rocks. He noticed the general trend of the strata, the sizes of other British coalfields, the question of the origin of mountains, stratigraphical considerations, and the faults of the North of England.—On some phases of the structure and peculiarities of the iron ores of the Lake Superior region, by W. S. Gresley. The author has studied heaps of ore brought from the region lying south-west of Lake Superior since 1890. He described certain structural features of the ore-fragments, and discussed the evidences of mechanical movements and chemical alteration exhibited by these fragments.

Chemical Society, December 6, 1894.—Dr. Armstrong, President, in the chair.—The relative behaviour of chemically prepared and of atmospheric nitrogen in the liquid state, by James Dewar, F.R.S.—On the use of the globe in the study of crystallography, by J. Y. Buchanan, F.R.S.—A new method of obtaining dihydroxytartaric acid, and the use of this acid as a reagent for sodium, by H. J. H. Fenton.—Essential oil of hops, by Alfred C. Chapman.—Interaction of 1 : 2-diketones with primary amines of the general formula $R'.CH_2 NH_2$ (second notice), by Francis R. Japp, F.R.S., and W. B. Davidson.—The isomeric dinitrodiazoamidobenzenes and their melting-points, by R. Meldola, F.R.S., and F. W. Streatfeild.—On the yellow colouring matter of *Sophora japonica*, by Dr. Edward Schunck, F.R.S.

PARIS.

Academy of Sciences, Dec. 24, 1894.—M. Lœwy in the chair.—On two invariant numbers in the theory of algebraical surfaces, by M. Emile M. Picard.—Displacement of carbon by boron or silicon in fused cast-iron, by M. Henri Moissan. Carbon is displaced by boron or silicon in a fused iron carbide or cast-iron much in the same way that salts will displace each other from aqueous solution. A state of equilibrium is set up between the iron carbide on the one hand, and the iron boride or silicide on the other hand.—On the circulation of the lymph in the small lymphatic vessels, by M. L. Ranvier.—On the importance of hybridisation in connection with the re-establishment of vineyards, by M. A. Millardet.—The Secretary announced the death of P. François Denza, Director of the Vatican Observatory (died December 14).—The elements of the planet 1894 BE, by M. J. Coniel. Provisional elements for the planet BI, by M. Capon. The planet BE is the most favourably situated, among the known planets, for the determination of solar parallax. The planet BI is identical with (369) of the *Annuaire du Bureau des Longitudes*.—Observations of Encke's comet and of the planets BH and BI, made at Algiers Observatory, by MM. Rambaud and Sy.—Observations of Encke's comet, made at Lyons Observatory, by M. G. Le Cadet.—Observations of the sun, made at Lyons Observatory during the third quarter of 1894, by M. J. Guillaume.—On the problem of three bodies. M. F. Siacci calls attention to his paper on this subject dated January 12, 1874. He remarks that a paper of August 27 (*Comptes rendus*, 451), "Sur la transformation des équations canoniques du probléme des trois corps," is a reproduction of his 1874 paper. — Remarks on the matter of a priority claim made by M. O. Staude, by M. P. Staeckel.—On the solution of numerical equations by means of recurring series, by M. R. Perrin.—On a doctrinal point relative to the theory of multiple integrals, by M. Jules Andrade.—On the *abaques* for 16 and 18 variables, by M. A. Lafay.—On the electrostatic capacity of a line traversed by a current, by M. Vaschy. The capacity per unit of length of a cable traversed by a permanent current has the same sense as in electrostatics. With rapidly varying currents, it cannot be supposed that the electric field admits of a potential, hence the notion of a definite capacity disappears.—Electric potentials in a liquid conductor in uniform movement, by M. G. Gouré de Villemontée. At speeds of 33 to 323 mm. per second, the uniform movement of a liquid conductor traversing wide glass tubes of uniform section, does not produce any appreciable difference of potential between two points in the liquid.—Experimental researches on radiation at low temperatures, by M. Raoul Pictet. This is an abstract containing a series of curves showing the variation of radiation with time, and a discussion of these curves.—Contribution to the study of atmospheric ozone, by M. J. Peyron.—On the metallic sulphides, by M. A. Villiers.—Combinations of hexamethylene-amine with silver nitrate, chloride, and carbonate, by M. Delépine.—On the cyano-ethers, by M. Albert Colson.—On the chromates of iron, by M. Charles Lepierre. The author has obtained thirteen chromates, of which two only were known before. Whether prepared from ferrous or ferric salts, all contain ferric iron. All are coloured. They form a series parallel to the basic sulphates of iron.—A new reagent allowing the demonstration of the presence of hydrogen dioxide in green plants, by M. A. Bach. A solution as made containing 0·03 gram of potassium bichromate and five drops of aniline per litre. 5 c.c. of this solution give a violet colouration with two drops of a 5 per cent. solution of oxalic acid only after 36 hours. The colouration is given in the presence of hydroxyl in from 10 to 30 *minutes*. This reagent allows the detection of one part of hydroxyl in 1,400,000. The method of testing green plants is given in detail, and a list of a number of plants showing the reaction is added.—On the valency of glucinum (beryllium) and the formula of glucina, by M. Alph. Combes. β diketones, particularly acetylacetone, form metallic derivatives but yield no acid or basic salts. These derivatives are generally well adapted for determining their molecular weights, and hence the valency of the contained metallic radical. By means of a determination of the vapour density of glucinum acetylacetonate, it is found that glucinum is divalent, and its oxide must be written Be O.—On the constitution of the aromatic sulphones, by MM. L. Zorn and H. Brunel.—On the cephalic lobe of the Euphrosinæ, by M. Émile G. Racovitza.—On the development of the kidney and the general cavity in the Cirripedes, by M. A. Gruvel.—On the functional differences between normal and enervated muscle, by M. N. Wedensky.—On the biological relations between *Cladochytrium viticolum*, A. Prunet, and the vine, by M. A. Prunet.—On a detailed botanical chart of France, by M. Ch. Flahault.—On a peculiar method of dehiscence of the pollen of the fossil Dolerophyllum, by M. B. Renault.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

BOOKS.—Woman's Share in Primitive Culture: Dr. O. T. Mason (Macmillan).—Conspectus Floræ Africæ: T. Durand and H. Schinz, Vol. v.: (Dulau).—Standard Methods in Physics and Electricity Criticised, and a Test for Electric Meters proposed: H A. Naber (Tucker).—Webster's Practical Forestry; A. D. Webster, 2nd edition (Rider).—Pithecanthropus Erectus eine Menschenaehnliche Uebergangsform aus Java E. Dubois (Batavia, Landesdruckerei).—A Dictionary of Birds: A. Newton and H. Gadow, Part 3 (Black).—American Spiders and their Spinning Work: Dr. H. C. McCook, Vol. 3 (Philadelphia Academy of Natural Sciences).—Zoological Record, 1893, edited by D. Sharp (Gurney).

PAMPHLETS.—Om Gula Febernš Spridningssätt: Dr. E. Åberg (Stockholm).—Sur la Transmission de la Fièvre Jaune: Dr. E. Aberg (Stockholm).—Sulle onde Elettromagnetische, &c.: Prof A. Righi (Bologna).—Versuch einer Theorie der Eletrischen und Optischen Erscheinungen in Bewegten Köpern: Prof. H. A. Lorentz (Leiden Brill).—Die Form des Himmelsgewölbes: W. Filehne (Bonn).—Blackie's First Stage Mathematics, Euclid and Algebra (Blackie).

SERIALS.—English Illustrated Magazine, January (198 Strand).—Good Words, January (Isbister).—Sunday Magazine, January (Isbister)—Journal of the Chemical Society, December (Gurney).—Zeitschrift für Wissenschaftliche Zoologie, lviii Band, 4 Heft (Williams).—Economic Journal, December (Macmillan).—Mathematical Gazette, No. 3 (Macmillan). Chambers's Journal, January (Chambers).—Longman's Magazine, January (Longmans).

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