THURSDAY, JANUARY 4, 1894.

RECENT CONTRIBUTIONS TO METEOROLOGY.

Report on the Present State of our Knowledge respecting the General Circulation of the Atmosphere. By L. Teisserenc de Bort. (London: Stanford, 1893.)

On Hail. By the Hon. Rollo Russell, F.R.Met.Soc. (London: Stanford, 1893.)

Weather Lore: a Collection of Proverbs, Sayings, and Rules concerning the Weather. Compiled and arranged by Richard Inwards, F.R.A.S. (London: Elliot Stock, 1893.)

WHILE meteorologists are generally prepared to admit the salient points of the theory of the atmospheric motions as outlined by Ferrel, there are to be met discussions by various authors, accentuating not only differences in the details of the scheme, but also defects in the theory on which the general circulation of the atmosphere is based. The latest contribution to the literature of the subject illustrating these points, is from M. Teisserenc de Bort, Meteorologist to the Central Bureau, and General Secretary of the Meteorological Society of France; for this authority cannot accept, in its entirety at least, either Ferrel's deductions or his method of conducting the inquiry. Ferrel, it is well known, having deduced the equations for the horizontal motion of the atmosphere, relative to the earth's surface, applied, with effect, the condition of continuity and the law of conservation of areas, or, what would possibly be a better term, the preservation of the moment of rotation, and demonstrated the existence of an easterly motion of the atmosphere in the higher latitudes, and a westerly motion in the lower. To define the limits of these zones, Ferrel remarked that the sum of the moments with reference to the axis of the earth, of the air forming the easterly winds, ought to be equal to that of the westerly winds, and that this condition was fulfilled on a hemisphere, if the easterly winds prevailed up to 30° latitude, and westerly winds to the pole. This line of argument receives some support from the suggestion, that otherwise there would be a residual unexpended force, tending to change the velocity of the earth's rotation. But M. de Bort replies, with some force, that this argument is inadequate, because there is no evidence that the earth's rotation is uniform; and, indeed, the action of the tides and the diurnal variation of the barometric pressure, point, pretty conclusively, in an opposite direction. If the effects of friction are omitted, the author seems prepared to admit the validity of Ferrel's argument, and it would be very unjust to deny that Ferrel neglected friction altogether, or failed to modify his original result, obtained without friction. Further, Oberbeck especially has considered the effects of friction, and he has assigned a lower limit to the zone of change of direction not greatly different from Ferrel's value. Apart, however, from this point of theory, the author differs from Ferrel as to the cause of the belts of maximum pressure, north and south of the equator, and adds an explanation of the low pressure zone in latitude 55° and of the polar maximum.

But the most interesting, and possibly the most valuable portion of the paper, is the insistance on the connection traced between temperature and distribution of pressure, and the effort to explain the observed variation of pressure along parallels of latitude by the presence of thermic anomalies. The author sees in the variation of temperature over continents and seas in the same latitude, and the consequent changes in the density of the lower strata of the atmosphere, the origin of many of the irregularities that mark the isobaric curves, and a cause not inferior in its effects to the rotation of the earth in establishing the prominent features of the general circulation.

M. de Bort has also made an ingenious attempt to compute from theory the mean isobars of January and July, and to compare the results with actual observations. This is a step in advance, but the measure of success that has attended the effort must be left to the decision of individual judgment. Two approximations, or two distinct attempts, have been made. In the first, it has been assumed that at an altitude of 16,000 feet the irregularities in the distribution of the isobars disappear, and only the influence of latitude remains. Consequently the observed barometric pressure at the surface should be given by adding to the mean pressure, corresponding to the latitude, the weight of the column of air of variable density extending from the surface to this altitude. When this operation is effected, a comparison with the observed quantity discloses the fact that the computed pressures are too great, and further shows a tendency to exaggerate the barometric minimum over the North Atlantic, while it exhibits a maximum of pressure over North America which does not really exist. There is therefore, admittedly, a more marked difference in the computed isobars over continents and seas than is actually observed. Two causes are assigned for the failure to reproduce actual facts, both of which are probably operative. The one is that the density of the column of air does not diminish uniformly with the temperature, which hypothesis, for the purpose of computation, it is necessary to assume. The other is that probably the slope of the surfaces of equal pressure from the equator towards the poles is greater where the temperature is already low, than where the temperatures are high, in the upper regions of the atmosphere.

The second attempt to reproduce the observed pressure is arranged to take into account the influence of the unequal distribution of temperature upon the form of the upper isobars, and it is contended that the computed values of the surface pressure "show close analogy with those representing the isobars deduced from direct observation."

Not content with surveying the conditions of our atmospheric circulation, the author proceeds to discuss those that obtain on the planets, and submits two ideal pictures of the earth with its surrounding cloud as seen from space. These are compared with a photograph of Jupiter, but we strongly doubt whether the author gleans any additional facts in support of his views. The red spot is a conspicuous feature in this photograph, and whatever may be the true explanation of that phenomenon, the tolerable permanence of its character forbids us to ascribe it to atmospheric circumstances. But M. de Bort is disposed to regard the dark markings, in which the red spot would be included, probably, as the real surface of Jupiter, seen through an unobscured atmosphere, and the position of the belts on Jupiter is thought to support the suggestions of the author. But we doubt whether astronomers are agreed that the dark markings represent clear sky, and the lighter portions cloudy vapour. Mars would seem to be the one planet in which we might expect to find atmospheric conditions similar to those here prevalent; but we are told that there are "probabilities based upon scientific reasons, that the clouds upon Mars are not distributed in the same manner as upon the earth." Though when we consider what a presumably comparatively unimportant factor the solar heat is upon Jupiter, and that, moreover, the axis of rotation is nearly perpendicular to the plane of the orbit, on a superficial view, this observation seems to be more applicable to Jupiter than to Mars. From the remark with which the paper closes, we gather that the author intends to prosecute this subject of investigation on the planets. We wish him success.

Of the second work mentioned at the head of this notice, it is rather difficult to speak. Although the author has not sketched the plan and scope of the work in any introductory chapter, it is easy to understand the principle that has guided him in the construction of the book. He has evidently been at great pains to bring together all that is valuable, or that he thought valuable, in the descriptions that have been given of hailstorms in the past, not only in the accompaniments of the hailstorms, or of the characteristics of the hailstones themselves, but also of the theories that various authorities have suggested to explain their occurrence. When we consider that in the case of nearly every hailstorm, some one is found to describe it, it is evident that the materials from which Mr. Russell can draw his information are very widely scattered. The list of authors quoted is a long one, and could no doubt have been made much longer, did not the reiteration of the same facts become wearisome. Having collected his information, the author has attempted to digest it, and has given us a summary of the characteristics of hailstones with a graphic description of the development of a hailstorm. One consequence of this method of dealing with the subject is that about three-fourths of the book consist of extracts from various authors, and only the remainder is original matter. This class of work, if not very brilliant, is, no doubt, valuable; and inasmuch as most of the extracts are given in the words of the author, with distinct references to the sources from which they are taken, this book may save much searching of original authorities, and a proportionate saving of time. Whether the materials are arranged in the most advantageous manner, is a question about which some doubt may be entertained. It would seem sometimes as though the extracts had been printed in the order in which they had been encountered, without any attempt at arrangement at all. To take the first chapter, "descriptions of hailstorms and hailstones," at first sight it would look as though some chronological order was to be maintained, for we begin in 1680, and pass next to the early years of this century; but when we get into the middle of the century, we flounder about from 1890 to 1870, and back again, without any guide. Neither is locality any rule, for we are taken all over the world, without method or system. Nor is it easy to trace any gradual scientific progress in the descriptions. We have simply more or less complete descriptions of some fifty hailstorms, or of the salient features that distinguished them.

The second chapter gives us observations of temperature, clouds, and winds at great altitudes, principally confined to the accounts of balloon ascents. In this chapter, which is very short, there might have been found room to discuss in more detail the observations made at some of the meteorological stations at considerable altitudes. The results obtained at Pike's Peak, Colorado, would seem to be of the highest importance in this connection; but the author prefers to drop this topic, though apparently germane to his subject, in order to discuss, or rather to collect, the opinions and observations of those meteorologists who have noticed the connection of electricity with the occurrence and formation of hail.

The chapter on theories of hail is interesting. In it is given the opinions of most of those whose opinions are worth recording, but in the popular and not the scientific language which some of the authorities quoted would have used. Von Bezold especially suffers from inadequate description, and, if we are not mistaken, Hertz's name is not mentioned. It would seem almost as though the author were not acquainted with much of the hydrodynamical analysis that has been applied to the atmosphere, or being acquainted with it, disapproves of its application to the present inquiry.

In the chapter on the development of a hailstorm, objection will probably be taken to the insistance and stress that is laid upon the part played in the mixture of air of different temperatures, as a primary cause in producing precipitation, whether it be of hail or any other form of moisture. The numerical example worked out to illustrate the author's point is not very clearly expressed; and even granting the figures of the author, he is obliged to fortify his case by a continual mixture. But the continual mixture would tend to produce uniformity of temperature, and disturb the accuracy of the original calculations. Undoubtedly we have present, in what it is usual to call the hail stadium, an amount of dry air which it is convenient to separate, in theory at least, from the saturated vapour also present, the drops of water, and the particles of ice or snow which probably constitute the germ of the large hailstones, and then, if the conditions are favourable, we get hail; and it is difficult to see that our author has carried the explanation much further. Nor possibly does the application of the mechanical theory of heat, however legitimate its methods may be, advance our knowledge very materially, at least in a practical direction. The local, and often confined, area over which hailstorms occur, is a marked feature of their occurrence, and is likely, for a long time to come, to baffle the applications of a general theory, and prevent any sufficient precautions being taken against the damage they produce, which it may be supposed is the practical outcome that sufferers hope to derive from the studies and inquiries of meteorological observers.

In the final chapter, headed "Conclusions," there is an attempt to gather up the results of the observations recorded in the previous chapters. It is a pretty fair record of our general knowledge of the subject, exhibiting a tolerably complete grip of all the circumstances attending these phenomena. It does not show much originality, perhaps, but it does show very extensive reading, accurate observation, and power of condensation.

The third book mentioned is scarcely of the kind that compels one to sit down and read off-hand. It is precisely what it professes to be—a collection of the many weather proverbs which possibly the wit of one, rather than the wisdom of many, has perpetuated. If these adages did contain the results of long-sustained and well-directed observation of the habits of birds, animals, and insects, they would possess a distinct value, though it is difficult to see how the information so gleaned could indicate the severity or the mildness of the coming season; but it is to be feared they too frequently record the opinion of one who is capable of a jingling rhyme, or of one whom his comrades consider to be wise in such matters.

There are also quotations from the poets, ancient as well as modern, and all bearing on the subject of weather prediction. The hope of the author is that the perusal of such a collection may induce students to take more intelligent notice of meteorological conditions, and to avail themselves of accurate instrumental means, rather than to rely upon hackneyed quotations. A somewhat similar collection of "wise saws" was published by the United States Signal Service, but we fail to see any reference to this work in Mr. Inwards's introduction. A study of these sayings would probably furnish some additional quotations, and as the compiler aims at greater completeness in the next edition, we would refer him to this source. The book is well printed and admirably "got up," and will no doubt be welcome to many interested in folk and weather lore.

PHYSICO-CHEMICAL MEASUREMENTS.

Hand- und Hilfsbuch zür Ausführung physiko-chemischer Messungen. Von W. Ostwald. (Leipzig: W. Engelmann, 1893.)

THIS manual must be regarded as the only guide to measurements in physical chemistry which has yet been published. The book is not intended to completely cover this field of investigation, but has evidently been devised with the primary object of assisting Prof. Ostwald in his course of instruction at Leipzig. It is not an introduction to the subject, as the detail supplied, both in connection with apparatus and methods, is insufficient for the requirements of the beginner; nor is it a treatise wherein a representative collection of methods may be consulted. The book is rather to be viewed as an aid to the teacher, or as indicating to the chemist or the physicist methods which for the most part the author has found to be of service in his own laboratory.

The information contained in the opening portion of the volume is of the kind usually met with in a physical text-book: modes of calculating results, the influence of errors, the use of corrections, the measurement of length, the balance and weighing, and the measurement and regulation of temperature. Succeeding chapters take up he more common operations in glass-working, the measurement of pressure, the measurement of the volume and density of solids and liquids, and the ordinary methods of measuring vapour density. Here it may be noted that Perkin's modification of Sprengel's pyknometer, which is perhaps the most useful of all the various patterns, is not included among those described. Kopp's pyknometer also is rendered more serviceable if a short mm. scale instead of a single mark be etched on the neck.

The thermal properties of liquids are next briefly considered. Modes of determining expansion and molecular volume at the boiling-point are given with a moderate amount of detail. The determination of the boiling-point itself is, however, described in the most meagre way. Of the various methods of measuring vapour pressure the dynamical process introduced by Ramsay and Young alone finds a place. Critical temperature and critical pressure are determined in separate pieces of apparatus in the manner recently described by Altschul. No general method is indicated whereby the relation between pressure and volume may be determined under varying conditions of temperature, and no practical method can thus be given for estimating critical volume, although the principle of the new method due to Mathias is mentioned.

Calorimetry is now dealt with, and short accounts are given of the simpler methods of estimating specific heat and the thermal changes accompanying vaporisation, dissolution, combustion, and reactions in dilute solution.

Descriptions of optical measurements relating to refractive indices, spectroscopy including spectrum photometry, calorimetry, and rotatory polarisation are now introduced, and are followed by a chapter on viscosity and surface tension. In connection with viscosity, the apparatus represented is only adapted for obtaining relative values, and is quite unsuited for investigating the effect of temperature. What appears to be the correct value of the kinetic energy correction used in calculating viscosity coefficients is ascribed to Finkener and Wilberforce, whereas the first published account of the mode of deducing it is due to Couette. None of the methods given for measuring surface tension are free from the objection that air is in contact with the liquid surface.

The remaining chapters are devoted to measurements on solutions. Methods of estimating the solubility of solids, liquids, and gases, and of determining molecular weights from the freezing-points and boiling-points of solutions are given at considerable length. At still greater length, and thus in marked contrast with the treatment elsewhere, electrical measurements are next set out. Here are found accounts of the methods of measuring electromotive force and conductivity, dissociation constants, the basicity of acids, &c. The last chapter takes up elementary problems in chemical dynamics relating to the velocity of chemical change, the catalysis of methyl acetate and the inversion of cane-sugar by dilute acids being given as examples.

From what has been said it is evident that the operations dealt with in the book are only such as are frequently performed, or which at the present time are considered to be of importance. Some of these even are occasionally omitted; no mention is made, for example, of the ordinary methods of obtaining melting-points.

It is noteworthy also that processes relating to the purification of substances for physical study are not touched upon. Accounts of the best systems of fractionation, either by distillation or crystallisation, or of distillation under reduced pressure, &c., have, it seems to us, a better right to a place in a book of this kind than, say, the chapter on glass-blowing. Again, no particular notice is taken of methods which have to be used when only a small quantity of material is available. It frequently happens that a substance can only be obtained sufficiently pure in but small quantity, and if methods of obtaining boiling-point, density, refractive index, &c. in such cases were more widely known, physical constants would no doubt be more generally estimated in the course of ordinary chemical investigations.

It is needless to state that the book is full of useful hints both on methods and apparatus, and will be indispensable to those for whom it is specially designed. It is also worthy of special recognition as being yet another effort on the part of Prof. Ostwald to place physical chemistry on a level with other departments of experimental investigation.

J. W. RODGER.

OUR BOOK SHELF.

Handbook of British Hepaticæ. By M. C. Cooke, M.A., LL.D. 1 vol. 8vo. 310 pp. 7 plates. 200 woodcuts. (London: W. H. Allen and Co., 1894.)

PROBABLY no group in the British flora has received so little attention as the Hepaticæ. This is due partly to the ordinary botanical text-books describing merely the life history of the ubiquitous *Marchantia polymorpha*, and ignoring or passing over with but scanty reference the foliaceous group. But chiefly is it due to the want of a handbook by which beginners could identify their plants and obtain references to the literature of the subject. Sir W. J. Hooker's magnificent monograph, which appeared in 1816, contained plates with copious descriptions of all the British species then known; but it is now scarce, costly, and having all the species described under one generic name, *Jungermannia*, it becomes necessary, after identifying a plant by it, to refer to some other source to ascertain the now accepted name. Hooker's "English Flora," vol. v., in dealing with the same group, divides the frondose group into several genera, but retains the generic name of *Jungermannia* for the whole of the foliaceous group.

In 1865 Dr. M. C. Cooke published, as a supplement to *Science Gossip*, a catalogue with outline figures of all the British species. This is now out of print. Since then notes scattered through various journals have formed the whole of the British literature upon the subject, except the commencement of a monograph by the late Dr. B. Carrington.

Dr. M. C. Cooke has now filled up the gap by producing a "Handbook of the British Hepaticæ," containing full descriptions of all the species, about two hundred in number, known to inhabit the British Islands. The volume opens with an introduction of 20 pp., describing the position, structure, reproduction, and subdivisions of the group. This is followed by a detailed account of the species, each arranged upon the same plan. First come the diagnostic characters, followed by copious synonymy, then the habitat, and finally a full description. Each species is also represented by an outline figure, either in the text or in one of the seven plates at the end of the volume. A bibliography and index complete the work. The size and clearness of the type will be appreciated by those who use the book, as it should be, in conjunction with microscopical examination of specimens. Altogether a very useful work has been produced, which ought to fill a gap already too long vacant.

C. H. W.

The Royal Natural History. Edited by Richard Lydekker. Parts 1 and 2. (London: Frederick Warne and Co., 1893.)

YET another "Natural History." There is certainly a demand for such, and without doubt there is a supply. The work is to be in six volumes, and the parts, published monthly, will complete the series in three years. The paper and typography leave nothing to be desired. The illustrations are in almost every instance, so far as our knowledge of the published parts goes, excellent; many of them are as artistic as they are accurate; and when we add that the editor of the series is an able and well-known zoologist, there can be no doubt but that the reader or purchaser will get full value for their expenditure of time or money.

In noticing a work of this nature, when the facts are as above stated, there is but little room for criticism, and despite the shock which the first blazing sound of its advent conveyed to our senses, despite the fact that "it is not compiled or translated from foreign sources," and that "the co-operation of the Bibliographic Institutes of Leipsic and Vienna" has been secured so as to obtain "all that is best and newest among the productions of the greatest natural history publishers of Europe," we yet most heartily recommend the work to all our readers, and we anticipate that most of those who take any interest in zoology will place it on their book shelves.

Of the six volumes, as was to be expected in a work of this kind, the larger number (five) is to be devoted to the backboned animals, and but one to the boneless crew; and of the first five volumes, two and a half will relate to the mammals, one and a half to the birds, and but one to the reptiles, amphibians, and fish. It is not at all a fair division, but then the mammals are thought to be the most generally interesting class, and we are promised a lot of information about "the larger game." The first two parts are devoted to the monkeys, and we have an account of nearly all the known species, accompanied with an immense number of illustrations. One suggestion occurred to us while reading over the account of the habits of the baboons; that when plants are referred to they should, when their scientific names are used, be quoted specifically as well as generically; thus a "very remarkable kind of West African plant" is mentioned as the "welwitschia," but the editor would never think of quoting the Anubis baboon as the "cynocephalus." We hope it will be a long time before *Welwitschia mirabilis* will be exterminated by the baboons. From a natural history stand-point there is really no such plant as an "*ixia*," but there are several species of the genus Ixia, upon the bulbous stems of which it would appear these baboons feed.

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 Origin of Lake Basins.

I WELCOME the criticism of my article on the glacial origin of a certain class of lakes by an experienced geologist like Mr. Oldham, because it probably embodies the strongest argument that can be adduced on the other side—at all events as regards the one aspect of the problem which he alone touches upon. He urges that my paper contains a fallacy and a misrepresentation. The alleged fallacy is, that because the lakes in question are found in glaciated and not in otherwise similar non-glaciated regions, "therefore the rock-basins in which the lakes lie were excavated by glaciers." But this is not my argument, and therefore not my fallacy, What I say is—"there must be *some* causal connection between glaciation and these special types of lakes. What the connection is we shall enquire later on." That there is a "causal connection" Mr. Oldham asserts as strongly as I do myself, though his is a different, and as I have endeavoured to show, an untenable one.

This brings us to the alleged misrepresentation, which is, that I have imputed to the opponents of the ice-erosion theory the view that the earth movements which, as they allege, produced the lakes, occurred in the period just before the ice-age came on. Mr. Oldham says, this is an unreasonable and unfounded limitation, since the movements in question probably occurred throughout the glacial period itself. I quite admit the validity of this criticism, and that I should have added, "or during the glacial period itself," to, "immediately before" it. I certainly had this probability in my mind, and the reason I did not express it was twofold. In the first place, all the advocates of the earth-movement theory appeared to assume, either directly or implicitly, the preglacial origin of the lakes; and secondly, this assumption gave them the strongest argument against my views, and I therefore gave them the benefit of it. Mr. Oldham appears to have overlooked this. Yet it is clear that the shorter you make the time since the formation of lake basins by earth-movements the more difficulty there is in explaining the total absence of valley-lakes from all the non-glaciated mountain regions of the world, since there is less time for them to have been all silted up. When arguing this point I said—in the passage evidently referred to by Mr. Oldham—"The only way to get over the difficulty is to suppose that earth-movements of this nature occurred only at that one period, just before the ice-age came on, and the lakes produced by them in all other regions have since been filled up." I thus gave my opponents the benefit of an extreme supposition which was all against myself; while the more reasonable view, that earth-movements are just as likely to have occurred during and since the glacial epoch as before it, renders my argument from the geographical distribution of lakes much stronger, since it is impossible to believe that, if lake basins as large and as deep as those of Geneva, Maggiore, Como, Constance, and Garda, were formed in non-glaciated regions as recently as the middle or latter part of the glacial epoch, a considerable number of them would not be still in existence.

Of course, if it can be shown that filled up lake-basins exist in tropical and subtropical regions, corresponding in number, position, size, and depth, with those of glaciated areas, the argument from geographical distribution will break down. At present I am not aware of any evidence that such is the case. But even if it were so, there remains the singular correlation between the size and depth of lake basins and the known size of the glaciers that occupied their valleys; together with the surface and bottom contours of the lakes themselves, so strongly opposed to their production by any form of valley-subsidence or earth-movements.

A friend has pointed out an unsound argument in my article on "The Erosion of Lake-basins" in the *Fortnightly*, and I therefore ask to be allowed to state what it is, and thus avoid its being possibly made the subject of discussion in the pages of NATURE. As a proof of the very great erosive power of ice I have adduced Dr. Helland's estimate of the quantity of Scandinavian *débris* in Northern Europe. But it is evident that this only proves the great carrying power of the ice, since the rock and gravel would be mostly of sub-aerial origin. It, however, indicates a very long period during which the ice-sheet was at work, while the clayey element in it would be due to erosion. The larger part of this, however, would certainly have been carried away into the North Sea during the passage of the ice-sheet across the Baltic. The enormous quantity of boulder-clay in North America, which I have also referred to, is a better indication of the extent of true ice-erosion. ALFRED R. WALLACE.

THE question you have allowed me to raise is too important and far reaching to justify its dissipation upon personal issues. It cannot be thought unreasonable that those geologists who propound transcendental theories should justify the mechanical postulates on which they claim to base them. This is all I have asked.

Dr. Wallace asks me to explain what will happen when sufficient pressure is applied to ice not only to crush it, but to induce regelation. I have already explained in my work, that the notion of fracture and regelation taking place in glaciers is at issue with the details of their differential motion as tested by experiment. There is no evidence that ice which on pressure being applied to it has ample room to move, will undergo regelation at all. The pressure when crushing ensues will be dissipated in the direction of least resistance, and most probably upwards. This emphasises Mr. Deeley's statement, and he wrote as a champion of Dr. Wallace, that "fracture and regelation have little to do with the question."

Dr. Wallace then returns to his charge against me that I have in some way committed myself in my work to a position inconsistent with the one I am now maintaining. I can assure him that if he has read this meaning into my words, it was not what they were meant to convey. In giving the history of the "Glacial Nightmare," I entered largely into the views of Charpentier, and in so far as he championed glaciers as against ice sheets I agree with him. I have said that his views "*are for the most part sound and unanswerable*, since *they finally established for the Alpine country and for Switzerland the fact that glaciers were formerly much more extensive*," &c. Beyond this I could not go, since my work was written to prove the unscientific character of the extravagant conclusions of the later glacialists, including Charpentier himself after he became a follower of Agassiz. Apart from this, however, what your readers I am sure would welcome would be an *argumentum ad rem*, and not one *ad hominem*.

In demanding that the advocates of the glacial theory in its extravagant form should justify their premises and postulates, I must not be understood to decline to meet the geological case against the glacial excavation of lobes. I have met it at great length already in my recent work, but not so ably and not so thoroughly as Mr. Spencer met it in his elaborate and crushing examination of the critical case of the North American lakes, which I commend most heartily to the study of enthusiastic champions of omnipotent ice.

The geological question, however, is necessarily contingent upon the mechanical question, and no amount of ingenuity will in the long run enable those who invoke ice as the author of all kinds of geological work to evade the duty of proving its capacity to do that work, and notably to explain how it can travel over hundreds of miles of level country, or suddenly begin to excavate deep and extensive lake basins after it has been moving gently over its own bed of soft materials for many miles, or, indeed, how it can excavate on level ground at all. The first step is to show that ice can convey thrust in a way to compass these ends; the second one is to show whence this thrust is to be derived. Your readers who are committed to no theories unsupported by facts, will not quarrel with the reasonable demand that these first steps should be surmounted before we advance any further. Those who like to traverse cloud-land on the wings of fancy may be otherwise satisfied. To them I would only say that the result cannot be science; it must remain nothing more than poetry.

HENRY H. HOWORTH.

30 Collingham Place, Earls Court, December 30, 1893.

Hindoo Dwarfs.

IN your issue of November 9, 1893, is a notice of some photographs, by Colonel A. T. Fraser, of two dwarfs, taken in the Kurnool district of the Madras Presidency, near Bellary. From the account given of these dwarfs—the hereditary nature of the deformity, its limitation to the males of the family, the inability to walk, the normal bodily growth up to six years of age—it seems possible, if not probable, that the family is afflicted with the disease known as pseudo-hypertrophic paralysis (Duchenne's paralysis). Any physician could settle the question immediately on seeing one of the subjects in question; and very probably a study of the photographs would be sufficient. I have had cases of this disease in my wards at the General Hospital, sent from Bellary. Perhaps Colonel Fraser would kindly send me a copy of one of the photographs, or show them to another medical officer, and tell us his opinion. A. E. GRANT.

Madras, December 2, 1893.

EWART'S INVESTIGATIONS ON ELECTRIC FISHES.[1]

THIS is a magnificent memoir containing the very interesting observations of Prof. Ewart concerning some of the most important chapters of comparative anatomy. Everyone who has an idea of the enormous difficulties connected with these investigations will admire the great skill and successful perseverance with which the author has followed up many dark problems and thrown light upon a number of the most obscure questions.

My studies have been in the same direction for nearly twenty years, and I congratulate my companion in work upon his great success.

It might appear a bold attempt for a foreigner to debate the complicated problems treated in the work; but, on the other hand, there is apparently a strong interest attached to the endeavour to enlarge the field of international intercourse, and this will serve as an excuse for any awkwardness of language.

There is no doubt that science is the chapter of knowledge most entitled to international treatment, and Prof. Ewart himself has done his best to acknowledge the merits of foreign authors. Still, I wonder whether he is aware of the fact that in many places his deductions bear a more or less national character. The proof of that fact cannot be compressed in a few notes, but it may suffice to point out the places where the differences of treating these matters between British and continental writers seem to be most apparent.

Everybody will probably agree that the whole of Prof. Ewart's work deserves very high praise. The plates, which have been accurately drawn by the author himself, are beautifully printed, and yield very ample and useful instruction to anyone who wants anatomical and histological information about these interesting, and yet very imperfectly known, organs. They give a clear account of the immense work the author had to accomplish before he could give so exact and complete representations of the electric organs as well as the cranial nerves and the sense organs.

It will remain to Ewart's undisputed credit that he has brought before the public a large amount of information on the anatomy and histology concerned. The explanation of the plates facilitates the understanding of them, and forms the connecting link between the figures and the tenour of the deductions. It is proved by comparing a great number of organs in different species of Raja, that there are two distinct kinds of electric organs, viz. "cup-shaped," which occurred in *R. circularis*, *radiata* and *fullonica* of the British seas, in *R. eglanteria* from abroad, and, on the other hand, "disc-shaped," which he found in *R. batis*, *macrorhynchus*, *alba*, *oxychynchus*, *maculata*, *clavata*, and *microocellata*.

Everywhere Ewart confirms the statement of former writers, that the electric organs were derived from muscles of the tail which became changed into electric tissue. With great care and skill he has followed the development of these organs in the embryo, and showed how the muscles gave up their firm hold on the sinews, and shrivelled up to discs or cups.

There remains yet one difficulty to be overcome, which the author has not considered; that is to say, he finds the termination of the nerves for each element at the proximal *end* of each muscular club, and compares them (as other authors do) to the motor end plates of common muscles, which are fixed alongside the striped fibres, and not at the end.

That is, of course, a subordinate question, and I am very glad to repeat here what I have stated in former publications of mine, that the muscular origin of the electric organ in the skate also appears beyond doubt, and that I thoroughly agree with Prof. Ewart, not only in that principal question, but also in his deductions regarding the phylogenetic development.

One of the general deductions appears rather strange, not only to me, but to most authors on the continent interested in the matter. How is it possible to consider the electric organs of the skate as such of "vestigial character" before any evidence is given in favour of a retrograde development, which takes place at any period of life? Are not all the statements of Ewart, as well as of former authors, clear proofs that the development is progressive, or at least resting at a certain degree of perfection, after having left the starting-point (muscular tissue) only for a comparatively short distance?

The organs might still advance to further perfection (which I presume they do), or they might become rudimentary again; but so long as there is only progressive and no retrograde motion in the development, it is hardly worth while to argue about the probability of their vestigial character.

I differ only so far from Ewart, as he does not convince me that the electric organs of the skate are as equally perfect as the organs of the Torpedo. In the skate, and up to a certain degree also in Mormyrus, the striated layer of the organ is histologically and optically (in polarised light) proved to be the rest of the original muscular tissue. If there is so much left of the former state of things, it proves, in my opinion, that the process of transformation going on is not so far perfected as in another case (Torpedo), where nothing of the muscular character is left.

I may be permitted to quote here a suggestion I made in a former number of this journal (January 19, 1893) regarding the probable way of phylogenetic development in these organs. It seems to me possible that a kind of physiological alteration changes certain muscles so gradually into electric tissue that a comparatively still imperfect element under favourable circumstances might give an electric shock, which proves useful to the skate for maiming small animals upon which it preys; and so the fish might continue to improve the organ by using it.

Ewart (*R. circularis*, p. 546) argues exactly in the same way as I did, but hesitates to assume that weak electric shocks might be of any use to the skate. It should be kept in mind that small aquatic animals are often extremely sensitive to electricity, and that an unexpected weak electric shock startles even a human individual. At another place, where the author treats the same theory, and grants the possibility of all the other presumptions, he holds back from the universally accepted principle, that constant use makes an organ increase. (Skate, p. 411).

Of course, up to a certain degree, the phylogenetic development of the electric organs contains still a good deal of mystery, and will, I fear, always lack the scientific proof so eagerly looked for; but I must repeat my conviction, that it is easier to imagine the transformation of striped muscle in electric tissue, than to explain by natural selection the development of any distinct, lively-coloured pattern on the wing of an insect.

The cautiousness of the author is, however, to be praised, and it is very interesting to follow his arguments about the *pro et contra* in these complicated matters.

I cannot admire as much another chapter of his paper, where Prof. Ewart does not seem to be quite up to the international mark; it is that in which he compares the number of electric elements in the different electric fishes. (Skate, p. 397). The total of electric elements for each organ in *Torpedo marmorata* he gives as 250,000, and in spite of the great difference of this number with the sum found by other writers before Ewart, he does not say one

[1] "Electrical and Lateral Sense Organs, and on the Cranial Nerves of Elasmobranchs." By Prof. J. C. Ewart. (Edinburgh, 1893.)

word about that divergence. I spent much time in counting the elements in the organ of Torpedo, and confirmed my results by counting also the ganglionic cells belonging to the plates. My total comes very near to that of Valentin, but amounts only to 179,625 in each organ.

Prof. Ewart cannot expect me to give up my number in favour of his, published much later, before he proves that I made a wrong estimate. He speaks also of the large Torpedo of America, and calls it *Torpedo gigantea*. It seems he is not aware of the fact that the name of *Torpedo gigantea* is given to the petrified species from the Monte Bolca, whilst the American species got the name of *T. occidentalis* (Storer). He ignores, or neglects, at the same time, the fact that a near related species, which has about the same number of electric elements, generally named *T. nobiliana* (Bonap.), occurs also in the British seas.

If it is difficult to explain such want of harmony with other authors; it amounts to an impossibility for a foreigner to give a clear account of the following papers concerning the cranial nerves of Elasmobranch fishes. Not that I mean to blame my learned colleague for this; on the contrary, I admire the papers very much, and recommend them with all my heart to everyone who wants instruction about the finer details of these nerves; but with regard to the nomenclature employed, I am afraid very few continental authors will agree with the homologies stated by Ewart. When he in a certain place says against Sappey, "that nerve is all but universally acknowledged to be a part of the facial," Sappey will most decidedly state "that nerve is all but universally acknowledged to be a part of the trigeminal."

Considering it of little use to discuss the confusion of names in the papers quoted by Ewart, I wish it to be borne in mind that the leading principle of the author to prove his homologies is the equality of distribution of the nerves in the peripheric organs. If that holds good, as I am convinced it does, how can he at the same time give the name of a true motor nerve (*N. facialis*) to a cephalic nerve of a true sensitive character? Perhaps he will answer, "All, or at least most, of the other authors do, why shouldn't I do the same?" Putting aside the protest many continental authors (myself included) make against such nomenclature, at any rate his principle of innervation is given up, and I am firmly convinced that the comedy of errors in the nomination of cranial nerves in comparative anatomy will not cease until quite new names or, perhaps still better, only numbers are applied to them according to the place of origin in the substance of the central nervous system. Motor and sensitive nerves must be kept separated by all means, segmental and not segmental nerves may be designated at the same time in any proper way.

It is in this respect that the want of an international understanding is most severely felt, and we must hope that the future may provide an advancement of science also in the matter; for before a firm and clear base for these homologies of nerves is given, we might just as well talk Chinese together.

Professor Ewart's investigations about the cranial nerves had for their chief purpose a clearer insight into the innervation of certain organs of sense, treated in another paper annexed to the same volume. I am very glad to state that the impossibility of accepting his homologies does not interfere with his results as regards the innervation of these sense organs.

The anatomical skill of the author is best shown in the treatment of the structure of these organs. So far as my own experience in these matters goes, I am led to ask, Are his statements and figures of the sensory canals and the nerves belonging to them very correct and complete? He overreaches the previous writers treating the same objects by the admirable finish of his papers which, as far as I see, ought to be followed by another concerning the ampullary canals.

In this chapter I have also to object to his way of treating the literature and of stating homologies in spite of his own principles.

The sense organs I discovered on the skin of Raja, and called "Spaltpapillen," named "pit-organs" by Ewart, were not found by any other author before. It is not true that Merkel saw them on the back of Mustelus and at the mouth of Spatina; he described only "freie Nervenhügel" (*free* nervous collines) in these places. The name itself proves that the sense organs described by Merkel belong to another group altogether, and so does their position.

How Ewart, who places such importance in the distribution of the nerves, can find that the "Spaltpapillen" probably correspond to organs of Squatina placed at the mouth of a very different make and different innervation, he may know himself, but the reader finds it impossible to follow such argument.

The figure he gives of the pit-organ (sensory canals, pl. 3, Fig. 10) does not show such an organ fully developed. Otherwise the split would be narrow and straight, the cells by which it is lined flattened and columnar, not rounded, the papilla itself much higher raised above the surface of the epithelium, and pigment cells frequently found between the epithelial cells. (Comp. my paper: "Uber Bau und Bedeutung der Canalsysteme unter der Haut der Selachier." *Sitz. Ber. d. Berlin Akad. d. Wischenss*, 1888, s. 291.)

The papilla is, in the adult, a good deal raised above the level of the skin, so that even the sense organ at the bottom of the split in the papilla has still a somewhat elevated position. This position prevents me from admiring the name of "pit-organs," as a pit ought to mark a depression below the main level. Continental writers will also shake their heads in reading that a differentiated group of epithelial cells forming a sense organ, and resting between them, is called a "follicle," which by all means wants a kind of stronger envelope enclosing the cells. But I quite agree that in England Latin words might be admitted, which would not do on the continent.

Before Prof. Ewart proceeds to describe the ampullary canals, I recommend him once more to study the paper of Savi concerning these canals. He places the French author amongst those who take also the ampullary canals for sense organs, which is a great mistake, as Savi affirms in most decided terms the excretoric function of the ampullary canals. (Mateucci et Savi, "Études Anatomiques sur le Système Nerveux et sur l'Organe Electrique de la Torpille," p. 331.)

But such objections very slightly detract from the great merit of the author. They only prove the strong interest which the perusal of Prof. Ewart's papers has aroused in me, as it will do in all other readers. I cannot conclude my remarks without acknowledging once more, with all my heart, the magnificent results obtained by him, and I trust that he may succeed further in the same direction.

GUSTAV FRITSCH.

NAVIGATION BY SEMI-AZIMUTHS.[1]

THE year 1893 should be an interesting one to nautical men. A new Daniel has come to judgment in the person of Mr. Ernest Wentworth Buller, M.R.A.I., M.R.U.S.I., M.I.E.E., the inventor of the semi-azimuth system of navigation, who is equally earnest in denouncing the shortcomings of the existing systems of

[1] "Semi-Azimuths. New Method of Navigation, being a combination of Spherical Trigonometry and Mercator's Sailing." (London: Norie and Wilson, 1893.)

navigation, and in advocating the merits of his own. For the precise object of the new method let our author speak for himself. In page iv. of the preface we read: "It is here claimed for the semi-azimuth method that it renders double altitudes unnecessary; that a better result than they have been supposed to yield can be obtained from *either observation* singly, and this also with a great saving of time and trouble."

Again on page 27: "The range of the General Method extends, either from the meridian, or from the limit at which the direct method becomes uncertain, viz., three points from the meridian, up to an azimuth of seven points, or more, that is to say to within less than one point from the prime vertical, which may be considered the practical limit of the semi-azimuth method. . . . It may safely be affirmed that nothing like this extent of range in the computation of latitude has ever been attained by the current systems of navigation."

The feats which our new teacher claims to have accomplished are two in number.

(1) The discovery of a new formula for reduction to the meridian.

(2) By a somewhat tedious system of approximations to obtain by the semi-azimuth method from either of two observations for double altitude the same or a better result than would under the usual double altitude method be derived from the two combined, subject only to the limitation that the body must not be within a point of azimuth from the prime vertical.

Let us consider (1) and (2) in detail.

First with regard to (1), the formula obtained is this:

$$\text{Reduction} = h \cos l \times \text{arc} \quad \text{azimuth}.$$

The formula is a simple one, and now that hour angle is so easily converted into azimuth by the Burdwood and Davis' tables, there seems no objection to its adoption by those who prefer to work out their correction rather than to take it direct from the tables.

Mr. Buller deduces the formula from a somewhat elaborate construction on the Mercator's chart. It is, however, merely an old friend masquerading in a new dress, and he might, if he pleased, have easily obtained it from the expression in ordinary use.

Thus in the formula

$$\sin\frac{\theta}{2} = \sin p \sin c \operatorname{cosec} z \sin^2\frac{h}{2},$$

where θ is the correction, p the polar distance, c the colatitude, z the zenith distance, and h the hour angle, if we assume that

$$\frac{\sin\frac{h}{2}}{\sin\frac{A}{2}} = \frac{\sin h}{\sin A} = \frac{\sin z}{\sin p},$$

where **A** is the azimuth, we obtain

$$\sin\frac{\theta}{2} = \sin c \sin\frac{h}{2} \sin\frac{A}{2},$$

or

$$\theta = \tfrac{1}{2} \sin c \,.\, h \,.\, A \,.\, \sin 1',$$

the Buller formula in a logarithmic shape.

The use of this formula is first exemplified by its application to examples from Jeans' "Navigation," and so long as the azimuth is small it answers well enough. But our author is tempted further afield, and so soon as he gets well away from the meridian his formula begins to give trouble, and he is only able to obtain an accurate result by a process of approximation so cumbrous as to be quite useless for practical purposes. As an instance of this, witness the example of which the results, but not the full work, are shown on p. 14. Were the work to be shown in full, it would occupy more nearly two pages than one page of the text.

Now let us suppose it had not been given to Mr. Buller to discover the methods lately presented to the nautical world, and that he had been content to follow mere ordinary processes.

Imagine, for instance, that he had selected the well-known formula

$$\text{vers Mer Zen Dist} = \text{vers } z - \sin p \sin c \text{ vers } h$$

in order to work out example § 16, p. 12.

Then, upon his own assumption that the latitude was $51°$, or $12'$ in error, he would obtain as a first result lat. $50° \, 49' \, 42''$. Repeating the process with this new latitude, a second approximation would be $50° \, 48' \, 16''$, while a third repetition would result in $50° \, 48' \, 3''$, the true value and this with less than one-third of the trouble.

The semi-azimuth had better, therefore, be confined to observations within a point or so of the meridian.

We pass on to the second and more important part of the task which Mr. Buller has set himself, namely, to show that the latitude within certain wide limits of azimuth may be obtained with accuracy from a single altitude without waiting for a second.

And this may be at once conceded, that by making the necessary adjustments for change of azimuth, and by successive approximations, an altitude may be reduced to the meridian, even when the azimuth is considerable.

But the same result may be obtained from the versine formula given above with far less trouble but greater accuracy.

The question, then, narrows itself to this: Why should we not in all cases of observation within seven points of the meridian, reduce the altitude to the meridian at once, by one method or another, and so obtain the latitude without waiting for hours to take a second altitude, and then making lengthy calculations?

Why have Robertson, and Raper, and Inman, and the other giants failed to hit the bull's-eye, while it is left to Mr. Buller to put his finger on the "blind spot"?

The answer is easily stated.

The "blind spot" is to be found not in the accepted custom of mariners, but in the author himself. Mr. Buller in arriving at his conclusions leaves out of consideration the real vital point which attaches to every observation taken ashore or at sea, but especially to the latter class, namely, what is well defined by Raper as the "Degree of dependence" to be placed on the observation.

In every observation off the meridian we have to deal in one form or another with a spherical triangle, in which three elements being given we have to find a fourth.

Now the three given elements are in general known only approximately, and it behoves us to find under what conditions an observation should be taken that the smallest possible error may be produced in the final result. In the problem under consideration, treated by the Buller process, the data are the polar distance, the approximate colatitude, and the zenith distance.

Of these the polar distance may fairly be regarded as accurately known, since the difference can only be at most but a few seconds.

The latitude is required to be known only approximately, the object of the observation being to find the amount it is in error, and it has already been admitted that the new process will suffice to obtain the correct latitude, *always supposing that the observed altitude is correct.*

What reason is there then to consider that the altitude is correct, and what will be the effect if it is incorrect?

When we take into account the haziness and uncertainty of the horizon, the difficulty of accurate observation on board a rolling ship, the varying effects of refraction, the imperfections of the sextant, and the personal error of the observer, it is probable that an average error of $2'$ is a very moderate estimate.

Taking $2'$, therefore, as an average value, let us see what will be its effect upon the typical examples in the text, worked first by the new process, and secondly by the old method of double altitude.

On page 30 an example (§ 32) is taken from Lecky, which will answer the purpose.

The data are as follows. Lat DR 32° 15′ N.

March 7, 1880.	Times by chron. h. m. s.	Altitude. ° ′ ″	Bearing. °
11h. a.m. ...	1 11 3·8 ...	50 0 0 ...	S. 25 E.
3h. p.m. ...	5 11 1·2 ...	33 17 45 ...	S. 57 W.

Sun's declination, 4° 59′ 15″ S. for first observation; 4° 55′ 30″ S. for second observation.

Let us suppose that an error of 2′ occurs in the second altitude, the one treated for reduction to the meridian.

The error in latitude (dl) produced by an error in altitude (dz) is given approximately by the formula

$$dl = \sec A_1 \,.\, dz,$$

where A_1 is the azimuth of the body.

Thus $dl = \sec 57° \times 2'$, or 3′ 40″ nearly.

But if treated, so as to take in both observations, by the double altitude process,

$$dl = \frac{\sin A_2}{\sin (A_1 + A_2)} dz,$$

where $A_1 = 57°$, $A_2 = 25°$, the azimuths being reckoned from south in each case. Thus

$$dl = \frac{\sin 25°}{\sin 82°} 2' \text{ or } 51''.$$

So that in one case a reasonable error in altitude gives an error in latitude of nearly 4′, in the other of less than 1′.

One other instance will perhaps suffice. On page 36 an example from Riddle is worked out, wherein an error of 2′ in altitude would produce an error of 18′ upwards in latitude.

Such a result at once condemns the observation. Indeed, for the purpose of accurate determination of latitude, the double altitude stands out among the various methods a very king. In other cases, as in the meridian altitude, we are satisfied if the latitude is no more in error than the original observation. In a double altitude, taken under advantageous conditions, only a fraction of the error in altitude appears in the final result.

It is somewhat remarkable that Mr. Buller's evident appreciation of Sumner methods has not made him more familiar with the main principles which apply equally to all classes of observations.

Every observation furnishes the observer with a circle upon the globe, a straight line upon the Mercator's chart, on which to place his position. The circle has the sun's projection on the earth for its centre. The line has the sun's line of bearing perpendicular to it.

If this line of position is inclined at a very acute angle to the meridian, that is, if the body observed is near the prime vertical, it is evident that a very small increase in the perpendicular drawn from the sun to the line of position, that is, a very small increase of zenith distance, will produce relatively a very large difference of latitude. And this condition, coupled with the impossibility of obtaining accurate altitudes at sea, is sufficient to account for the restriction of ex-meridian observations to a point or two in azimuth from the meridian.

The British seaman, therefore, had better pause before he throws overboard his Norie or his Raper, and takes to his heart the new Buller methods.

The greatest self-confidence, the most implicit belief in the reality of the mission to which he has been called, will not enable Mr. Buller to find the latitude accurately by a single altitude near the prime vertical, for the very simple reason that the error (even when supposed small) which must be expected in the altitude produces a large error in the latitude, and thus vitiates the result.

If he would make the ex-meridian method available as he proposes to do, at almost any time of day, the author must supplement his treatise by the invention of some appliance for measuring altitudes very much superior to any now in use.

Pending its production the very pertinent question asked in page 36, "We have known long enough how to get a fairly correct A. T. S. from an observation near the Prime Vertical and the latitude D. R., but who has yet shown how to obtain the *True Latitude*"? must remain unanswered.

There is indeed freshness in the "New Method of Navigation," Part I., but no light. That perhaps will be supplied by Part II. G.

VOICES FROM ABROAD.

THE following literal translation of parts of an article recently published in the *Chemiker Zeitung* (Nos. 85 and 86, 1893) is an appropriate addendum to a recent article of mine in this journal. It must be sorrowfully admitted that in essential particulars the picture is a true one. HENRY E. ARMSTRONG.

"Notwithstanding the enormous industrial development of England, the appreciation of science by technical workers is inconceivably slight, the main cause being deficient comprehension. The Englishman is conservative in all his customs, in his way of living, and not less in his methods of manufacturing, so that there are still very many manufacturers who would be as little prepared to place the control of their works in the hands of a scientific chemist as to convert them into philanthropic institutions. At present great efforts are certainly being made to alter this condition of affairs by the aid of technical schools modelled on German lines, but opinions as to the value of these schools are as yet much divided; and, indeed, for various reasons their ultimate success is doubted. In the first place, it is to be borne in mind that these institutions are not under State control, but are governed and controlled by local boards. Moreover, the preliminary training which their students have received is not to be compared with that of students in the German institutions, as an education such as is given in the German Realschulen and Gymnasiums—of the character given in England at most by the grammar schools—is only procurable by those who are well off, owing to the enormously high school fees (about £20 or 400 marks a year). The possibility of consolidating and widening the technical training by a short subsequent course of scientific study at the University is absolutely out of the question in most cases, owing to its extreme costliness. It is therefore probable that these schools will but produce a number of half-educated persons who will take up positions as chemists and will thereby but bring the chemist proper into discredit.

"It is clear that under these circumstances there is but very little prospect that a chemist coming to England will find a suitable position. I cannot sufficiently strongly caution 'young chemists' against coming to England on the chance of picking up something good, even when provided with good introductions. So few analysts are in demand here that the chance of securing such a post is most uncertain. Works and laboratories in which scientific work is systematically carried on scarcely exist, not one even of the English aniline colour works having a scientific laboratory worthy of the name. The 'young chemist' has therefore very little chance of securing an appointment, as he does not possess the necessary qualification for a works post, that is to say experience, and the volunteer nuisance is scarcely known here even by name. I can therefore only repeat that it is a very risky enterprise for a young, inexperienced chemist to come to England without a definite engagement, as so often happens. The result, with very few exceptions, is disillusionment, and many get into most unfortunate positions through financial pressure. The outlook is somewhat better for a chemist who has had experience and practice in works. But even such will find it infinitely more difficult to find posts in England than it is either in Germany or Austria, and will do well to go to England only when offered a definite appointment. The thorough scientific training and business capacity of the German chemist is unreservedly recognised by all unprejudiced judges in contradistinction to that of his English colleague. In carrying on routine operations, the English chemist is doubtless as competent as the German chemist, even if he be not his superior, but in conducting and developing chemical industries on a scientific basis the latter is far in advance of the former.

"I need refer but briefly to the great chemical industries, as they are well enough known. Of these the first to be mentioned is the soda industry, including that of sulphuric acid and chlorine ; furthermore, tar-distilling, dyeing, calico-printing, the manufacture of iron, steel, copper, tin, and antimony, glass-making, the utilisation of fatty matters, the Scotch paraffin industry, and the manufacture of bichromate. These industries, excepting glass-making, employ a considerable number of chemists, although, in proportion to their output, not nearly so many as the German works. This is especially the case in dye works, calico-printing works, and in those dealing with fatty matters, many of which carry on their manufacture without chemists, or only with the aid of very imperfectly trained chemists, as every one here regards himself as a full-blown chemist who, after a most elementary preliminary training, has attended a technical course during one, or at most, two years. At least 80 per cent. of the chemists engaged in the industries mentioned are Englishmen, the remainder being either Germans or Swiss. I have never met a French chemist here. With few exceptions the condition of these industries during recent years must be characterised as dull and even as bad in some cases ; they therefore offer the chemist little prospect of employment, and foreign capital is certainly not to be invested in them with advantage. Only dye-works and those utilising fatty matters offer a prospect to the experienced chemist, as these are both distinctly capable of being improved in position. The helpless condition of the English aniline colour works is peculiar, these having been simply stifled by the German works, which have developed with such giant strides. The English works eke out a miserable existence, and altogether do not employ as many chemists as are to be found in a similar German works of the fifth or sixth rank. Fuchsine, soluble blue, chrysoidine, Bismarck brown, and the few naphthol colours unprotected by patents, are almost the only colours manufactured. Not a single dyestuff of importance is made by any English firm alone, as scientific laboratories such as are a matter of course in every German works exist here only in the most rudimentary form. Most of the chemists engaged in English aniline colour works are German (??), but the demand for chemists in these works is very small. The erection of such a works in England on the German model could only be achieved by the large German firms engaged in this industry ; it is another question whether it would pay. But the manufacture of pigments—of mineral colours and lakes—is certainly capable of development here. It is true there are a number of such works, but these rarely employ a chemist, and still more rarely one who has had a thorough scientific training. Consequently, enormous quantities of lakes are imported, especially for printing oil-cloths and carpets, which might equally well be manufactured on the spot. The necessary capital would be not an inconsiderable one, and may be estimated at, at least, 150,000 marks. Competent chemists in this branch can probably count on easily finding employment here.

"The manufacture of fine chemicals, which at present are almost entirely imported from Germany and France, is certainly capable of considerable development here. Of these may be mentioned especially, tannin, tartar emetic, pyrogallol, oxalic acid, cyanide of potassium, and most of the almost innumerable chemicals and preparations which are made use of in trade, and which are either not made here at all, or in altogether insufficient quantity and of poor quality. With reference to such articles, in the case of which wages form a considerable item in the cost of production, it is to be borne in mind that English wages are on the average considerably higher than German."

THE EFFECTS OF LIGHT ON THE ELECTRICAL DISCHARGE.

WHILE engaged on his classical experiments Hertz noticed that the appearance of the discharge between the two terminals of the oscillator was greatly changed upon the spark gap being illuminated by the light coming from another spark. This change was not due to an electrical action of the sparks, for it was equally well produced by other sources of light, such as the electric arc and burning magnesium, while all effect immediately ceased on interposing a plate of glass. Since the time when the above observations were made many experimentalists have investigated this subject and have obtained rather divergent results. In most cases the source of light employed has been the electric arc formed between carbon rods, though, with a view to increase the proportion of ultra-violet rays emitted, Bichat and Blondlot used carbon rods with aluminium cores, while Righi used a zinc rod for one terminal. Other observers have used the spark of an induction coil passing between terminals of copper, zinc, or aluminium. While Hertz had only noticed that the illumination of the discharging knobs increased the facility with which sparks passed, Wiedemann, Ebert and Hallwachs found that it was only when the negative terminal was illuminated that this effect took place. More recent observations by Branly have led to this view being modified, for he finds that on illuminating a piece of zinc by the sparks of a large induction coil produced between aluminium terminals, if the source of light is sufficiently near to the plate, the loss of charge is nearly as rapid for a positive as for a negative charge. On increasing the distance between the spark and the charged plate, the decrease in the rate of loss of charge is much more rapid for positive than for negative charges, and thus at some distance from the source of light the negative charge is the only one which is appreciably affected. Hence radiation of certain kinds increases the rate at which a positively charged body loses its charge, just as in the case of a negative charge, but the rays which are active in the case of positive electricity are absorbed by even a small thickness of air, while those rays which are unabsorbed are still able to accelerate the discharge of a negatively charged body.

After having made a series of experiments in air at ordinary pressures, Stoletow on the one hand, and Righi on the other, have investigated the influence of pressure on the phenomenon, and have both found that the effect increases with a decrease of pressure, while Stoletow has shown that if the rarefaction is carried to the extreme limit there exists a pressure, after which the effect decreases as the pressure is further diminished.

An experiment of Bichat's seems to show that the loss of electricity is due to convection currents, and this view has been further strengthened by Righi, who placed a plate of ebonite covered with tin foil on its upper side above a brass plate on which some figure, such as a cross, had been traced with varnish so that the plate was protected at these points from the effect of the illumination, the active rays being absorbed by the varnish. The negative pole of an electric machine was connected to this plate, the positive pole being connected to the tin foil, and the light of an electric arc allowed to fall on the under plate for a few seconds. The plate of ebonite being removed and powdered over with a mixture of sulphur and red lead a yellow cross on a red background was obtained of the same size as the one traced on the brass plate. As the sulphur attaches itself to those parts of the plate which are positively, and the red lead to those which are negatively charged, it follows that the parts of the lower plate which were not protected by the varnish have lost some of their negative charge, which has been carried on to the ebonite plate, and that this displacement has followed the lines of force of the electric field between the plates, which are in this case perpendicular to the two plates. This conclusion is further strengthened by observing that, if the electrified particles which escape from the lower plate are prevented, by means of a screen, from reaching the ebonite, a shadow of the screen is obtained.

The explanation that this convection is caused by the molecules of gas which, after being in contact with the body, become charged and are repelled, is hardly satisfactory, and the experiments of MM. Lénard and Wolf seem to show that it is particles of dust which carry the charge, for they suspended an insulated plate of metal in a box filled with air which had been carefully freed from dust. A plate of quartz fixed in one side of this box allowed the light from an electric arc to fall on the metal plate, while a stream of some vapour could be introduced through a side tube. Under these circumstances the vapour was condensed on allowing the light to fall on the plate if it was uncharged or negatively charged, while if the plate was positively charged no condensation took place. As it is known that a given space can become supersaturated with vapour when no dust is present, but that the introduction of the least trace of dust causes an immediate condensation, it appears that when a body either uncharged or negatively charged is illuminated it gives off some dust, and that the loss of charge is due to this dust. Further particulars of the work which has been done in this subject are given in a paper by M. Blondin in *Électricité*, p. 313, 1893. W. W.

NEOLITHIC DISCOVERIES IN BELGIUM.

THE fact that in Belgium flint was in certain districts largely worked during Neolithic times, for the manufacture of hatchets and other implements, has long been well known. The mines in the chalk near Mons, from which the rough blocks of flint were procured by the ancient flint-workers, have frequently been described, and bear a close analogy with the old workings at Grimes' Graves, near Brandon, and with the pits near that place, still being sunk by the flint-knappers of the present day. The fields in the neighbourhood of Mons have their surface strewn with roughly-chipped hatchets, and in other districts the occurrence of worked flints has been not unfrequently noted. In a memoir, recently published in the *Bulletin de la Société d'Anthropologie de Bruxelles* (Tome xi. 1892–93), M. G. Cumont has placed on record his discovery of two important Neolithic stations at Verrewinckel and Rhode-Saint-Genèse, neither of which places is far from the main road from Brussels to Charleroi, while both lie at but a short distance from the field of Waterloo. The forest of Soignes extended in early times over the whole district, and though both stations are on promontories of high land, there are or were, in the neighbourhood of each, springs or ponds from which to obtain a supply of water and, possibly, of fish.

The principal of the two was that at Rhode-Saint-Genèse, whence, including flakes and scrapers, M. Cumont has obtained no less than 3591 worked flints, a few implements made of other kinds of stone being reckoned among them; while Verrewinckel is credited with 815 specimens. Of all the forms a good summary account is given, and characteristic examples are figured in five plates. A detailed map of the district is also given. That the manufacture was carried on at the stations is proved by the presence of upwards of 240 nuclei from which flakes have been dislodged; but few of these appear to have rivalled in size those of specimens near Mons. It is indeed suggested that the hatchets and larger implements were rough-hewn at Spiennes, and finished where they were found. That this was the case is further shown by the fact that some twenty *polissoirs* were collected by M. Cumont, who also regards the flint which forms the material of the implements as having been derived from Spiennes, Obourg, or the neighbourhood of Mons. Over a hundred arrow-heads figure in the lists, and some of these, as shown in the plates, exhibit skilful workmanship. A few quaternary or palæolithic implements from the same region have been described by M. Cumont in another paper. He is to be congratulated on the rich harvest that he has reaped by his labours, which have now extended over a period of eight years. J. E.

THE LATE SIR SAMUEL BAKER.

NOTHING impresses more vividly upon one the rapid unfolding of our knowledge of Africa than the fact that the pioneers who forced the first paths into the unknown interior have survived to see generation after generation of younger men, who followed in their footsteps, fall victims to the fatal fascination of that continent. Burton, Grant, and Oswell, the companion of Livingstone's earliest journeys, have died so recently that we realise with a feeling of sorrowful surprise that the last of the first great group of explorers has passed away in the person of Sir Samuel White Baker, on December 30, 1893.

He was born in London in 1821, and after his school education turned his attention to engineering, but his professional work never took so thorough a hold upon his mind as the love for travel and sport, which his private means fortunately enabled him to gratify to his heart's content. Baker first went to Ceylon for elephant shooting in 1845, and saw a great deal of the island in subsequent years. Two books resulted from this experience—"The Rifle and Hound in Ceylon," published in 1854, and "Eight Years' Wanderings in Ceylon," in 1855. The study which he made of the climate of the elevated part of Ceylon led him to establish a colony of English agriculturists, fully equipped with a stock of cattle and sheep, at Nowera Eliya, over 6,000 feet above the sea, which is now a noted health resort. On the death of his wife, in 1855, he went to the Crimea, and carried out some railway work subsequently on the Black Sea coast. In 1860 he married a Hungarian lady, who survives him, after being his devoted companion through the trying years of African adventures, and in the pleasanter wanderings of his later life.

In 1861 he went to Egypt, resolved to carry on an extensive scheme of exploration at his own expense. With this object he spent a year in Abyssinia, working out the complete hydrography of the Atbara and its tributaries, and then started from Khartum to follow up the White Nile itself. In February, 1863, he met Speke and Grant at Gondokoro, returning from their great journey to the

Victoria Nyanza, and a year later Baker was able to supplement their discovery by arriving on the shores of the Albert Nyanza, the size of which he considerably over-estimated. He did not return to London until 1866, and found his fame as a traveller established. He received many honours, including that of knighthood and the gold medals of the Royal Geographical Society and the Paris Geographical Society; but in the following year, again accompanied by Lady Baker, he returned to Africa. The story of his first journey is recorded in two fascinating books—"The Albert Nyanza Great Basin of the Nile," in 1866, and "The Nile Tributaries of Abyssinia," in 1867. In 1869 he commenced the occupation of the upper White Nile provinces for the Egyptian Government, at the head of a body of Egyptian troops, and for five years laboured at the heavy task of restraining the slave-dealing Arabs and keeping in order his apathetic and often disaffected Egyptian subordinates. He established steam navigation on the Nile to the equator, and in his "Ismailia," published in 1874, told the story of the extension of Egypt. This completed his career as a pioneer and explorer; but a traveller he remained to the very end of life, and until last year he spent almost every winter either in Egypt or in India. He took a keen interest in the geography of Africa, and at critical moments in the course of recent developments in that continent he did not fail to give the benefit of his advice for the guidance of the country.

In 1879 he visited every part of the island of Cyprus, recording his impressions in "Cyprus as I saw it in 1879." The many reminiscences of his hunting adventures in every continent made his last book, "Wild Beasts and their Ways," a most valuable contribution to that liberal form of natural history which studies the lower animals as mankind is studied by the sociologist or historian rather than by the anatomist or physiologist. Baker was elected a Fellow of the Royal Society in 1869, and received the official recognition of several governments and innumerable learned societies in all countries for his services to geography and to humanity. His health kept up to within a month of his death, and to the last he remained a keen sportsman. He died in his residence at Sandford Orleigh, Newton Abbot, in Devonshire, and his funeral takes place at Woking to-day.

NOTES.

THE list of New Year honours contains the names of two men of science in the public service—Mr. Norman Lockyer, F.R.S., Professor of Astronomy in the Royal College of Science, and Mr. W. H. Preece, F.R.S., Engineer-in-chief to the General Post Office—upon both of whom have been conferred Companionships of the Bath.

WE note with much regret that Prof. Milnes Marshall, F.R.S., of the Owens College, Manchester, met with a fatal accident while ascending Scawfell, on Sunday, December 31. A notice of his life and work will appear in our next issue.

WE have to record the death of Mr. R. Bentley, Emeritus Professor of Botany in King's College, on December 24, at the age of seventy-two. Mr. Bentley became botanical lecturer at King's College in 1859, and three years later he was appointed professor of botany at the London Institute. He was twice—in 1866 and 1867—elected president of the Pharmaceutical Conference, and was well known for his works on pharmaceutical botany.

THE death is announced of Mr. R. Spruce, the well-known botanist and explorer, in his 67th year. Rather more than forty years ago Mr. Spruce visited South America on behalf of the Royal Gardens at Kew, and successfully carried out some very important scientific investigations. He explored the river Amazon, and crossed the continent from the Atlantic to the Pacific. The introduction of the cultivation of cinchona into India was very largely the result of Mr. Spruce's work, and his fine collection of plants have done good service to commerce and to botanical science.

THE chair of Agricultural Chemistry in the University of Tokio has been accepted by Prof. Loew, of Munich.

MR. SMITH HILL has been appointed Principal of the Aspatria Agriculture College, in succession to the late Dr. Webb.

WE understand that the Queensland Government, in pursuance of their policy of retrenchment, have abolished the post of Government botanist hitherto held by Mr. F. M. Bailey.

PROF. W. H. CORFIELD has been appointed President, and Dr. P. F. Moline secretary, of the English committee of the International Congress of Hygiene and Demography to be held at Budapest this year.

DALZIEL'S correspondent at Copenhagen states that the time of Central Europe was adopted throughout Denmark on the first day of this year.

A PRIZE of 1250 francs is offered by the Natural History Society of Dantzic for the best means of destroying the poisonous insects in the forests of Western Prussia.

WE learn from the *Times* that the sum of £600 a year has been bequeathed to the trustees of the Mason College, Birmingham, by the late Mr. Aubrey Bowen, of Melbourne. In making the bequest the testator stipulates that the trustees shall apply the sum in founding six scholarships of £100 a year each in connection with the college, to be called respectively the first, second, and third Bowen scholarships, for the promotion of the study of metallurgy, and civil, mechanical, and electrical engineering; and the rest Priestley scholarships, for the promotion of the study of chemistry.

THE refusal of the S.P.C.K. to withdraw a book by Prof. Percy Frankland because in it experiments on living animals were approved, has led Lord Coleridge to address a letter to the secretary of the Society, in which he says: "I have learned from what seems unquestionable authority that those who administer the affairs of the Society for Promoting Christian Knowledge have finally determined to range the society in the number of those favouring the practice of vivisection and advocating its horrors. It is my duty, as I regard it, to separate myself at once from such a body, and I have accordingly directed Messrs. Childs not to pay any further subscription to the society. As I informed you of what I should feel bound to do in the events which have happened, I shall not occasion the society any inconvenience."

THE following officers of sections have been appointed for the meeting of the Australasian Association for the Advancement of Science, to be held at Brisbane this year:—Section A—Astronomy, Mathematics, and Physics: Vice-presidents, Mr. Clement Wragge and Mr. John Tebbutt; secretary, Mr. J. P. Thomson. Section B—Chemistry: Vice-president, Mr. J. B. Henderson; secretary, Mr. G. Watkins. Section C—Geology and Mineralogy: Vice-president, Mr. W. H. Rands; secretary, Mr. Hargreaves. Section D—Biology: Vice-presidents, Dr. A. Dendy, Mr. F. M. Bailey, and Mr. J. J. Fletcher; secretary, Mr. J. H. Simmonds. Section E—Geography: Vice-president, Mr. D. S. Thistlethwayte, C.E.; secretary, Major A. J. Boyd. Section F—Ethnology and Anthropology: Vice-presidents, Rev. James Chalmers and Mr. E. M. Curr; secretary, Mr. Archibald Meston. Section G—Economic Science and Agriculture: Vice-presidents, Mr. G. A. Coghlan and Mr.

James Tolson; secretary, Mr. Wm. Soutter. Section J—Mental Science and Education: Vice-presidents, his Grace Archbishop Dunne, Mr. G. J. Anderson, M.A., and Mr. D. Cameron.

An earthquake shock was felt in Shepton Mallet, Somerset, and neighbourhood on December 30, about 11.30 p.m., and another shortly after midnight. The direction of motion of the waves was apparently from north to south. Prof. F. J. Allen sends us the following description of what was noticed by some friends of his. "At about 11.20 p.m. a shock was felt by three persons in one house; and about an hour later a second and more severe shock was observed by two of these persons. In another house, a quarter of a mile distant, three distinct shocks were felt by several persons. Both these houses are situated on the south side of the valley, whereas the reports published in the papers refer more particularly to movements observed on the north side. For those who are not acquainted with the district, I would mention that the strata (Carboniferous limestone, with overlying Trias, Lias, and Oolite) are very much disturbed, and present many interesting studies of horizontal as well as vertical faulting. It is just the kind of spot in which one might expect to have superficial movements occurring from time to time."

A LETTER from Prof. S. J. Bailey, of the Harvard College Observatory, to the editor of *La Bolsa*, published at Arequipa, Peru, gives an account of the establishment of the meteorological station on the summit of the Misti, in the Peruvian Andes, at an altitude of 19,300 feet above sea level, this being at present the highest observatory in existence. The fatigues undergone by observers ascending the conical peak from Arequipa are such as to render exact observations impossible, and it was therefore found necessary to construct a mule-path to the summit from a stone hut erected at an elevation of about 16,000 feet. This hut was erected on the north-east slope, being the most accessible side of this peak, which maintains its aspect of an isolated symmetrical cone from all points of view. On September 27 the summit was reached by Prof. Bailey, his assistant, several Indians, and two mules. The latter could hardly be made to go more than twenty paces without a rest. On October 12 the summit was revisited with two members of the Arequipa observatory, twelve Indians, and thirteen mules carrying materials for erecting two huts, and the registering meteorological instruments, comprising a barograph, a thermograph, several mercury thermometers, an hygrometer and anemometers. Each of the registering instruments works for ten days, and a member of the observatory will visit the station three times a month. A store of provisions is kept at the stone hut, and of the wooden huts at the top, one, provided with double wooden walls, is intended for the observer, the other for the instruments.

WITH regard to meteorological work in Australia, Sir Charles Todd remarked at the last meeting of the Australasian Association for the Advancement of Science, that in New South Wales there were 175 meteorological stations and 1063 rain gauges; in Victoria, 31 meteorological stations and 515 rain gauges; in South Australia, 22 meteorological stations and 370 rain gauges. In Australia there were 385 meteorological stations and 2580 rain gauges. During the last four years the forecasts issued in South Australia have been justified to the extent of 73 per cent., partially justified 20 per cent., and wholly wrong 7 per cent.

THE radius of curvature of the cornea, together with the indices of refraction of the various refractive media of the eye, constitute the experimental data for determining the most important points about the eye. Drs. H. C. Chapman and A. P. Brubaker have measured this radius in fifty individuals by means of the ophthalometer (Proc. Acad. Nat. Sci., Philadelphia, 1893, p. 349), and they have found that in the average young man it amounts in the horizontal meridian to 7·797 mm., and in the vertical meridian to 7·552.

THE Director of the Central Meteorological Observatory of Mexico, Señor M. Bárcena, has published an interesting pamphlet on the climate of the city of Mexico, based on the hourly observations of sixteen years 1877–92. Mexico, from its position of 7431 feet above the sea, and latitude 19°, might be supposed to be subject to great extremes of temperature, but as one geographical element neutralises the other, the result is a temperate and agreeable climate. The mean annual temperature is 59°·7, and the monthly means vary from 53°·6 in December to 64°·6 in May. The absolute maxima in the shade vary from 73°·4 in December to 88°·9 in April, while the absolute minima vary from 28°·9 in December to 46°·8 in August and September. The greatest daily range amounted to 41° in the month of March. The mean annual rainfall amounted to 23·8 inches, the wettest months being June to September; the greatest fall in one day was 2·5 inches in August 1888. The prevalent wind is north-west, which blows during most part of the year, and is the coldest and wettest quarter. The strongest wind blows from the north-east; the greatest hourly velocity observed during the sixteen years was about 56 miles per hour.

A DETAILED investigation of the properties of mirror silver chemically precipitated on glass is published by Herr H. Lüdtke in *Wiedemann's Annalen*. The three modifications of silver obtained in the wet way, termed by H. Vogel the arborescent, the powdery, and the mirror variety respectively, have been recently enriched by Mr. Carey Lea through his discovery of colloid silve . Herr Lüdtke thinks that this last variety and mirror silver are closely allied; that the latter, when newly formed, is indeed identical with the former. The electrical resistance of several varieties of mirror silver decreases considerably with their age. No such decrease was, however, observed in the case of mirrors produced by Martin's process or by that of Liebig, *i.e.* reduction by means of milk-sugar. On introducing a pole of ordinary silver and one of allotropic mirror silver into a weak acid or salt solution, and closing the circuit, a current was obtained indicating a difference of potential of about 0·1 volt between the two varieties, the allotropic variety being the positive pole. These conditions were reversed if the solution was one of silver nitrate, but the difference of potential was less. Lehmann's surmise that the precipitation of the mirror on the glass is due to a thin layer of sodium silicate, was invalidated by precipitating it on mica, porcelain, quartz, and platinum by the same methods.

THE *Philosophical Magazine* for the present month contains a paper, by H. Nagaoka, on the hysteresis attending the change in length produced by magnetisation in nickel and iron. The author at first used the interference fringes produced between a plano-convex lens and a plate of plane glass attached to the end of the rod under examination to measure change in length. He found, however, that it was impossible to keep the temperature of the apparatus constant during the time necessary to make an observation, and also that there was considerable difficulty in counting the number of fringes displaced. To overcome the temperature difficulty the author has made use of the principle of the gridiron-pendulum, and has by this means succeeded in almost entirely overcoming this difficulty. In place of the interference bands he uses an optical lever, that is, a mirror fixed to a small base, to which are attached three needle points, two of these rest in a grove on the base-plate of the instrument, while the third rests on a small glass plate fixed to the end of the iron or nickel rod. The rod was placed along the axis of a solenoid which lay in a horizontal position pointing magnetic east and west. The deflection of the mirror was measured by means of a microscope with a micrometer eyepiece, such that one division of the scale corresponded to a deflection of the mirror of 0"·295 of arc, or to an elongation of 0·805 × 10^{-7} c.m. Experiments were made on wires of iron and nickel of different lengths, and he finds in every case that the elongation in iron and the contraction in nickel by magnetisation is accompanied by marked hysteresis. The curve of hysteresis is symmetrical with respect to the line of zero magnetising force, so that the elongation or contraction during cyclic changes is an even function of the magnetising force. When a wire has been magnetised it cannot be brought to its original length by simply reversing the magnetic field.

IN a note communicated to the same number of the *Philosophical Magazine*, Prof. Knott calls attention to the similarity between the effects observed by Mr. Nagaoka, and those which he has himself observed in the case of magnetic-twist cycles for iron and nickel. A steady current was passed along the wire under observation, and the longitudinal field acting on the wire was gradually altered between the limits $\pm H$, and at suitable intervals observations of the twist made. It was found that with a small range of field the hysteresis curve obtained by plotting twist against field was very similar to the well-known hysteresis curve of magnetisation. With limiting fields, however, stronger than the field which produces the maximum twist, the hysteresis curve crossed itself twice and formed three loops. In the magneto-elongation cycle the change of sign of the magnetising force does not produce a change of sign in the elongation. On the other hand, in the magnetic-twist cycle, as the magnetic force passes through zero from positive to negative, the twist tends to do the same, though with a lag. The author considers that the twist, under a given combination of circular and longitudinal magnetising forces, depends not only upon the elongations but also upon some function of these forces which changes sign with each, and to which the existence of the maximum twist is largely if not entirely due.

AN entertaining chapter on minerals, and the popular superstitions connected with them in Germany, is contributed to *Die Natur* by Friedrich Klinkhardt. The fact that variety among minerals is less easily perceived than that among plants and animals, is emphasised by the great influence that "a stone" pure and simple, without further specification, is capable of exerting in the popular estimation. Children under the age of one may not play with stones, otherwise bread will be scarce. An ill omen may be made innocuous by throwing a stone into the road before taking the next breath. Chalk is credited with many virtues, and is used both for its own efficacy and for making signs with. Cows marked all down the spine with chalk consecrated at Epiphany, remain healthy, and always find their way home. Alabaster in water is used for curing sick children in Bohemia. A flint pebble from the brook, if thrown over the roof into the poultry yard, encourages the hens to lay eggs. The beliefs connected with "thunderbolts," which are sometimes flint instruments, or quartz crystals, or lightning tubes, are exceedingly numerous. In the Palatinate it is believed that thunderbolts, after penetrating seven yards into the ground, rise a yard every year; this reminds one of Miölnir, Thor's hammer, which returned to his hand.

WE read an interesting paper "On the Kulm District of Lenzkirch in the Black Forest," by Dr. Rafael Herrmann. A geological map of the district is given, scale 1 : 50,000 (*Berichte der Naturforschenden Gesellschaft zu Freiburg i. B.*, June, 1893). In the Black Forest, just as in the Hartz and in Thüringia, two main series of carboniferous rocks are recognisable, an older group of dark shales, and a younger formation of conglomeratic rock. During the intermediate epoch, the upraising and folding of the rocks took place, associated with intrusions of crystalline rock. The eruptive rocks of the district are granite, coarse and fine grained, granitite, granitic dykes, quartz porpyhry, porphyritic dykes, and porphyritic breccias. Herrmann does not agree witk Vogelgesang that the granite and granitite are petrographical varieties of one and the same rock united by a complete transitional series, but regards them as two independent masses of rock, differing in composition and structure. All the granitic rocks have been intensely affected by pressure, whereas the younger porphyry shows no appearance of it. Herrmann deduces, therefore, that the intrusion of porphyry marks what was probably the last phase of folding and overthrusting of rocks in the Black Forest during the Carboniferous period.

THE region watered by the upper part of the Yenisei (which is known to the Mongols under the name of Ulu-Khem, and is made up by the confluence of the Bei-khem and the Kha-khem) belonged until lately to the least known parts of north-west Mongolia. The opinion expressed in the "General Sketch of the Orography of East Siberia" (*Zapiski* of the Russian Geographical Society, vol. v. 1874), to the effect that it must be a high plateau, and that the so-called circular chain Erghik-targak-taiga is nothing but a border ridge, or often but the steep slope of the plateau, had been contested. Now it finds its full confirmation in the recent exploration of the region by Mr. Kryloff, published, with a map, in the *Izvestia* of the Russian Geographical Society (vol. xxix. 4, 1893). The whole region really has the above-menioned character. After having left the valley of the main river, which has, even at the junction of the two Khems, an altitude of 1873 feet, Mr. Kryloff had to travel all the time on the level of the high plateau, never finding altitudes less than 3000 feet, till he returned to the Russian dominions in the basin of the Tuba. Mr. Kryloff's journey having been performed for the St. Petersburg Botanical Garden, special attention has been paid by the explorer to the flora of the region; and he found that the vegetation on the plateau assumes in many places the character of a Steppe vegetation, namely, in the flat but high valleys of the rivers, which are dotted by numerous small lakes. At the sources of the Bei-khem, the flat surface of the water parting, as well as large portions of the plateau itself, raise above the level of the tree-vegetation, usually marked by the cedar, and are covered with Alpine meadows. As to the ridges which rise above the surface of the plateau, they attain heights of over 7000 feet, and over 8000 feet in the Tannu-ola ridge in the north of Lake Ubsa-nor.

THE same number of the *Izvestia* contains a paper by M. M. Pomortseff, on his extremely valuable observations on the directions and angular speed of motion of clouds. The method resorted to for these observations is described at length, and the instrument which was used for this purpose is figured on a plate. The chief results are given on 94 separate small maps. The author himself sums up his results as follows:—(1) The middle of the cumulus clouds moves almost in the direction of the isobar which passes through the place of the observer. (2) The cirrus, cirro-cumulus, and cirro-stratus clouds move on a pretty long distance as a broad and nearly straight-line current—the direction of the stream being almost parallel to the part of the 760 mm. isobar which stands on the line connecting together the centres of two nearest and contiguous regions of high and low pressure. (3) There is doubtless a connection between the distribution of atmospheric pressure and the march of the barometer on the earth, and the vertical circulation of the atmosphere; but this connection does not extend farther than the height of the upper, *i.e.* cirrus clouds.

IN a letter addressed to the Russian Geographical Society from Lan-chou, in March last (*Izvestia*, vol. xxix. 4), Mr.

Obrucheff wrote that while crossing the plateau of Shan-si, he was enabled to supplement to some extent the observations of Richthofen; namely, he has discovered some fossil plants in the middle parts of the series of deposits which cover in China the carboniferous formation, and which Richthofen had described under the names of *Ueberkohlen-sandsteine* or *Plateau-sandsteine*. The plants unearthed by Mr. Obrucheff would indicate that the middle portions of this formation belong to the Mesozoic age, and are Triassic or Liassic. This formation spreads from Shan-si into the Shensi, the Alashan, and Gan-su, without losing in thickness, and probably represents an uninterrupted series of deposits from both the Mesozoic and the first half of the Cainozoic times.

WE notice in the *Memoirs* (*Trudy*) of the Kazan Society of Naturalists (vol. xxvi. No. 2) a very interesting work by N. Wnukow, on the bacilli of leprosy. In addition to his own experimental researches, the author has carefully studied the West European and Russian literature of the subject, and has divided his memoir into three parts: the localisation of leprosy bacilli in the tissues of the human body; the inoculation of the bacilli to animals; and the artificial culture of the bacilli. The paper is accompanied by a coloured plate. The author's conclusions are:—The *Bacillus lepræ* is motile, and is found both within and outside the cells; but it has never been discovered in the cells of the epithelial layers of the skin or the mucous membranes. In the wounds the bacilli are brought to the surface, and undoubtedly may be transported on the skin of other individuals, thus becoming a cause of infection. Neither the injection of the pus containing leprosy bacilli, nor the grafting of pieces of skin taken from leprous patients to rabbits, could provoke leprosy in these animals. The bacilli introduced from man into rabbits and fishes, diminish in numbers after a time, and ultimately disappear. Most inoculated rabbits contract tuberculosis, but the illness must be ascribed in such cases to other causes than infection proper. As to the artificial culture of the *Bacillus lepræ*, it has failed with all culture media experimented upon by the author; the culture of *B. Uffreduzzi*, described by Eisenberg as leprosic, cannot be recognised as such.

AN elaborate paper, entitled "Les Vibrions des Eaux et l'Etiologie du Choléra," by Dr. Sanarelli, has recently appeared in the *Annales de l'Institut Pasteur*, vol. vii. Numerous bacteriological examinations were made of the river Seine water above and below Paris, as well as of drain water, and the effluent of sewage after irrigation. In all no less than thirty-two vibrios were isolated, morphologically distinct, four of which gave the indol reaction, and in their pathogenic action on guinea-pigs could not be distinguished from the cholera-bacillus. Dr. Sanarelli is of opinion that there exist many varieties of vibrios, morphologically distinguishable, but capable of exciting in man and animals a disease in its morbid and clinical aspects identical with those regarded as typical of cholera, and that the conception of a restricted monomorphism is no longer tenable in the diagnosis of the cholera-vibrio. In all the more or less contaminated waters which were examined vibrios were present, finding in these surroundings conditions highly favourable to their existence and multiplication. It is possible that although the larger number of such vibrios may exist in the saprophytic or harmless state, yet probably pathogenic vibrios are more frequently present in such waters than has hitherto been suspected. Dr. Sanarelli points out that the saprophytic condition of some at least of these vibrios is, in all probability, due to the modification in and attenuation of their biological functions which residence in such media has produced. Thus an extremely virulent vibrio was reduced to a harmless saprophyte deprived of its pathogenic properties and power of producing the indol reaction, by being kept in boiled Seine water for a month, whilst even after three months it had undergone no change in its morphological condition. In the same manner that pathogenic organisms may be deprived of their virulence, it is conceivable that circumstances may arise under which they may recover their toxic character; so far, however, bacteriology has been unable to establish the correctness of this hypothesis, either in the laboratory or in actual experience.

FOR several years the State of Massachusetts has been attempting to exterminate the Gipsy Moth, and a Bill has recently been introduced into the House of Representatives to appropriate 100,000 dollars to rid the State of that troublesome insect. The *American Naturalist* points out, however, that the desired end can never be attained by merely hunting the moths in trees, hedgerows, and garden patches. In its future work, the Gipsy Moth Commission of Massachusetts should employ at its head a trained entomologist who should devote his time to finding and introducing some natural enemy to the pest. Moths, eggs, larvæ, and cocoons will escape the most careful of field agents, whereas insect parasites will keep the pest in continual check.

MESSRS. T. D. A. COCKERELL AND WALTER E. COLLINGE have published "A Check-list of the Slugs." It is a reprint from the *Conchologist*, vol. ii., 1893. The authority for the list is the first-named author; Mr. Collinge adds an appendix and notes; 628 species are recorded with very numerous varieties. There would appear to be a very ardent discussion as to the respective value of morphological and anatomical characters for the due determination of the species and varieties among these molluscs; but surely here, as elsewhere, the rational method would be to employ all such points of difference, whether external or internal, as may be found constant.

THE Association for the Promotion of Home and Foreign Travel has issued a programme of tours arranged for this winter.

THE December number of *Insect Life* is almost entirely taken up with the proceedings of the meeting of the Association of Economic Entomologists, held at Madison in August last.

MR. C. MELDRUM, the Director of the Royal Alfred Observatory, Mauritius, has issued his report for the year 1891, and also the results of meteorological observations made at the Observatory during 1892.

A PAPER on "Technical Education in Glasgow and the West of Scotland: a Retrospect and a Prospect" read before the Philosophical Society of Glasgow in November last, by Dr. Henry Dyer, has been issued in pamphlet form. It is of interest to all concerned with matters of technical instruction.

MR. W. WARDE FOWLER, a disciple of Gilbert White, has put on record his observations of the Marsh Warbler (*Acrocephalus palustris*) in Oxfordshire and Switzerland, and the differences between it and the Reed Warbler. His paper (issued by Simpkin, Marshall, & Co.) will be read with pleasure by all lovers of nature.

THE number just issued of the Journal of the Royal Agricultural Society (vol. iv. part 4) contains several important articles. Mr. Carruthers describes the "Cross-fertilisation of Cereals," and his paper is given additional interest by means of seven good illustrations. "Water in Relation to Health and Disease" is treated by Prof. J. Wortley Axe, and under the title "Peat and its Products," Dr. Fream gives an account of the occurrence and utilisation of peat in various peat-producing countries of Europe.

MESSRS. BAILLIÈRE AND SON have recently added to their series of works on chemical industries a volume entitled "Le

Cuivre," by M. Paul Weiss, in which the origin, mode of occurrence, properties, metallurgy, applications, and alloys of copper are fully treated. The author has visited the chief copper mines and works in Europe, and his book is a very useful *résumé* of the fundamental principles of the copper industry. The ninety-six figures inserted in the text include twelve excellent sections illustrating the molecular structure of various metals and alloys.

THE structure of snow-crystals photographed by G. Nordenskiöld formed the subject of an article in our last volume. Another important contribution to the subject has recently been published, namely, Prof. G. Hellmann's "Schneekrystalle" (Rudolf Mückenberger, Berlin). The work begins with a brief history of the study of snow-crystals, illustrated by reproductions of the various forms observed and drawn by different observers, from the spikes, crescents, and daggers of Magnus in the sixteenth century, to the elaborate and perfectly symmetrical stars designed by Glaisher. But in meteorology as in astronomy, photography is rapidly taking the place of the observer; so much, indeed, is this the case that the modern meteorologist and physical astronomer views visual observations with more or less suspicion. At any rate, the remarkably fine series of microphotographs of snow-crystals obtained by Dr. Neuhauss during the winter 1892-3, and reproduced in Prof. Hellmann's work, indicates that eye-observations of their forms are no longer necessary. After discussing the structure of snow-crystals, Prof. Hellmann proposes a classification into tabular and columnar crystals, the former class being subdivided into radiating stars, plates, and a combination of the two, and the latter into prisms and pyramids. A descriptive bibliography is given, thus increasing the value of a work upon a subject of which much more can yet be said.

THE cause of the violent explosion which usually occurs when any considerable quantity of metallic sodium is brought in contact with water in a more or less confined space, forms the subject of a communication to the *Journal für praktische Chemie*, by Prof. Rosenfeld. It has been hitherto supposed to be due to the formation of a quantity of sodium peroxide, by the decomposition of which oxygen is liberated, which mixes with the hydrogen produced in the main reaction, thus forming an explosive mixture. Prof. Rosenfeld has fully investigated the question experimentally. It was first established that steam may with impunity be passed over sodium contained in a slightly bent iron tube, no explosion ever occurring under these conditions. This would be quite compatible with the above explanation of the cause of the explosions, for any explosive mixture would be rapidly carried from the seat of the reaction by the escaping hydrogen or the excess of water vapour. No oxygen, however, was ever detected in the gas thus liberated. In all the experiments in which explosion was brought about by the action of water, whether in open vessels or in vessels closed by a water column, it was invariably observed that the sodium was blown to powder from the centre outwards—that is to say, the seat of the explosion was the interior of the piece of metal experimented with. Prof. Rosenfeld comes to the conclusion, from the whole of the phenomena observed, that the explosion is brought about by the sudden dissociation of a hydride of sodium which is formed in the first stage of the reaction. As such a compound can only be produced in an atmosphere of hydrogen, the only safe mode of decomposing water by metallic sodium is considered to be that previously mentioned, of passing a rapid current of steam over the metal; for the hydrogen is then removed from the sodium as quickly as it is produced, and the formation of hydride, and therefore all risk of explosion, is consequently avoided. In order to carry out this reaction an iron crucible is best employed which is capable of being closed in a gas-tight manner by means of an iron plate, which can be pressed firmly down against a flange on the edge of the crucible by means of a screw threading through a suitably supported nut. Steam is blown into the body of the crucible containing the sodium by means of a side tube, and the escaping hydrogen is led away by a similar tubulus upon the other side. If the supply of steam is arrested the moment hydrogen ceases to escape, solid caustic soda is obtained, mixed in a curious manner with more or less finely divided iron, probably owing to the formation of a quantity of an alloy of iron and sodium, which is subsequently decomposed with liberation of iron. Silver is likewise attacked in a similar manner. The method may also be employed to prepare solutions of soda of known strength. Thus, if twenty-three grams of sodium are employed, and the escaping hydrogen is washed through a little water, an exactly normal solution of soda can at once be obtained by dissolving the product in water, adding the wash water, and making up to a litre.

AN interesting investigation of the amount and nature of the gases occluded in the coal derived from several collieries in the Durham coal field has been carried out by Mr. W. McConnell, of the Durham College of Science. The collieries from which samples were taken are situate at different points along the same seam, known in Durham as the Hutton seam. It is bituminous coal used as gas-coal and as steam-coal. The coal or coal-dust was placed in an apparatus constructed entirely of glass, and which was capable of continuous exhaustion while heated in baths to known temperatures varying from 100° to 180°; the gas previously occluded by the coal was delivered by the pump into a receiving gas-holder, and subsequently measured and analysed. The coals from the Ryhope colliery were found to contain as combustible gases considerable quantities of occluded free hydrogen, marsh gas, ethane, and other members of the paraffin series of hydrocarbons as far as pentane. Moreover, a portion of the gas, consisting chiefly of the higher members of the paraffins and smaller quantities of olefines, is so firmly retained that crushing to fine powder and heating to 180° under reduced pressure is insufficient to remove it. It is also singular that the coal retains a remarkably high proportion of free oxygen in the occluded form, even after heating to 180°. In the case of the Hebburn colliery, a notably "gassy" mine, in which frequent "blowers" are met with, the results are especially interesting. The "blowers" deliver such large quantities of gas that some of it is actually "piped" up to the bank and burnt under the boilers. The combustible constituent of the gas thus utilised is found to be entirely marsh gas. The coal itself is found to contain a relatively very large volume of occluded gas, the combustible constituents being mainly marsh gas and ethane; and the ground coal and coal-dust yield in addition considerable quantities of higher members of the paraffin series. From the whole of the results derived from the various collieries, there can be no doubt that the coal-dust largely owes its sensitiveness to ignition to the denser occluded gaseous hydrocarbons which it retains so tenaciously.

THE additions to the Zoological Society's Gardens during the past week include a Leopard (*Felis pardus*, ♂, black variety), from India, presented by the Duke of Newcastle; a Herring Gull (*Larus argentatus*) from Jersey, presented by Mr. John Stanton; an Alligator (*Alligator mississippiensis*) from the Mississippi, presented by Mr. C. Knox Shaw; a Diamond Snake (*Morelia spilotes*) from Australia, presented by Commander A. Burgess, R.N.R.; a Diamond Snake (*Morelia spilotes*) from Australia, purchased.

OUR ASTRONOMICAL COLUMN.

PRIZES AT THE PARIS ACADEMY.—Among the numerous prizes presented by the Paris Academy (*Comptes Rendus*, No. 25, Dec. 18), those devoted to the science of astronomy were as follows:—M. Schulhof, the Lalande Prize, for his magnificent researches on comets; Dr. Berberich, the Valz Prize, for his well-known connection with the calculations of cometary and (minor) planetary orbits; and Prof. Langley, the Janssen Prize, for the work he has done relating to the distribution of the heat in the normal solar spectrum, and to the influence exerted on this distribution by both the solar and terrestrial atmospheres. Among the general prizes we notice that the Arago medal has been awarded to two American astronomers, Profs. Asaph Hall and Barnard. The former receives this medal as he was the discoverer of the two satellites of Mars, although on a former occasion he was the recipient of the Lalande prize for the same reason. The latter, it is needless to say, owes this honour to the fine use he made of the great 36-inch telescope of the Lick Observatory, in searching out the fifth, or, as it should be named, the first satellite of Jupiter.

THE TAIL OF COMET BROOKS (*c* 1893).—Last week, under this heading, we referred to Prof Barnard's remark that the fall of this comet had encountered some outside or obstructive medium. It is interesting, in the face of this, to look at the drawings of the great comet of 1882, and to notice the fragments and their relative positions and forms. With the drawing before us (Young's "Astronomy," 1888, p. 427) the following description is given:—"Besides this" (referring to that curious phenomenon called the *sheath*) "at different times, three or four irregular shreds of cometary matter were detected by Schmidt, of Athens, and other observers, accompanying the comet at a distance of three or four degrees when first seen, but gradually receding from it, and at the same time growing fainter. Possibly they may have been fragments of the tail which belonged to the comet before passing perihelion, or of the matter repelled from the comet when near perihelion. Since the comet, in passing the perihelion, changed the direction of its motion by nearly 180° in less than three hours, it was, of course, physically impossible that the tail it had before the perihelion passage could have made the circuit of the sun in that time. . . . Visible or invisible, the particles of the old train must have kept on their way under the combined action of the sun's gravitation and repulsion. . . ." Would not a more simple explanation in this case be that these fragments were the result of collisions near perihelion passage, for here most certainly we should expect to be in the presence of meteoritic matter in abundance, and these travelling at high speed?

THE PLANET VENUS.—This planet, which forms such a brilliant object in the evening sky, will during this month become brighter, reaching its maximum brilliancy on the 10th of January. For observers in northern latitudes its position is becoming more favourable for observation, owing to its movement northward in declination. A conjunction with the moon takes place on the 10th of January, so that about the day before and after that date these two bodies will form a striking pair.

GEOGRAPHICAL NOTES.

WITH the first of January the weekly South German geographical paper, *Das Ausland*, edited by Dr. Sigimund Günther, of Munich, and established as long ago as 1827, comes to an end, having sunk its identity by amalgamating with *Globus*, which for thirty-two years has been its North German contemporary and rival. *Globus* will continue to be published, with numerous illustrations, as heretofore, and with the additional attraction of Roman type being substituted for the old German character. It is somewhat remarkable that weekly papers of this kind, entirely devoted to geography and travel, with no political purpose, should be so thoroughly established in Germany and France, while no successful attempt has ever been made in an English-speaking country to start a similar publication.

THE Russian geologist, W. A. Obrucheff, who started in the early part of 1893 for a journey into the little known region of Ordos, lying in the great bend of the Hoang-ho, has (says *Globus*) been able to make many new observations. Leaving Tai-Yuen-fu, the farthest point reached by Richthofen in this direction, on January 18, and crossing the Hoang-ho on the ice on the 28th, he selected the route to Ning-hsia, across the south-western edge of Ordos, as the least known, with the intention of proceeding to study the mountains of Alashan and the left bank of the Hoang-ho, up to the Nan-shan range. On his way Obrucheff was able to throw some light on the hills between the plateau of Shan-si and Kansu, and the plain of Ordos, which he found to be only the denuded edge of the plateau, and in no sense a range. The portion of Ordos which he intended to cross is a blank on all maps, and the whole district in the great bend of the Hoang-ho north of the Great Wall is practically unknown territory.

THE last number of the *Verhandlungen* of the Berlin Geographical Society contains a short note on a journey to Hadramaut undertaken last year by a German explorer named Hirsch, whose experience gives some clue to the difficulties now being encountered by Mr. and Mrs. Bent. At the outset Herr Hirsch met with opposition from the British Resident at Aden, but overcoming this he reached Makalla and started for the interior, with two camels and a small party, on July 1. He ascended the Wadi Howere to the great plateau, and crossed the watershed at an elevation exceeding 6,000 feet. From the barren plateau Hirsch descended to the fruitful and populous Hadramaut valley, several of the towns of which were visited. At Terim he was very badly received, subjected to insults, and compelled to leave at very short notice, returning to Makalla through the scarcely known Wadis Bin Ali and Odym. Altogether the journey in the interior only lasted forty days, but observations of considerable value were made, which are now being prepared for publication.

A REMARKABLE discovery has been announced by the Austrian Institute for Historical Research, in the form of a copy of a map by Columbus, drawn on a letter written from Jamaica in July, 1503. This, although only a rough pen-and-ink sketch, shows exactly the opinion of Columbus himself as to the part of the world he had reached, which he believed to be the east coast of Asia. The original map, drawn by Columbus and his brother Bartholomew, was presented to Frate Hieronymo, who gave the map and a description to Alexander Strozzi, a noted collector of early voyages. He is supposed to have copied the original map on the margin of the letter of Columbus, which he had bound in a volume with other documents, and this volume is now in the National Library at Florence, where the existence of the map was discovered by Dr. R. v. Wieser, the Professor of Geography at Innsbruck.

NEW FRENCH LAW FOR THE PREVENTION OF FOREST FIRES.[1]

THE wooded tract of country comprising the hill ranges of Les Maures and l'Esterel in the departments of Le Var and Les Alpes maritimes, in the south-east of France, has been annually ravaged by forest fires from time immemorial. It is stocked with conifers, *Pinus Halepensis*, and *P. Pinaster*; the cork oak, and the pubescent variety of *Quercus sessiliflora*, and there is a dense undergrowth of *Erica arborea*, the roots of which are used for briar (*bruyère*) pipes, also of *Erica scoparia*, lavender, juniper, broom, dwarf palms, wild olive, and *Arbutus*, &c. During the months of June, July, August, and September, the drought, high temperature, and the violent *mistral* wind which prevail, increase the danger from forest fires and their severity.

Owing to the great destruction of property which these fires cause, a law was enacted in 1870, to be in force for twenty years, and has given excellent results, the frequency and extent of forest fires in the region having diminished by half during the period 1870–90. This law was renewed up to the present time, in order to allow Government to draw up a permanent law on the subject. The Minister of Agriculture accordingly drafted a bill, which, after consideration by a Committee of the Chamber of Deputies, and some unimportant amendments, was passed by the legislature, and received the consent of the President of the Republic, as a law, on August 19, 1893.

The principal clauses of the Act deal with methods of prevention and extinction of fires: thus the first clause prohibits, during the dangerous season above mentioned, all fires in forests

[1] The text of this Law is given in the *Revue des Eaux et Forêts*, vol xix. part 18, for September 25, 1893.

or shrubby waste lands, or within a distance of 200 metres from their boundaries. The period during which these fires were declared illegal by the former Act of 1870, was fixed annually by the prefects, but experience has shown that it can now be fixed once for all by the law. As exceptions to this law, Clause 2 also authorises the prefects to allow charcoal-makers and other woodmen to light fires at their own risk, in case of damage arising, and subject to certain rules made by the prefects.

Among the fires prohibited during the close season is the so-called *petit feu*,[1] by which strips of undergrowth were carefully burned every six or seven years in the cork forests, to save the valuable cork oak trees from more dangerous uncontrolled fires. This system costs only 3*s.* 6*d.* an acre, as compared with £4 an acre for uprooting the dangerous undergrowth. It is evidently more hurtful to the forest than the other method, as the fire occasionally gets out of control, and, in any case, the burning diminishes the fertility of the soil.

The ninth clause directs that all landed proprietors, whose land has not been entirely cleared of all woody growth, may be compelled by an adjoining proprietor to keep a strip of land between the two estates entirely free from shrubs or conifers. The breadth of this strip will vary, according to circumstances, between 20 and 50 metres.

It is further enacted in Clause 11 that similar bare strips 20 metres broad shall be kept up along all lines of railway through a wooded area, and that these strips in adjoining property shall be kept clear at the expense of the railway companies. As it may not always be necessary to keep up these fire lines along the railways, a committee, consisting of a departmental councillor (*conseiller général*), a forest officer, and a railway engineer, shall decide when they may be omitted. All proprietors, whose woods are cut down in clearing these strips, are to obtain indemnities. This is a new provision, and called for owing to the extension of railways. The Act looks to the future in a clause exempting railway companies from this liability if they should use electric motors, or other inventions which cannot cause a forest fire.

In case any fire should break out, and it may appear advisable to light a counter fire, the two fires meeting and extinguishing one another for want of inflammable material, the local mayor, or his deputy, or failing these the most senior forest officer present, is to take charge of all measures to extinguish the fire, and no indemnity arises for woods burned under such circumstances. As in India, it is found in the south-east of France that fire is frequently caused by sportsmen, or poachers during the dry season, and the prefect is therefore authorised to delay the commencement of the shooting season until the commencement of the rains, which generally happens before the end of September.

As it is found that the construction of a network of roads greatly facilitates fire protection, by giving more value to forest produce, and rendering it possible to transport the material cleared from fire lines, and as roads serve as lines from which counter fires may be started, the State offers a subvention of 3000 francs per kilometre (£200 per mile) for roads constructed in the district, up to a total outlay of 600,000 francs (£24,000).

It appears that since 1870, 479,000 francs (£19,160) have been spent by the State on new roads in the State forests of the Esterel. The penalties attached to the breach of the first clause of this law are one to five days' imprisonment, or fine of 20 to 500 francs, and both fine and imprisonment can be inflicted, so that magistrates can make the penalty proportional to the gravity of the offence, and all police, forest guards, whether belonging to the State or to private properties, are directed to carry out the law by reporting offences, their written statements being received as evidence in cases which may arise. If the railway companies do not clear the fire lines along the railways, these lines will be cleared at their expense by the French Forest Department.

Although much land which might otherwise be planted is wasted in England owing to heather fires, and not only is a large area of pine forest destroyed annually by fire, but also the increase of destructive pine beetles is thus greatly favoured, there is little hope of our Legislature interfering; but the matter is more serious in North America, and along the Northern Pacific Railway about 1000 miles of treeless country exists, where the forests have been destroyed by fires, whilst the immensely valuable pitch-pine forests of the Southern States are rapidly disappearing from the same cause.

Matters have been dealt with in British India much more prudently, and regulations against forest fires have been enacted for the last twenty years at least in all the provinces under our control, and also to a certain extent within the native States. As a result of these regulations, and the careful management of the Indian Forest Department, 23,144 square miles of State forest in India were protected from fire in 1891 at a cost of 9 rupees per square mile, and this in addition to large areas of evergreen forest where no danger from forest fires exists.

W. R. FISHER.

[1] *Vide* "A Forest Tour in Provence and the Cevennes," by Colonel Bailey, R.E., in Transactions of the Botanical Society of Edinburgh, vol. xvi. part 3, 1886.

PRIZE SUBJECTS OF THE PARIS ACADEMY OF SCIENCES.

THE following are the subjects for which prizes will be awarded by the Paris Academy in the years 1894, 1895, 1896, and 1898:—

1894. *Grand Priz for Mathematical Sciences*—The developmen, of an important point in connection with the deformation of surfaces. *Priz Bordin*—The study of problems in analytical mechanics admitting of algebraic integrals with regard to velocities, and especially quadratic integrals. *Priz Francoeur*—Discoveries or useful works on the progress of pure and applied mathematical sciences. *Priz Poncelet*—To the author of the most useful work on the progress of pure and applied mathematical sciences. *Extraordinary Prize of six thousand francs*—For any work tending to increase the efficacy of French naval forces. *Priz Montyon*—Mechanics. *Priz Plumey*—To the author of an improvement of steam engines or any other invention which promotes the advance of steam navigation. *Priz Dalmont*—To the engineer of bridges and highways who presents the best work to the Academy. *Priz Lalande*—Astronomy. *Priz Damoiseau*—Improvement of the method of calculating the perturbations of minor planets so as to give their positions within a few minutes of arc for an interval of fifty years; also the construction of tables which allow the principal parts of the perturbations to be rapidly determined. *Priz Valz*—Astronomy. *Priz Janssen*—Astronomical physics. *Priz Montyon*—Statistics. *Priz Jecker*—Organic chemistry. *Priz Vaillant*—Study of the physical and chemical causes determining the existence of rotatory power in transparent bodies, especially from the experimental point of view. *Priz Desmazières*—To the author of the most useful work on all or part of the cryptogams. *Priz Montagne*—To the authors of important works having for their subject the anatomy, physiology, development, or description of the lower cryptogams. *Priz Thore*—Awarded alternately to works on the cellular cryptogams of Europe, and to researches on the habits or anatomy of a species of European insect. *Priz Savigny*—To young zoological explorers. *Priz da Gama Machado*—On the coloured parts of the integumentary system of animals, and on the fertilising matter of living things. *Priz Montyon*—Medicine and surgery. *Priz Breant*—For a means of curing Asiatic cholera. *Priz Godard*—The anatomy, physiology, and pathology of genito-urinary organs. *Priz Parkin*—Researches on the curative effects of carbon in its various forms, and more especially in the gaseous form of carbon dioxide, in cholera, different kinds of fever, and other ailments. *Priz Barbier*—For a useful discovery in surgery, medicine, pharmacy, or botany in connection with the art of healing. *Priz Lallemand*—For the recompensation or encouragement of works relating to the nervous system, accepting the widest meaning of these words. *Priz Bellion*—To the writers of works or discoverers of facts of special importance to the health of human beings or the improvement of mankind. *Priz Mège*—For the completion of Dr. Mège's essay on the causes that have retarded or favoured the progress of medicine. *Priz Montyon*—Experimental physiology. *Priz Pourat*—On the influence exercised by the pancreas and suprarenal capsules on the nervous system, and reciprocally, on the influence that the nervous system exercises on these glands, studied especially from a physiological point of view. *Priz Gay*—The study of subterranean waters; their origin, direction, the strata they traverse, their composition, and the animal and vegetable life that live in them. *Priz Montyon*—Unhealthy occupations. *Priz Cuvier*—For the most remarkable work on the animal kingdom, or on geology. *Priz Trémont*—To the savant, artist, or mechanic requiring assistance to attain an object of use or benefit to France. *Priz Gegner*—For the assistance of the savant distinguished for his contributions to the positive sciences. *Priz Delalande-Guérineau*—To the young French explorer, or the man of science, who shall have rendered the greatest service to France or science. *Priz Jerome Ponti*—To the author of scientific work of which the continuation or development is important to science. *Priz Tchihatchen*—To the naturalist of any nationality who shall have pursued explorations in the Asiatic continent or neighbouring islands, having for their object the advancement of any branch of natural, physical, or mathematical science. *Priz Houllevigue*—Awarded in rotation by the Academy of Sciences, and by the Academy of Fine Arts. *Priz Cahours*—For the encouragement of young workers known for their interesting researches, and more especially for researches in chemistry. *Priz Alberto-Lévy*—For a means of preventing or curing diphtheria. *Priz Laplace*—To the head student of the Ecole Polytechnique.

1895. For the improvement of the theory of the relation between the flywheel and the regulator. *Priz Gay*—For a study of the régimé of rain and snow over the whole surface of the earth. *Priz L. La Caze*—To the authors of the best work on physics, chemistry, and physiology. *Priz Delesse*—To the author of a work dealing with geology or mineralogy. *Priz Bordin*—For the memoir that adds most to the knowledge of natural history (zoology, botany, or geology) of Tonkin or one of the French possessions in Central Africa. *Grand Priz des Sciences Physiques*—For the work that contributes most to the advancement of French palæontology by dealing in a thorough manner with the vertebrata of the coal measures, and those of the secondary epoch, and comparing them with existing types. *Priz Chaussièr*—For important works in legal or in practical medicine. *Priz Petit d'Ormoy*—Pure and applied mathematics or natural science. *Priz Leconte*—To be awarded (1) to the authors of new and important discoveries in mathematics, physics, chemistry, natural history, or medical sciences; (2) to the authors of new applications in these sciences. *Priz Gaston Planté*—To the French author of a discovery, invention, or important work in the domain of electricity.

1896. *Priz Janssen*—Astronomical physics. *Priz Serres*—On general embryology, applied as far as possible to physiology and medicine. *Priz Jean Renaud*—For the best work published during the preceding five years.

1898. *Priz Damoiseau*—For a development of the theory of the perturbations of Hyperion, the satellite of Saturn discovered simultaneously by Bond and Lassell in 1848, principally taking into account the action of Titan. Also to compare observation with theory, and thence deduce the mass of Titan.

SCIENCE IN THE MAGAZINES.

PROF. J. W. JUDD contributes to the *Fortnightly* an article on "The Chemical Action of Marine Organisms," dealing with the nature of the ocean-floor, and showing that the operations going on there are similar to those described by Darwin in his work on vegetable mould and earthworms. Prof. Judd favours the organic view of the origin of manganese nodules, and believes that the chemical theory is improbable. He says:—

"All the facts collected by the deep-sea exploring expeditions point to the conclusion that accumulation of material is going on with the most extreme slowness at these abysmal depths where the manganese nodules are found in greatest abundance, and it may well be that these slowly accumulating muds have been passed through the bodies of marine worms or other organisms an almost infinite number of times. At each passage of the clay through the organism a small addition of manganese and iron oxides would be made to the mass by the action of the living structure on the sea water, and thus in the course of time these oxides might be sufficiently concentrated to build up, by concretionary action, the remarkable nodules on the ocean-bed.

"Such action would be in complete analogy with processes going on both in fresh and salt water, by which calcareous, silicious, phosphatic, and ferruginous deposits are being everywhere formed in the waters of the ocean, while all theories of the direct separation of the manganese and rarer metals from their state of excessively dilute solution in sea-water by chemical reactions appear to me to be beset with the greatest difficulty. All the observations that have been made in recent years upon the deposits of the ocean-floor point to one conclusion, namely, that where materials have once passed into a state of solution in the waters of the sea they can only be separated from it in the open ocean by the wonderful action of living organisms."

Prof. Buechner discusses "The Origin of Mankind," his article being more or less a review of a pamphlet by Abel Hovelacque, entitled "Les débuts de l'humanité," in which the results derived from archæological researches are compared with the observations of travellers as to the lowest types of the human family that can exist. Captain Gambier, R.N., writes on "The True Discovery of America." He shows that Jean Cousin, a sea-captain of Dieppe, discovered the River Amazon in 1488, that is, four years before Columbus discovered San Salvador. There is clear evidence that Cousin was thoroughly conversant with all that was known of geography, hydrography, and nautical astronomy in his day, and that he sailed up the Maragnon, which was his name for the River Amazon as he heard it from the natives. On board Cousin's ship, as second in command, was a man named Vincent Pinçon, and Captain Gambier's contention is that this Pinçon was the same man as the Vincent Pinçon who is known to have commanded one of the ships under Columbus. The Pinçon that sailed with Cousin was tried by court-martial for insubordination when the ship returned to Dieppe, and was condemned to perpetual banishment from the soil of France. He went to Genoa, and from there to Palos, in Andalusia, where his two brothers carried on the business of shipowners and traders, making occasional voyages themselves. It is not too much to suppose that Columbus met the Pinçons, and was indebted to them for information about Cousin's voyage. Jealousy and human self-interest will explain why Cousin's name was carefully omitted from all writings referring to the discovery that was afterwards made by Columbus and the three Pinçons who accompanied him on the celebrated voyage.

In addition to these articles, the *Fortnightly* contains one in which Dr. Thin comments upon the most important points brought out in the Report of the Leprosy Commission in India.

The *New Review*, which appears for the first time this month as an illustrated review, contains an article on the late Prof. Tyndall, by Mr. P. Chalmers Mitchell. Sir William Flower contributes to *Good Words* an excellent description of the structure and action of "Birds' Wings." "The Vanishing Moose and their Extermination in the Adirondacks" is the title of a well-illustrated article by Mr. Madisson Grant in the *Century*. Mr. Grant says that the last moose in New York State was killed on the east inlet of Raquette Lake, in the autumn of 1861. In the *Century* also is related the circumstances that led to the first employment of chloroform, in 1847, by Sir James Simpson, the scribe being his daughter, Miss E. B. Simpson. Since chloroform may soon be superseded by some newer anæsthetic, it is well that the events which established it as the great alleviator of animal suffering have been recorded. Other magazines received by us are the *Humanitarian*, which reprints the address on "Biology and Ethics," recently delivered by Sir James Crichton Browne at Sheffield, *Scribner*, the *National Review*, *Contemporary*, the *Modern Review*, and *Longman's*, but none of these contain any articles of scientific interest.

THE RISE OF THE MAMMALIA IN NORTH AMERICA.

I.

IN a remarkable address delivered before the Zoological section of the American Association for the Advancement of Science, at the Madison meeting, in August, Prof. H. F. Osborn gave an account of the recent achievements of exploration and research in connection with the rise of the mammalia in North America, and suggested the lines along which further advances were desirable. The length of the address precludes its complete publication here, but the most important features will be found in the following extracts. Among the omitted portions are sections dealing with the origin and evolution of Trituberculism, the succession of the Perissodactyls, and that of the Artiodactyls, and the relation of the Ancyclopoda (Cope) to the law of correlation.

Twenty years ago an era opened in the mammalian palæontology of Europe and America. Partly inspired by the *Odontographie* of Rütimeyer, Kowalevsky completed and published in 1873 his four remarkable memoirs upon the hoofed mammals. He wrote these four hundred and fifty quarto pages in three languages not his own, in French upon *Anchitherium* and the ancestry of the horses, in English upon the *Hyopotamidæ*, in German upon *Gelocus*, *Anthracotherium* and *Entelodon*, including the first attempt at an arrangement of a great group of mammals upon the basis of the descent theory. These memoirs swept aside all the dry traditional fossil lore of Europe; they breathed the new spirit of Darwin, to whom the chief one was dedicated, making principles of descent of more importance than new genera and species. Kowalevsky thus summed up the contemporary palæontology:

"After the splendid osteological investigations of Cuvier had revealed to science a glimpse of a new mammalian world of wonderful richness, his successors have been bent rather upon multiplying the diversity of this extinct creation, than on diligently studying the organisation of the fossil forms that successively turned up through the zeal of amateurs and collectors. . . . With the exception of England (referring to Owen, Huxley, Falconer, and others), where the study of fossil mammalia was founded on a sound basis, and some glorious exceptions on the Continent (referring to Rütimeyer, Gaudry, Fraas, Milne-Edwards), we have very few good palæontological memoirs in which the osteology of extinct mammals has been treated with sufficient detail and discrimination; and things have come to such a pass, that we know far better the osteology of South American, Australian, and Asiatic genera of fossil mammals than of those found in Europe."

At the same time, between 1871 and 1873, the pioneers of American palæontology, Leidy, Marsh, and Cope, began the exploration of our ancient lake basins rich in life. The first ten years of their work not only revolutionised our ideas of mammalian descent, but brought together the data for the generalisations of the second decade; for Marsh's demonstration of the laws of brain evolution in relation to survival; for Cope's proof of ungulate derivation from types with the simple foot resting upon the sole, and with the ancestral conic or bunodont molar tooth; and finally for Cope's demonstration of the tritubercular molar as the central type in all the mammalia. These four generalisations furnished a new working basis for morphology and phylogeny.

In these twenty years, thanks to energetic field work, we have accumulated vast materials for the history of the rise of the mammalia, enough for ten students where there is one, and the questions arise: How shall we take best advantage of it? What methods shall we adopt? In this address, besides bringing before you the more recent achievements of exploration and research, I will try to illustrate the advances already made in lines of thought, observation and system in palæontology, and indicate other advances which seem to me still desirable. In the problem of how to think and work most effectively, and with most permanent results, all the sciences meet on common ground.

It is to the renown of the veteran Rütimeyer and of Kowalevsky, whose death we have to deplore, that, while their main inductions suffer by American discoveries, their methods of thought have not been displaced. It matters little that their theory that ungulate molars sprang from lophodont or crested forms, has been disproved; that Kowalevsky's tables of descent are full of errors; that his main generalisation as to the persistence of adaptive and extinction of inadaptive foot types does not hold good; that the horses and Anchitherium spring not from Palæotherium as he supposed, but from *Pachynolophus* and *Hyracotherium*, types which he carefully studied and yet omitted from the horse line! It is the right system of thought which is most essential to progress; better in the end wrong results such as the above, reached by the right method, than right results reached haphazard by a vicious method. If a student asked me how to study palæontology, I could do no better than direct him to the *Versuch einer natürlichen Classification der fossilen Hufthiere*, out of date in its facts, thoroughly modern in its approach to ancient nature. This work is a model union of the detailed study of form and function with theory and the working hypothesis. It regards the fossil not as a petrified skeleton, but as moving and feeding; every joint and facet has a meaning, each cusp a certain significance. Rising to the philosophy of the matter, it brings the mechanical perfection and adaptiveness of different types into relation with environment, the change of herbage, the introduction of grasses. In this connection it speculates upon the causes of the rise, spread and extiuction of each animal group. In other words the fossil quadrupeds are treated *biologically*—so far as is possible in the obscurity of the past. From such models and from our own experience we learn to abandon such traditions in the use of the tools of science as mere methods of description and classification, and to regard priority only in the matter of nomenclature.

To illustrate some of these modern methods, let us first look at the evolution of the teeth in the rise of the mammalia. The teeth and the feet are the foci of mammalian evolution, the only direct points of contact with food and the earth. Their combined use in phylogeny has increased in interest, because their evolution has proved to be wholly independent. We recall Cuvier's famous claim, of which Balzac said at the time: "Rebuilt like Cadmus, cities from a tooth."

No generalisation has been more thoroughly routed than that of a necessary law of correlation between tooth and foot structure. Besides the orthodox clawed carnivores and hoofed pachyderms of the great French anatomist, we have discovered hoofed carnivores such as Mesonyx, and clawed pachyderms such as Chalicotherium. Even the apparently lasting barriers of correlation, which Owen raised between the even and odd-toed ungulates, have broken down by Ameghino's discovery of a Litoptern odd-toed horse with an even-toed type of astragalus. Not only is there no correlation of type, but none in the rate of evolution. Hipparion, the most progressive horse in tooth-structure, probably owed its extinction to its conservative preservation of its ancestral three toes. For these reasons the teeth and feet, owing to the frequent parallels of adaptation, may wholly mislead us if taken alone; while, if considered together, they give us a sure key; for no case of exact parallelism in both teeth and feet between two unrelated types has yet been found, or is likely to be. This, I believe, is the one lesson of later work which reverts to older methods; we should not base either classification or descent upon the teeth or feet alone. Every additional character diminishes the chances of error.

Lower Mesozoic Pro-mammalia.

With the exception of the triassic *Theriodesmus* of Seeley, no mammal is known by its limbs or skeleton until we reach the basal Eocene; in studying the first steps in the rise of the mammalia, we are thus practically driven to the teeth and jaws alone. In these straits of the fossil-hunter, embryology has lately come to our aid.

Assuming their remote reptilian origin, agreeing with Baur and Kükenthal that the theriomorph reptiles were parallel with rather than ancestral to the mammals, and therefore placing before both groups the hypothetical *Sauro-mammals* in or below the Permian, we come to the old question which Huxley discussed in his famous anniversary address: "Was there a succession between Monotremes, Marsupials, and Placentals, or a parallel development from a common pro-mammalian type?" Then we look to the newer questions: "When were the Edentates and Cetaceans given off?"

Modern tooth science springs first from the recent demonstration of Rütimeyer's hypothesis of 1869, that the teeth of all the mammals centre around a single reptile-derived type. With a single exception, which I believe can be disposed of, various stages of trituberculism or a three-cusped condition have become the standard for the teeth, as pentadactyly has long been for the feet, except that this is developed within the mammalian stem, while our five fingers are a reptilian legacy. Second, it springs from the recent thorough exploration of the youngest jaws for evidences as to the primitive form and succession of the teeth. This also supports the reptile theory of tooth descent by proving, what has been in considerable doubt, that the Pro-mammalia had a multiple succession of teeth like the reptiles, and that even some of the modern mammals retain dim traces of four series of teeth.

The brilliant discoveries of Kükenthal, Leche, and Röse begin to show how in various ways the mammals early modified the regular succession of all the teeth by suppression of parts of the multiple series. This is the first thing to consider. The next is how heterodontism arose, how the rows of conic teeth were specialised in different parts of the jaw for three or four functions. As a certain number of teeth took up each function, the question arises whether their number or dental formula was ever the same in all the mammals, for we know it is very different now. After the teeth were thus divided, some functions became more important than others, and established a monopoly, causing first a marked difference in the relative development of the series, which we may express in a dental curve, resulting finally in a loss of certain teeth. In the meantime began the special evolution of the form of the back teeth, or molars. Was this alike in all mammals; was it tritubercular? It is surprising how many problems of early relationship are at stake in the discussion of these simple processes.

Primitive Diphyodontism.

What does *succession* really consist in? It now appears that Baume was right in denying that the first tooth is the mother of the second; for the teeth of the lower as well as the upper series spring from the common epithelial dental fold (Schmelz leiste) which dips down from the surface and extends the whole length of the jaw; at intervals it buds off the dental caps (Schmelzkeim) of the first series; after these are separated off, the dental fold sinks and buds off the dental caps of the second series, always below and inside the first; thus the fold is the mother and the caps are sisters, twins, or triplets, according to the number of the series. In all young mammals, including the traditional monophyodont Cetaceans and Edentates, and excepting only the still unexplored Monotreme embryos, traces of two series of teeth have been found. Both Leche and Röse have detected evidence that the dental fold sometimes buds off parts of a third series, thus explaining the occasional reversion of supernumerary teeth on the inner side of the second series, and Leche has seen traces of budding preceding the first series—thus giving us vestiges of four successions!

All our perplexities as to the relations of the milk and permanent teeth, and the ingenious but mistaken hypotheses of Baume, Flower, Wortman, and Cope have sprung from our want of evidence of the regular and complete diphyodontism of the stem mammals. The solution is in brief that the "milk teeth" and the "true molars" are descended from the first series, while the second series is represented by the "permanent incisors, canines, and pre-molars" and rudiments of dental caps beneath the true molars. The mammals early began to diverge from this primitive diphyodontism in many ways; apparently adapting the first and second series, respectively, to their infant and mature feeding habits; losing parts or all of one series or the other, and in some cases pushing teeth of the second series in among the first; this intercalation has been a most confusing factor to us.

In the Marsupials (Kükenthal) almost the entire first series became permanent; thus from the Jurassic period to the present time only a solitary fourth premolar of the second series has pushed out its elder-sister tooth, and Röse has observed that an outer upper-incisor also pushes up from the second series; the remainder of the second series still persist as rudimental dental caps beneath the first, even beneath the first and second molars! There are wide variations among the Placentals; thus in the lowest existing forms, the Insectivora, Leche finds that in the Shrew (*Sorex*) the second series is suppressed entirely, while in the Hedgehog (*Erinaceus*) of the twelve permanent teeth in the anterior part of the jaws five belong to the first series and seven to the second. We thus meet with the paradox, that among the "primitive" Marsupials and Insectivores the regular reptilian succession was early interrupted, while in all the "higher" mammals the reptilian succession of two series was retained in the anterior part of the jaw. Beneath the posterior highly-specialised molar teeth of both Marsupials and Placentals, the second teeth were early suppressed, although in the Edentates, which also originally had specialised molars, there is a typical succession of seven teeth behind the canine. These discoveries prove that the whale teeth, like their paddles, have acquired a secondary adaptive resemblance to those of the Icthyosaurs. How did the single and simple teeth of the Edentates and Cetaceans develop? Clearly by retrogression. As Leche points out in the aquatic Carnivora, in which the first series are degenerating, the single-series condition (monophyodontism) advances step by step with retrogressive simplification of the tooth form (homodontism); thus in the true seals, the eared seals and the walruses, as the permanent teeth become simpler, the milk teeth become smaller. The Edentates, so widely separated genetically, parallel the seals in tending to suppress the first series of teeth and simplify the crowns of the second series at the same time. We might jump to the conclusion that this gives us an explanation of the homodont and apparently monophyodont condition of the toothed whales, especially as it has been supposed they sprang from aquatic carnivora, but in this Order matters were reversed, for the first series persisted and the second series were suppressed and persist as a rudimental row of tooth caps buried in the jaw.

Each dental series has an adaptive evolution of its own, in Erinaceus the first series has an ancient and the second a modern form; in Ericulus both series are alike; in the Bats the first series is homodont, the second is heterodont (Leche); in the Edentates the first series is ancient and heterodont, the second is modern and homodont (Thomas, Rheinhardt), so among the Cetacea and Ungulata.

What deep and ancient clefts the different laws of succession mark between the Marsupials and these three Placental groups!

Primitive Heterodontism and Formula.

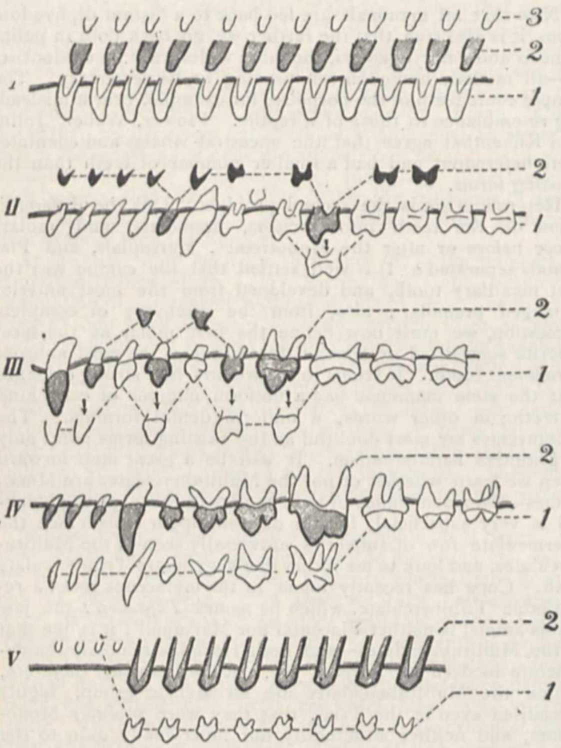
Now that all mammals are led back to a distant diphyodont stem, it is also true that the further we go back both in palingenesis and embryogenesis, the more widespread heterodontism is—all modern homodontism proving to be secondary. The simple conic teeth of the porpoise, for example, bear a misleading resemblance to those of a reptile. Flower, Weber, Julin, and Kükenthal agree that the ancestral whales and edentates were heterodont and had a smaller number of teeth than the existing forms.

Heterodontism is the second problem. Did the differentiation of the teeth into incisors, premolars, and molars occur before or after the Monotremes, Marsupials, and Placentals separated? It is well settled that the canine was the first maxillary tooth, and developed from the most anterior bi-fanged premolar; also, from the discovery of complete succession, we must now define the first molar as the most anterior specialised or triconid tooth, not as the most anterior permanent tooth. It seems to me we now find strong evidence that the stem mammals had a uniform number of each kind of teeth; in other words, a uniform dental formula. The Monotremes are most doubtful as the existing forms point only to primitive heterodontism. It will be a great step forward when we learn whether or not the Multituberculates are Monotremes—the resemblance of their molars to those of the duckbill is very superficial, for the duckbill upper molars lack the intermediate row of tubercles universally seen in the Multituberculates, and look to me rather like degenerate Trituberculate teeth. Cope has recently found in the cretaceous rocks a remarkable Trituberculate, which he names *Thlæodon*; the jaw of this animal is neither Placental nor Marsupial; it is like that of the Multituberculates—and both resemble remotely the degenerate modern Monotreme jaw. All we can say, therefore, is that the Multituberculates are an archaic group, highly specialised even in the Trias, that they were *probably* Monotremes, and neither structurally nor functionally akin to the Diprotodont Marsupials (Owen), nor to the Microbiotheridæ (Ameghino). With a dental mechanism and a condyle exactly like that of the rodents, they show no trace of canines, and the mode of evolution of their peculiar molars was probably paralleled later in the rodents. They present vestiges of a primitive dental formula like this: *I*3. *C*?. *P*4. *M*4+. *Thlæodon* shows *C*1. *P*4. *M*3. Thus, so far as this doubtful palæontological evidence goes, the Monotremes had a typical formula.

Our next step is to unify the typical 5. 1. 3. 4 of recent Marsupials with the 3. 1. 4. 3 of higher Placentals. Thomas has shown in his studies of recent Marsupials that they have probably lost one of the four typical premolars (*pm.* 2); this observation, fortunately, is partly confirmed by Röse's finding an embryonic germ of this tooth. Ignoring the incisors of the Jurassic Marsupials, Thomas raised the number of ancestral incisors to five, the highest number known among recent Marsupials; Röse therefore made another step towards uniformity when he showed that the Marsupial *i.*5 is probably a member of the second series of incisors, and should not be reckoned with the first. Now, if we suppose that the Placentals have lost one incisor, and one molar, abundant evidence of which is found in Otocyon, Centetes and Homo, we derive as the ancestral formula of both orders: *I*4. *C* & *P*5. *M*4.

The aberrant placental Cetacea point in the same direction, as we read in the conclusion of Weber's fine memoir: "All the

Cetacea sprang from a stem with a heterodont, but only partly specialised dentition (something like that of Zeuglodon, *I*3. *C*1. *P & M*). . . . not direct from Carnivores or Ungulates, but from a generalised mammalian type of the Mesozoic period, with some affinities with the Carnivora. . . . Zeuglodon itself branched off extremely early from the primitive line, and the heterodont Squalodon" (mark its formula, 3. 1. 4. 7.) "branched off later from the toothed whale line, after the teeth had begun to increase in number and before homodontism had set in." It would be easier for us while speculating to take Squalodon and the Odontocetes directly from the Jurassic mammalian formula (3. 1. 4. 8.). As for the multiplication of this formula, we have found the way, says Kükenthal, by which numerous homodont teeth have arisen from a few heterodont molars, namely, *by the splitting up of each of the molars of the Jurassic ancestors into three.* He substitutes this hypothesis for the one advocated by Baume, Julin, Weber, and Winge, that the multiple cetacean teeth represent the intercalation or joint appearance of both the first and second series of teeth, owing to the elongation of the



Relations of the First and Second Series of Teeth.

I. Reptiles. *II.* Marsupials. *III.* Insectivores (*Erinaceus*) *IV.* Higher Placentals. *V.* Edentates. *VI.* Cetacea, Odontocetes

jaw—a view which is now disproved by Kükenthal's discovery of the second row beneath the first. Since even by Kükenthal's hypothesis the typical Mesozoic mammals could not furnish as many teeth as are found in some of the dolphins, a likelier explanation than his seems to be that as the jaws were elongated the dental fold was carried back and the dental caps multiplied.

The Edentates, like the Cetaceans, point back to diphyodontism, and somewhat less clearly to a typical dental formula. We are here indebted to Flower, Rheinhardt, Thomas, Kükenthal, and Röse. It is their rudimental and useless first series which gives the evidence of heterodontism, while the second series has become adaptively rootless and homodont. The especially aberrant feature is that a double succession exists in the typical "true molar" region. The adult nine-banded Armadillo presents only eight maxillary teeth, seven of which are preceded by two-rooted milk teeth (Tomes); in the embryro Leche finds fifteen dental caps, of which only thirteen are calcified; this number probably includes the four rudimentary incisors observed by Rheinhardt. In the aberrant Orycteropus (Aard-Vark), with ten adult teeth, Thomas finds seven milk teeth behind the maxillary suture (thus taking us into the molar region of the typical heterodonts). The last of these milk teeth is large, and two-rooted; behind this are three large permanent posterior teeth, apparently belonging to the first series. The large lateral tooth of Bradypus is suggestive of a canine. From this rapidly accumulating evidence it appears probable that the ancestral Edentates had four incisors, a canine and eight or more teeth behind it, the double succession extending well back, so that the first series did not become permanent at the fifth tooth behind the canine, as in the Marsupials and higher Placentals. If these are primitive conditions, as seems probable from comparison with fossil Edentates, they carry the divergence of the Edentates, like that of the Cetaceans, back into the Mesozoic period. Comparative anatomy and embryology thus point back to highly varied branches of a generalised placental heterodont stem in the Mesozoic, and a much earlier divergence than we formerly imagined. Now let us see to what the early Mesozoic mammals point.

There are three distinct and contemporary Jurassic types, the Multituberculates, the Triconodonts, and the Trituberculates. Are not these the representatives of the Prototheria, Metatheria, and Eutheria? In the archaic Multituberculates we have seen a monotreme type of jaw and vestiges of a typical ancestral formula. The Triconodonts are a newer group, perhaps derived from the Domotheriidæ (incipient Triconodonts) of the Trias, although these appear to be aberrant; the typical forms extend from Amphilestes to Triconodon, and exhibit the first stages of development of the inflected Marsupial jaw. The Trituberculates include the Amphitheriidæ and Amblotheriidæ with true tuberculo-sectorial lower molars, like those of modern Insectivores; they alone exhibit the typical angular placental jaw—no reason can be assigned for calling them Marsupials, excepting the traditional reverence for the Marsupial stem theory. Now, it is very significant that the average dentition of these old but highly diverse forms, namely, Multituberculates, 3.?4. 6., Triconodonts, 4. 1. 4. 7., Trituberculates, 4. 1. 4–5. 8., is also the dentition to which the existing mammals apparently revert.

The third problem is from what type of molar tooth did the mammalian molar diverge?

(*To be continued.*)

SCIENTIFIC SERIALS.

Symons's Monthly Meteorological Magazine, December 1893.—In an article entitled "March to October, 1893," Mr. Symons deals with the temperature of the last eleven years in the north-west of London, with instruments identical in themselves and in exposure, with especial reference to the exceptional summer of the year 1893. The first two tables give the average monthly maximum in the shade, and the average maximum in the sun for 1883–92, compared with the mean values for 1893. In both cases the means of 1893 exceeded the average in each month, the excess of the shade maximum ranging from 1°·2 in September to 9°·7 in April, while the sun maximum shows an average excess of 7°·1, ranging from 1°·8 in July to 10°·8 in May. The tables showing the extreme maxima in shade and sun for each month of 1893, with the average of the highest reading in the ten corresponding months, again have plus signs in every instance in 1893, the greatest excess of the former being 10°·5 in April, while the mean of the eight months (March to October) shows an excess of 6°. The severe test of comparing the highest reading for each month of 1893 with the absolute highest reading in the corresponding month during the preceding ten years, shows that the season as a whole was unprecedented. The shade maxima were unequalled in April, June, July and August, the excess in April amounting to 5°·4, whereas in no other year of the ten have unequalled shade maxima occurred in more than two months. In some particulars August 1893 is unparalleled in thirty-six years. The shade temperature at 9h. a.m. on the 18th, viz. 84° 3, was 3°·5 higher than any other 9h. a.m. reading, and the shade maximum on the 16th to 19th all exceeded 90°, the only instance of this temperature being reached on three consecutive days.

Wiedemann's Annalen der Physik und Chemie, No. 12.—On the change of intensity of light polarised parallel to the plane of incidence by reflection on glass, by Paul Glan. The light reflected from a glass prism was compared with that of a petroleum flame by means of a polarising arrangement consisting of a doubly-refracting prism and a Nicoll, between which a Hofmann prism was placed in order to obtain a spectrum of the reflected light. For crown glass, the ratio of the intensity of the reflected to that of the incident light polarised in the plane of incidence ranged from 0·055 at 30° to 0·293 at 70°, the corresponding values for flint glass being 0·070 and 0·327.—Hydrodynamico acoustical investigations, by W. König. The turning moment exerted by a moving column of a fluid upon a disc suspended in it was subjected to experimental investigation, the torsion being balanced by a magnet. For very small velocities of the column of air employed the form of flow was uniform, but it was found impossible to keep it so in the case of any considerable velocities. The contemplated determination of all the dynamical conditions of Rayleigh's disc swinging in an organ pipe, and its application to the absolute measurement of sound intensities has not yet succeeded.—Experimental investigations concerning elastic longitudinal and torsional fatigue in metals, by Louis Austin. The wires experimented upon were 23 m. long, and were suspended in the tower of the Strassburg Physical Institute. It was found that longitudinal and torsional fatigue phenomena are subject to similar laws. The fatigue effects in copper, silver, and brass were, for torsion, as 7 : 3 : 2, and for tension, as 4 : 3 : 2 approximately.—On the properties of various modifications of silver, by H. Lüdtke.—On thermopiles made of electrolytes and unpolarisable electrodes, by A. Gockel.—On the magnetism of iron cylinders, by O. Grotrian.—On the passage of electric waves through layers of electrolyte, by G. Udny Yule.—On some modifications of the Thomson quadrant electrometer, by F. Himstedt.—A calibrated electrodynamometer, by J. W. Giltay.—A new method of measuring self-potentials and induction coefficients of induction, by L. Graetz.

SOCIETIES AND ACADEMIES.

LONDON.

Chemical Society, December 7, 1893.—Dr. Armstrong, President, in the chair.—The following papers were read:—An apparatus for the extraction and estimation of the gases dissolved in water, by E. B. Truman.—The magnetic rotation of hydrogen chloride in different solvents, and also of sodium chloride and of chlorine, by W. H. Perkin. The author confirms his previous observations on this subject, and also shows that isoamylic oxide and hydrogen chloride do not appreciably interact. The magnetic rotation of hydrogen chloride in isoamylic oxide solution is 2·245, in alcoholic solution 3·324, and in aqueous solution 4·300. The magnetic rotations of sodium chloride and of chlorine were also determined.—Analysis of water from the Zem-Zem well in Mecca, by C. A. Mitchell. The author gives analyses of water obtained by the late Sir R. Burton from the holy well in Mecca.—The preparation and properties of bromolapachol, by S. C. Hooker. Bromolapachol is obtained by reducing dibromolapachone; when dissolved in sulphuric acid it yields bromo-β-lapachone. The latter is converted into bromo-α-lapachone by the action of hydrobromic acid, whilst the reverse change occurs on dissolving the α-isomeride in sulphuric acid.—Studies on citrazinic acid (Part ii.), by T. H. Easterfield and W. J. Sell.—The oxides of the elements and the periodic law, by R. M. Deeley. The author obtains a new periodic diagram by plotting the atomic weights of the elements against the numbers obtained on dividing the densities of the oxides by the atomic weights of the corresponding elements.—The freezing points of alloys in which the solvent is thallium, by C. T. Heycock and F. H. Neville. The mean depression of the freezing point by the addition of one atomic proportion of gold, silver, or platinum to one hundred atomic proportions of thallium is 6°·31; the addition of lead to thallium, however, raises the freezing point.

Geological Society, December 20, 1893.—W. H. Hudleston, F.R.S., President, in the chair.—The following communications were read:—On the stratigraphical, lithological, and palæontological features of the Gosau beds of the Gosau district, in the Austrian Salzkammergut, by Herbert Kynaston. The author, after referring to the previous literature of the subject, treated of the situation and physical aspects of the Gosau valley, the distribution of the Gosau beds, their stratigraphy, palæontology, and geological horizon, and the physical conditions under which they were deposited, and a comparison was instituted between the Gosau beds and the equivalent beds of other areas. He showed that Hippurites occur at two horizons in the Gosau beds—a hippurite-limestone immediately above the basement-conglomerate being characterised essentially by *Hippurites cornuvaccinum*, which is overlain by *Actæonella*- and *Nerinæa*-limestones and an estuarine series, and above these was a second hippurite-limestone characterised essentially by *Hippurites organisans*. It was pointed out that Toucas similarly distinguishes two hippurite zones in Southern France, the lower, characterised essentially by *H. cornuvaccinum*, being placed by him at the top of the Turonian system, whilst the second, with *H. organisans*, is referred to the summit of the Senonian; and the author gave reasons for regarding the Gosau zones as the equivalents of those of the South of France, in which case the Gosau beds will represent the uppermost Turonian and the whole of the Senonian, *i.e.* the zones of *Holaster planus*, *Micraster*, *Marsupites*, and *Belemnitella mucronata* in England, whilst the upper unfossiliferous beds may be the equivalents of the Danian beds. The strata are, on the whole, of shallow-water origin, and were deposited in shallow bays in the Upper Cretaceous sea of Southern and Central Europe, on the northern flanks of the Eastern Alps. Probably towards the close of Upper Cretaceous times the southern area of the Gosau district was cut off from the sea to form a lake-basin in which the upper unfossiliferous series was deposited. Mr. W. Whitaker, Sir John Evans, and Prof. J. F. Blake spoke on the subject of the paper, and the author briefly replied.—Artesian boring at New Lodge, near Windsor Forest, Berks, by Prof. Edward Hull, F.R.S. The boring described in this paper was carried down from a level of about 220 feet above Ordnance datum through the following beds:—London Clay and Lower London Tertiaries, 214 feet; Chalk, 725 feet; Upper Greensand, 31 feet; Gault, 264 feet; Lower Greensand, 7 feet. The chalk was hard, and contained very little water; but on reaching the Lower Greensand the water rose in the borehole to a height of 7 feet from the surface. The author discussed the probability of the Lower Greensand yielding a plentiful water supply in the Windsor district. In the discussion that followed, the President said it was satisfactory to learn that there was an area near West London in which the Lower Greensand was full of water. He thought that the section exhibited by the author explained why it was full in that particular locality, for the rainfall about the extensive area of Hindhead, which lay nearly due south, must be considerable. Mr. W. J. Lewis Abbott and Mr. W. Whitaker also spoke, and the author replied.—Boring on the Booysen Estate, Witwatersrand, by D. Telford Edwards. An account was given of a boring on the Booysen estate, situated about two miles from Johannesburg, and about 5000 feet south of the nearest point of outcrop of the "Main Reef" of the Witwatersrand. The "Bird-Reefs" crop out generally at a distance of 4000 feet south of the Main Reef. The borehole, 1020 feet deep, passed through sandstones (often micaceous), quartzites, and conglomerates, the last-named having a collective thickness of 91 feet 7 inches, the two thickest reefs being respectively 26 and 22 feet thick. The dip of the beds was 35°. Traces of gold were obtained. All the reefs were highly mineralised, principally with iron pyrites, and belonged to the "Bird-Reef" series which overlies the Main Reef.

PARIS.

Academy of Sciences, December 26.—M. de Lacaze-Duthiers in the chair.—On the motion of Jupiter's fifth satellite, by M. F. Tisserand. A calculation of the displacement of the "perijove" of the fifth satellite due to the polar depression of Jupiter shows that it would amount to 882° per annum, or one revolution in nearly five months. It is hoped that powerful instruments will enable observers to verify this.—On the propagation of electricity, by M. H. Poincaré. Starting from the "telegraphists' equation," the author shows that when an electrical disturbance proceeds along a wire, the head of the disturbance moves with a velocity such that, in front of this head, the disturbance is nil, as in the case of light and of plane sound waves, with the difference, however, that the electric disturbance leaves behind a residue of finite magnitude.—Numerical verifications relating to the focal properties of plane diffraction gratings, by M. A. Cornu. The verification of the theory of focal anomalies in gratings already published, by testing actual gratings showing such anomalies, was based upon the following theorem: When the observed pencils make a constant angle with the incident beam remaining fixed, half the sum of the azimuths of the grating corresponding to spectra of symmetric orders is constant, and equal to the azimuth corresponding to the reflected beam.—Remarks on the spontaneous heating and ignition of hay, by M. Berthelot. Hay dried and stacked under normal circumstances loses moisture and oxidises slowly, without being sensibly heated. The initial heating, where it takes place, is due to the action of ferments, but not the higher stages of the process. When the ferments are no longer capable of further raising the temperature without endangering their own existence, it often happens that purely chemical action steps in, and leads up to the ignition of the haystack. The temperature of ignition for these materials is far below red heat.—On the composition of winter drainage waters from bare and from cultivated soils, by M. P. P. Dehérain.—Observations of the minor planets 371 and 372 (1893) made with the great equatorial of the Bordeaux Observatory, by MM. G. Rayet and L. Picart.—The analysis of commercial butters, by M. C. Viollette.—On the approximate development of the disturbing function in the case of inequalities of higher orders, by M. M. Hamy.—Investigation of that part of the coronal atmosphere of the sun which is projected upon the disc, by M. H. Deslandres.—Is there oxygen in the sun's atmosphere? by H. Duner.—New applications of the tables of increasing latitudes to navigation, by M. E. Guyon.—On the successive radii of curvature of certain curves, by H. R. Godefroy.—Calculation of electro-magnetic forces, according to Maxwell's theory, by M. Vaschy.—On the diurnal variation of the tension of aqueous vapour, by M. Alfred Angot. The observations made at the top of the Eiffel Tower since the end of 1889 have shown that at the height of 300 m. the change of vapour tension during winter does not exceed a few hundredths of a mm. During the eight months beginning with March, a single maximum was observed during the day at 9 a.m., and a minimum at 5 p.m., while in the adjacent Parc Saint-Maur, there were two maxima, at 9 a.m. and 8 p.m., and two minima, at 4 a.m. and 4 p.m. It appears that the variation of vapour tension, as observed in ordinary meteorological stations, is a local phenomenon, limited to the lower strata of the atmosphere.—On the diurnal variation of atmospheric electricity, observed near the summit of the Eiffel Tower, by M. A. B. Chauveau. The indications of an electrometer registering photographically the potential of the air, lead to conclusions similar to those of the preceding paper. The two sets of maxima and minima observed on the ground are replaced by one set only, consisting of a maximum at about 6.30 p.m. and a minimum at 4 a.m. The potential, which sometimes exceeded 10,000 volts, was reduced to a convenient amount by the interposition of condensers in cascade.—On the weight of a litre of normal air, and the density of gases, by M. A. Leduc.—Sketch of a system of atomic weights of precision, founded upon the diamond as standard substance, by M. G. Hinrichs.—General method for the volumetric estimation of silver under any form, by M. G. Denigès.—On the stability in air of a 0·001 solution of corrosive sublimate, by M. Tanret.—Remarks on the critical pressures in the homologous series of organic chemistry, by M. E. Mathias.—On caseine and the organic phosphorus of caseine, by M. A. Béchamp.—On a new source of rhodinol, by MM. P. Monnet and Ph. Barbier.—Presence of camphene in essence of aspic, by M. G. Bouchardat.—On the volatile carbides of the essence of valerian, by M. Oliverio.—Contribution to the study of the ptomaïnes, by M. Œchsner de Coninck.—Influence of certain causes upon receptivity; bacterian associations, by M. V. Gattier.—Toxicity of the blood of the viper (*Vipera aspis L*).—Modifications of the emissive power of the skin under the influence of the electric brush discharge, by M. Lecercle.—Influence of iron upon the vegetation of barley, by M. P. Petit.—Influence of bark-stripping upon the mechanical properties of wood, by M. E. Mer.—On the natural dessication of grains, by M. H. Coupin.—On the oolitic strata of the Paris Tertiary, by M. G. F. Dollfus.

BERLIN.

Physiological Society, December 8.—Prof. Munk, President, in the chair.—Prof. A. Kossel gave an account of his further researches on nucleic acid, carried on in conjunction with Dr. Neumann. The acid, as obtained from the thymus, differs from that obtained from other sources, in that during its decomposition it yields only adenin; it has hence been distinguished as adenylic acid. It occurs in two forms: one readily soluble, the other soluble with difficulty. When boiled with water, this acid yielded a paranucleic acid, which contained no adenin. By boiling with dilute hydrochloric acid a fourth acid (thyminic) was obtained, from which crystalline thymin could be obtained. All the above well-characterised substances possess, when analysed, an extremely complex constitution; thus the molecule of adenylic acid contains 75 atoms of carbon, and that of paranucleic acid 90 atoms. Dr. H. Kossel had studied the action of nucleic acid on bacteria, and found that cholera-germs and streptococci are readily killed by small quantities of the acid; whereas anthrax germs are much more resistent. He therefore considered that the bactericidal action of lymph-cells was attributable, in part at least, to this action of nucleic acid.—Dr. Rawitz spoke on spermatogenesis in Hydromedusæ. Unlike all other animals, the spermatozoa in this animal are developed in the outer layer of the bell, and are discharged direct into the surrounding fluid. The same speaker further described curious large branching villi in the jejunum of Macacus, not met with in the intestine of other species of monkey.

BOOKS PAMPHLETS, and SERIALS RECEIVED.

BOOKS.—The Canadian Ice Age: Sir J. W. Dawson (Montreal).—The Genus Salpa, 2 Vols., Text and Plates: Prof. W. K. Brooks (Baltimore).—The Butterflies and Moths of Teneriffe: A. E. H. White (L. Reeve).—Journal of Microscopy and Natural Science, Vol. 3, Third Series (Baillière).—Linnean Society of New South Wales, the Macleay Memorial Volume: edited by J. J. Fletcher (Dulau).

PAMPHLETS.—Origin of the Pennsylvania Anthracite: J. J. Stevenson (Rochester).—On the Use of the Name "Catskill": J. J. Stevenson (Rochester).—The Marsh Warbler, &c.: W. W. Fowler (Oxford, Blackwell).—On Technical Education in Glasgow and the West of Scotland: H. Dyer (Glasgow).—Imperial Institute Series, Handbooks of Commercial Products, Indian Section, Nos. 1–22, 24–25, 27–29 (Calcutta).—Guides to Commercial Collections, Indian Section, No. 1 (Calcutta).—Agricultural Ledger Series, Nos. 1–13 (Simla).

SERIALS.—Bulletin de l'Académie Royale des Sciences de Belgique, 63 Année, No. 11 (Bruxelles).—Journal de Physique, December (Paris).—Zeitschrift für Physikalische Chemie, xii. Band, 6 Heft (Leipzig).—Zeitschrift für Wissenschaftliche Zoologie, lvii. Band, 1 Heft (Leipzig).—Bulletins de la Société d'Anthropologie de Paris, December 15 (Paris).—Verhandlungen des Gesellschaft für Erdkunde zu Berlin, Band xx. Nos. 8 and 9 (Berlin).—Verhandlungen der Gesellschaft für Erdkunde zu Berlin, Band xxviii. No. 4 (Berlin).—American Naturalist, December (Philadelphia).—Journal of the Royal Agricultural Society of England, Third Series, vol. iv. part 4 (Murray).—L'Astronomie, January (Paris).—The Asclepiad, No. 39, vol. x. (Longmans).—Geological Magazine, January (K. Paul).—Séances de la Société Francaise de Physique, April–July, 1893 (Paris).

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