THURSDAY, MAY 21, 1891.

## *PYCNOGONIDS.*

*Den Norske Nordhavs-Expedition*, 1876–78. *XX. Zoologi—Pycnogonidea.* Ved G. O. Sars. Med 15 Plancher og 1 Kart. (Christiania : Grondahl & Sons, Bogtrykkeri, 1891.)

*Studies from the Biological Laboratory, Johns Hopkins University, Baltimore : A Contribution to the Embryology and Phylogeny of the Pycnogonids.* By T. H. Morgan. With Eight Plates. (Baltimore : The Johns Hopkins Press, 1891.)

THE group of sea spiders, or the Pycnogonidea, was for a long time among the least known, though by no means the least interesting, of the divisions of the marine invertebrates. Linnæus described a species as a Phalangium, placing it among terrestrial forms, and though a century and a quarter has passed since then, the problem of where to place these Pycnogonids cannot be said to be finally settled.

Within the last ten years or so, an immense advance has been made in our knowledge of the morphology, anatomy, and embryology of the group, thanks to the labours of Anton Dohrn, who, in 1881, described the forms found in the Gulf of Naples, and of Hoek, who about the same date described the species found during the cruises of the *Willem Barents* and the *Challenger.* During all this period opinions varied as to whether these forms should be placed among the Arachnids or the Crustacea, but apparently both the authors just referred to have agreed that the Pycnogonids should be placed with neither, but that they, with the Arachnids and the Crustacea, have come down the stream of evolution in parallel lines.

To the existing recent memoirs of these Arthropods, the splendid volume just published on the Pycnogonidea found during the Norwegian North Atlantic Expedition, 1876–78, by Prof. G. O. Sars, adds, perhaps, from a morphological point of view, the most important of the recent publications on the group ; for, valuable as beyond question are the structural and developmental details, a special knowledge of general morphological detail is also needed for the convenient understanding and classifying of any group.

The material at Prof. Sars's disposal was very large, and in addition he has made use of collections made by himself during many years back on the coasts of Norway, and also of some few forms sent to him by Dr. A. Stuxberg, which had been found in the Kara Sea during Nordenskiöld's expedition. A very great contrast is to be seen on comparing these northern forms with such a collection as that of Dohrn from the Mediterranean. The great number of species belonging to the family Nymphonidæ is specially characteristic of the Northern Seas as contrasted with the Mediterranean, while again the Northern Sea species attain very generally much larger dimensions, some being gigantic in comparison with those of the Mediterranean.

In working out the classification of the group, Sars has found it necessary to treat the families in a somewhat more restricted sense than has been done by most of the previous writers, and has been obliged to increase their number. While fully agreeing that the descriptions and even figures of the Pycnogonids given by the earlier writers leave much to be desired, and are as a rule even exceedingly defective, in some cases indeed being so bad as not to be intelligible, yet he thinks that some quite recent describers have rejected as bad a greater number of descriptions than with a little patient research was really necessary. Thus he finds it hard to believe that, while not a few species have been described from the Gulf of Naples, all the species described as found there by Dohrn, with one exception, should be new. Most certainly as regards the northern species we cannot sufficiently admire the pains which Sars has taken in working out all the imperfect descriptions and rough figures of our past recorders of new forms, with the result that he has succeeded in re-establishing many wholly forgotten or ignored species of Goodsir and others.

As regards the terminology used in describing the various parts, some, classing the Pycnogonids with the Crustacea, adopted terms in use among the latter ; while others, holding their affinity to be with the Arachnids, employed again a different set of terms. Dohrn, to avoid the difficulty as regards the limbs, rejects all special terms, describing them as No. I., II., &c. ; Sars uses a terminology the terms of which involve as little as possible of any homologous references.

Forty-three species are described and figured. Several of them are here fully described for the first time, though short diagnoses of them appeared in a preliminary report. The fourteen genera are arranged in eight families, and these are grouped into three orders, the ordinal characters being based on the relations of the "chelifors." Thus in Order 1, Achelata, these chelæ are, except in the larval state, entirely absent ; in Order 2, Euchelata, the chelæ are well developed throughout all the stages of life ; while in Order 3, Cryptochela, the chelæ are present, as a rule, in the young stages (not alone in the larvæ), but in the fully developed condition they become atrophied or disappear. This arrangement no doubt will have to be modified so as to fit it to receive the very numerous forms from other parts of the world, but it is a first step in the right direction of an intelligent grouping of the genera.

The second memoir on our list treats of the Pycnogonids from a different standpoint, being a contribution to our knowledge of the embryology and phylogeny of the group, by T. H. Morgan, Fellow of Johns Hopkins University. After a short allusion to the work of Dohrn and Hoek, who have "placed the morphology of the order on a very firm basis," he proceeds to treat of the early stages of the embryology of the Pycnogonids, stages which have been practically unexamined, and a knowledge of which is needed to enable the relationship of the group to be guessed at.

The material for this work was collected at Wood's Holl. Three genera, each with a single species, are to be found at this place—*Pallene empusa*, *Phoxichilidium maxillare*, and *Tanystylum orbiculare ;* and during July, August, and September, these were found carrying ova. The alcoholic picro-sulphuric acid process was adopted for hardening ; the eggs being cut in paraffin. The eggs of Pallene were large, 0·25 mm., and well adapted for investigation. After a minute description of the early stages of development, the author considers that from them there is little or no ground for a comparison between the Pycnogonids and the Crustacea, certainly not with any existing forms. The multipolar delamination of the endoderm in the Pycnogonids has no homologue amongst the Crustacea, nor is there any special similarity in the formations of the organs. There seems to be no trace of gastrulation like that in the Crustacean in the ontogeny of the group. And if there be reason for rejecting a relationship between the Pantopod larva and the Nauplius, and with Dohrn he believes that there is, then there remains nothing in common to the ontogeny of the two groups.

Nor are there any special affinities between the insects and Pycnogonids ; but between these latter and Peripatus a striking similarity is met with in the paired ventral organs, both in the structure and position of these, but for the present there is no proof forthcoming as to a real homology of these bodies. The process of the formation of the endoderm, as described by Heider and by Wheeler in insects, shows a certain resemblance to multipolar delamination ; but if it be such, it is a more complicated form than is shown by the Pycnogonids. With these two exceptions there would seem to be nothing else in common in the ontogeny of the two groups.

Lastly, as to a decision as to the relationship with the Arachnids, or as to their being an independent phylum. While Dohrn and Hoek ably maintain the latter, though not agreeing as to the why in all details, yet the study of the early stages of the embryology has brought to light certain facts which lead the author of this memoir to believe in a community of descent between the two. The reasons for this belief are given in full detail, with difficulty admitting of abbreviation. The Pycnogonids form the endoderm by a process of multipolar delamination, which is shown in its simplest form in Phoxichilidium and Tanystylum, and in a more modified condition in Pallene. In no other group of the Triploblastica is a similar phenomenon found except in the Arachnids. In the spiders the process is not so well marked, but if Balfour's conception of the formation of the yolk nuclei be correct, then a direct comparison may be made between the two groups. The first trace of the embryo to appear in Pallene is a round opaque area at the spot where the stomodæum invaginates. In Schimkewitsch's recent account of the development of the spiders, he shows that the primitive cumulus in them is the place where the stomodæum invaginates ; and in calling attention to the fact that the stomodæum of spiders in its earliest development is a triangular invagination, he actually compares it with the triangular invagination of the œsophagus of the Pycnogonids. It is also exceedingly probable that the early formation of the body cavity surrounded by mesoblast in the legs of spiders has an exact parallel in Pallene and Phoxichilidium. In both Arachnids and Pycnogonids there are well-marked diverticula from the mid gut into the legs. In both Arachnids and Pycnogonids the first pair of appendages are chelate, and in both this first pair is innervated from the brain ; these facts alone, it will be remembered, were considered by Balfour to indicate a relationship between the groups. Mr. Morgan was unable to find any post-oral ganglia for Pallene, but the first pair of appendages arises on the sides of the stomodæum and moves forward later. In this respect, it compares closely with the spiders, and the early innervation of this pair from the brain itself may be regarded as a more abbreviated condition than what was seen (by Balfour) in the spiders. Metchnikoff's figures for Chelifer show the first pair of appendages to arise above and on each side of the proboscis-like upper lip, and if future investigation verifies Metchnikoff's suggestion that this proboscis is homologous, entirely or in part, to the proboscis of the Pycnogonids, as his figure seems to indicate, then does the whole development of the Chelifer show remarkably close resemblances to that of the Pycnogonids. The fourth pair of ambulatory legs—the seventh pair of appendages—has been a stumbling-block in the way of an Arachnid relationship, and the attempts to solve the difficulty have been many. Here, again, Balfour's suggestion that this last segment and its appendages may represent the first abdominal segment of the Arachnids is of value, as we know that the embryos of spiders have rudimentary appendages on the abdomen. In a second part of this memoir the metamorphosis of Tanystylum is described, and in a third part we have a very complete study of the structure and development of the eyes of Pycnogonids and a comparison with the Arachnid simple eyes, a comparison that seems to verify the relationship pointed out in the first part of the memoir. E. P. W.

## *A TEXT-BOOK OF CHEMISTRY BASED ON THE PERIODIC SYSTEM.*

*A System of Inorganic Chemistry.* By William Ramsay, Ph.D., F.R.S. Pp. 700. (London : J. and A. Churchill, 1891.)

DURING the twenty-five years or so which have elapsed since the recognition of the periodic law of the chemical elements as a valid relationship, the pronounced influence which it has exercised both on the aspect and aims of chemical science cannot be questioned. Whether in the prediction of undiscovered elements, or as an indicator of needful research, especially in the department of atomic weight estimations, it has met with signal success. In connecting the physical properties of the elements themselves and of their compounds with atomic weight, it has opened up new fields of investigation, and thrown fresh interest into old ones. Properties so widely different as those measured by refraction equivalent and breaking stress find an explanation, nowadays, in the magnitudes of the atomic weights.

As a means of classification, too, the success of the periodic arrangement has not been less striking. Indeed, to its power as an instrument of classification it owes its general acceptance in the first instance. When the ideas of Avogadro had become recognized, and by their means the old system of "equivalents" had been replaced by the true atomic weights, then the periodic arrangement resulted in a grouping of the elements so much in harmony with existing notions of their relationships, that the far-reaching power of the generalization could no longer be resisted.

The distinguishing feature of the book before us consists in the use of the periodic arrangement as a means of classifying the subject-matter of inorganic chemistry. Here, the time-honoured methods of putting the facts and theories of chemistry before the student are set aside, and as the method adopted is novel to English text-books, it may be advisable to consider its characteristics. After a short historical introduction, the author proceeds to describe the occurrence, preparation, and properties of the elements in the order in which they are found in the periodic table. First, Group I., hydrogen and the alkali metals; then Group II., metals of the alkaline earths, and so forth. The descriptions refer, as far as possible, to the elements of the same group taken collectively.

The compounds of elements of the different groups with the halogens form the next part, and in the introductory portion the student meets for the first time with matter which it is customary to discuss at an earlier stage in the text-books; such matter as the distinction between element and compound, the use of chemical symbols, the gaseous laws, &c. The fourth part deals with the oxides, sulphides, selenides, and tellurides, and under these headings are to be found hydroxides, hydrosulphides, &c., classed as compounds of the oxides with water, hydrogen sulphide, &c. Here, also, are treated the salts of the oxyacids, classed as double oxides, and compounds as $POCl_3$ treated as double compounds with the halogens.

Part v. gives an account of the borides, carbides, and silicides; such of the hydrocarbons as are considered, and the organo-metallic compounds occur in this part. Compounds with the elements of the nitrogen group, including the cyanides, form Part vi. Alloys and amalgams are discussed in Part vii. The first chapter of the next part gives a short account of spectrum analysis and the rare earths. The second chapter is chiefly concerned with the criteria for fixing atomic and molecular weights, the Raoult methods finding a place, and the last chapter is devoted to the periodic law. The closing part of the book takes up, mainly with regard to the chemical principles involved, the manufacturing processes usually treated in the text-books.

It will be seen, as the author states in his preface, that the method adopted does away with the distinction between metals and non-metals; no special stress is laid on the properties of acids as contrasted with bases; equal prominence is given to rare and more common substances; and the commercial importance of a substance or process is not considered an argument for its special consideration.

Such a work as this may be looked at from two points of view. Regarded as a systematic arrangement of the facts of inorganic chemistry, from which any desired information may be speedily taken after one has become familiarized with the method of classification adopted, its success is undoubted. The book is quite in touch with recent investigations, nothing of importance seems to be omitted from the descriptive portion, and, what is a recommendation to a large class of readers, the size of the book is not excessive. Whatever be the results of the system adopted, economy of space is assuredly achieved. To the teacher or to the advanced student who wishes to use the book as a work of reference, or desires to systematize his knowledge, it will be eminently useful.

If, on the other hand, the system be regarded from the point of view of a basis for teaching, its construction from its very novelty must be open to discussion. A method of teaching chemistry often employed may be said to consist in giving the learner in as easy a manner as possible the leading facts of chemical science with regard, in the first instance, more to the correct appreciation of the meaning of the facts themselves, than of the exact arrangement or classification of the same. To this end the student is led from the study of the chemical properties of commonly occurring bodies to the description of the elements contained in them, explanations of chemical terms being given as they crop up, or in short reviews at intervals not far apart. When the properties of the elements are being explained, their reactions with other elements have to be noticed, and hence it appears natural to describe the important compounds of an element after its own properties have been discussed. The periodic system does not seem to provide the means for such a course of teaching, and this appears to us to be the main reason for its non-adoption in the text-books.

Indeed, the new method has little in common with that indicated above. The entire series of the elements apart from their compounds are described, and chemical and physical terms are freely used without any attempt being made to define them till all the elements have been treated. In fact, a few terms, as critical point and heat of formation, are used, but as far as we can see, not defined in the book. Again, compounds containing a common constituent are classed together, but compounds of what may be taken as a parent element are scattered throughout the various groups. Surely, in connection with this point, reasons similar to those which lead to the grouping of compounds containing the same element, on the new system, would hold for the old method of considering compounds. The position of the iron group of elements after the aluminium group and of the copper group—the last one described—may be taken as an indication that even in the author's opinion the periodic law does not in all cases indicate most clearly the relationships of the elements. Such considerations as these must weigh with a teacher before he can adopt the system; during four years' experience, however, the author has had no reason to doubt its success.

The book is clearly printed, and the illustrations, though not very numerous, are for the most part new. The frequent use of vapour jackets in the apparatus represented is suggestive of the author's more recent contributions to scientific literature. The useful system adopted by Ostwald in his "Lehrbuch," of indicating the state of aggregation of a substance by the type, has been employed.

Setting aside the points which may be urged against the work as a basis for teaching, the periodic law, as expounded by Prof. Ramsay, does more than any other system of classification to put the matter of inorganic chemistry on a footing resembling that which holds for organic chemistry.

## OUR BOOK SHELF.

*Eighteen Years of University Extension.* By R. D. Roberts, M.A., D.Sc. (Lond.). (Cambridge: University Press, 1891.)

THE University Extension movement takes so prominent a place among the educational influences of the age that a good account of the system has for some time been needed. This is supplied by Mr. Roberts, who, first as lecturer, then since 1881 as assistant and organizing secretary to the Cambridge Syndicate, and since 1886 as secretary to the London Society, has had the best possible opportunities of studying the new method, and of forming a judgment as to its fitness for the uses to which it is applied. He begins with an account of the origin and growth of the movement, then describes the character of the audiences, the reception of the idea by artisans, and the signs of earnestness displayed by various classes of students. Mr. Roberts also discusses the conditions of success, has a chapter on the consolidation of the work, and presents a summary of results. No essential fact has been omitted, and the general impression which will be left on the minds of most readers probably is that those connected with the movement have done much to foster and to satisfy the desire of a very large number of persons for intellectual training. There are certain rules—some of them rather difficult—with which the system must be brought into accord if it is to be capable of further development; and these are stated with much force and precision in the present useful little volume.

*Evening Work for Amateur Photographers.* By T. C. Hepworth, F.C.S. (London: Hazell, Watson, and Viney, Ltd., 1890.)

IN this book the author has written, in an interesting manner, a series of chapters relating to many points in photography that are generally found most useful to amateurs. The following are the subjects of some of the chapters: lantern entertainments, lantern-slides on gelatine plates, clouds in lantern pictures, frame-making, enlarging, photography by magnesium light. There are also two or three chapters on electric light, light by incandescence, and methods of making cheap batteries.

The subjects are treated in a manner that makes the book well worth reading, and its value is increased by numerous illustrations obtained from photographs and drawings by the author.

## LETTERS TO THE EDITOR.

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### The University of London Question.

THE Convocation of the University of London has, by a large majority, thrown out the scheme for the reconstitution of the University proposed by the Senate. Even those who had little love for it must feel some sympathy at the frustration of labours which were as patient as they were undoubtedly disinterested.

For the moment the whole question remains in abeyance. I am disposed to think that it may be useful to discuss, in the interval which must elapse before any further step is taken, some of the fundamental questions which seem to me to underlie the solution of the problem, and have never, as it seems to me, been properly considered.

On such a subject one might easily write a great deal. For the sake of brevity I shall therefore attempt to sum up what I have to say under separate heads.

*The Examination System.*

One factor in the present situation is undoubtedly the growing dissatisfaction of many distinguished teachers with the examination system as applied to University education. And as the University of London at present does nothing but examine, it is obvious that the question lies at the root of any judgment that may be pronounced on its present work and constitution. Those who wish to know all that can be said against the present use of examinations in University work cannot do better than study a paper by Prof. Lankester, which he has reprinted in his "Advancement of Science" (pp. 175-192). He has stated his case with all the force and lucidity of which he is a happy master. He sees "the most injurious result of the system" in "the degradation of the teacher." The "intrusive board of examiners" draws "away from him the attention and the respect of his pupils," or urges "him to put aside his own thought and experience, and to teach the conventional and commonplace."

I am free to admit that there is a certain element of truth in what Prof. Lankester says. But having had, like him, a good deal of experience both of examining and of being examined, I am disposed to think the picture somewhat over-coloured. No doubt the University of London in the past has exalted examination into a sort of idol. But as regards the superior degrees in science and medicine, at any rate, examination is now practically dispensed with, and the test of competence is the performance of some kind of original investigation.

For the inferior degrees, as far as I am aware, the examination system in more or less prescribed subjects obtains everywhere in the three kingdoms. For my part, I think the system may be defended, and upon the same lines as those on which Prof. Lankester defends "leaving examinations" at schools. For he says, and I think rightly, that such an examination "may be regarded as a means of criticizing and testing the performance not merely of the schoolboys but of the schoolmasters." Now in University education, as carried on in this country, I can only see a prolongation of school education, with methods and a moral discipline modified to suit the more advanced age of the pupils. And the inferior degree (I am not speaking, of course, of professional subjects) is, in my view, nothing more than the corresponding "leaving examination." It is a test of whether teaching has been faithfully done and learning diligently pursued.

I am quite ready to admit that impending examinations are more or less irksome both to teachers and taught; but I am not convinced that that discipline is in itself an evil. It is not undesirable that some restraint should be put on the possible vagaries of the one and the very probable desultoriness of the other. It is necessary in entering upon the study of a subject to go over its fundamental groundwork in a methodical manner. To many teachers and to many pupils this is not a little dull. It is easy and it is pleasant to dwell at some length on attractive parts of a subject and to skim superficially over others. There are probably few persons who, looking back upon their own student days, will not admit the truth of this. The fact is that to get any mastery of a subject one must learn its grammar; and the majority of young people require some degree of compulsion to make them do it. It may be irksome at the time, but the advantage is life-long. I know, speaking from my own experience, that the compulsion of schedules which is so odious to Prof. Lankester has made me devote my energies to the mastery of the rudiments at any rate of many subjects which I should certainly have carefully avoided if I had not been compelled to do otherwise. And I do not believe that, if students are carefully and soundly taught, they suffer any real injustice at the hands of competent examiners. But then I agree with Prof. Lankester that the examiners must know their business, and must not be either ill-informed or pedantic. No one, I think, can urge that the kind of men that the University of London enlists in its service as examiners are open to the charge of being either.

If these views are correct, and I believe in the main they are, then the evil consists not in the examination system as the incentive to the orderly performance of a curriculum, but in another and perfectly distinct evil on which Prof. Lankester very sagaciously puts his finger—the mischievous importance which the outside world attaches to academic achievement. "A man refers throughout his life to the fact that he obtained a 'first-class' as a sort of perpetual testimonial." Of course, in so far as this is true it is very absurd. A course of University study is a means, not an end; it is a sort of apprenticeship to a subject. The student learns its technique, its language, and something of its literature. If he has done this earnestly and well, his University will applaud him, will call him in academic language "a good boy." But when the congratulation of his friends has subsided, the real question arises, what will he do with the tools he has learnt to use? Here, I think, University work enters upon a new phase, and one, it seems to me, too little regarded—I mean post-graduate study. To control this in any measure by means of examination seems to me in the highest degree absurd. And I must contend that by making original investigation, at any rate for its doctorate of science, the qualification for that degree, the University of London has taken a step in advance of many of the older Universities towards destroying the idea that the passing of examinations is the final end of University study.

*A Teaching University.*

I have always found it not a little difficult to understand what those people exactly mean who so strenuously demand a teaching University for London. What Prof. Lankester means, there can, as is usually the case, be no sort of doubt about; and this I shall discuss presently. But, as far as I can make out, all other persons seem to think that London University students labour under some special disadvantage which undergraduates at Oxford and Cambridge do not experience. Perhaps, then, it may surprise many to be told that there is no essential difference in the two cases. Examining in the two older Universities is in the hands of the University, and is just as much distinct from the teaching in their case as in that of London. I can speak with some positiveness upon this point, for having been for four years an examiner for the Natural Science Tripos at Cambridge, a University with which I have no connection, I found the functions I was called upon to perform exactly the same as those I have also fulfilled at Burlington Gardens. In fact, I can see no essential difference between the position of an undergraduate of New College, Oxford, examined for his degree by the local University and an undergraduate of University College, London, examined by the University of the capital. If Oxford and Cambridge are teaching Universities in any intelligible sense of the phrase, then I contend that the University of London is equally so.

Prof. Lankester adopts the view of Fichte, who says "that a University is not a place where instruction is given, but an institution for the training of experts in the art of making knowledge, and that this end is attained by the association of the pupil with his professor in the inquiries which the latter initiates and pursues." Most excellent, and I can imagine nothing more delightful than for some wealthy man to give, say, half a million of money to found such a University in some quiet country town in England, where professor and pupils might labour together, undisturbed by the life and movement of a big city, or the worry of the examination-room, for the advancement of knowledge But if such "a seat of learning," in the true sense of the words, could be brought into existence, it would probably be found in practice that the students would be men who had already graduated, *i.e.* in my view acquired that knowledge of the elements of a subject which is essential to the proper performance of any work in it. A Professor of Biology, for example, would not care to have to teach a pupil at the commencement of a research how to interpret what he saw through the microscope, or how to cut a section. And if we firmly grasp the idea of the non-finality of the graduation course, we get an intelligible distribution of labour amongst the staffs of the older Universities: the college lecturers will prepare men for their degrees; the professors will guide their maturer studies afterwards.

While I cannot help thinking that those who advocate the creation of a so-called teaching University in London, have got hold of an idea which they have only imperfectly assimilated, it is still worth while to examine some of the ways in which it might be realized.

With an adequate endowment a new so-called teaching University might no doubt be established in London. It would have a staff of professors who, we may assume, would be adequately paid. The posts would in that case be no doubt filled with men of distinction and eminence. Would they be able to spend their time, full of enthusiam but free from care, in leading students in the paths of research after Prof. Lankester's ideal? Not a bit of it. Such an institution would not be very different from a Scotch University, where one of the most distinguished scholars of his age is said to have found his time largely taken up with teaching schoolboys of larger growth the mysteries of Greek irregular verbs. In proportion as the new institution became a success the drudgery would increase and the advantage diminish. The bigger a professor's class the less personal contact he can have with his pupils, till at last he has to rely for any influence at all on the stimulus of lecture-room oratory. As Mrs. Garrett-Anderson has, it seems to me, correctly pointed out in the *Times*, there is very little really to be said in favour of anything like a great central teaching institution for such a city as London.

The other alternative is to combine University and King's Colleges into a teaching University. But can this be regarded as in any way a statesman-like proposal? Why should two out of many institutions be picked out for University honours? And can anyone really suppose that such a settlement would have any finality about it? Why, for example, should Bedford College be left out, developing, as it apparently is, in usefulness and activity every day? Then how can the Royal College of Science at South Kensington be ignored? It is already in popular esteem ranked as a University, and bids fair to become in time in actual fact the great science University of the country. Why, too, ignore the City and Guilds Institute? It is difficult, then, to believe that a teaching University founded on University and King's Colleges can be regarded as in any way a final solution of the problem. If it is sought in this direction it must be based on a wider federation of institutions of academic rank. But in this case all the teachers will have something to say as to the conditions of common examination. Yet, according to Prof. Lankester, the essence of a true teaching University idea is the "absence of examiners—the professor himself is examiner and teacher in one." Schedules will nevertheless reproduce themselves, and the influence of colleagues will be quite as much an obstacle to the independence of the individual professor as the oppression of boards of examiners.

Furthermore, it is quite a mistake to suppose that unless the existing University is abolished, it will be possible for a younger one to escape its influence. Notwithstanding the establishment of the Victoria University, it is still found necessary, and at the request of Owens College, to hold the examinations of the University of London in Manchester. Consequently, the professors of Owens College have to adapt their teaching to a double curriculum. If the proposed University of Westminster were founded, it cannot be doubted that the same thing would happen. The professors would still have to bow their necks to the yoke of the Burlington Gardens schedules.

*Expansion of Existing University.*

It may be taken as quite certain that the existing University of London is too well rooted in the esteem of the community to be got rid of. Nor, with its own consent, will it readily submit to be mutilated or dismembered. And its pride and confidence in itself admits of easy justification. With all its demerits it can hardly be denied that it has accomplished a great work in raising the standard, throughout the country, of academic education. This need not be wondered at, seeing that it has always succeeded in enlisting in its service the most accomplished and distinguished men in every branch of education. If examination is to be conducted at all, I can hardly imagine conditions more favourable to its conduct than the University of London affords.

Instead of trying to diminish and curtail the usefulness of an institution which has such strong claims on public gratitude, I prefer to make the suggestion—and it is odd that it should have any novelty about it—that the future needs of University education in London should be provided for by an expansion of the existing University. This has always been the ambition of Convocation, and many, I know, share my own opinion that, if the Senate would have given greater heed to the representations which the former body has from time to time made to it, the present crisis in the history of the University would never have arisen.

I will briefly indicate the by no means drastic changes by which this might be gradually provided for.

*Organization of the Faculties.*

I am myself personally impressed with the conviction that the first step that should be taken in the interests of the higher education in London, and of those parts of the country which look to London for academic guidance, is the organization of the faculties. Everyone is agreed, whatever view they take on the examination question, that the teaching bodies should be brought into as intimate a relation as possible with the central University. At present there is no recognized channel of communication between them, and it has been long felt that this is a great evil. Examination is an art, and it is a progressive art. To minimize its possible harmfulness it should keep touch with the teaching. And it must be admitted that the system which now obtains at the University of London does not make this always easy. The Senate is hard to move and slow to act. This would not be so if those who had the right to move it possessed the momentum which would be derived from a more obvious authority. In fact this tendency to inaction arises from a natural timidity. The Senate is too largely composed of persons who have no direct touch with actual education.

The momentum to which I have referred above would come with all needful force from the faculties if they were organized in a comprehensive way to include every competent authority in academic education in London. I will not stop to discuss the precise machinery by which this should be brought about. It seems to me that it would be probably sufficient if the Senate were to have power to admit to the faculties the teachers of all institutions of academic rank which supplied it with candidates. To these should be added the past and present examiners, a certain number of non-graduates conspicuous for their distinction in the subjects with which the faculty was occupied, and a proper proportion of members of Convocation.

Such a body would occupy itself with any and every subject relating to academic education. Its resolutions would embody the deliberate conviction of instructed and competent persons, and would afford the Senate a solid basis for administrative procedure. I need hardly say that the faculties—if they took, as I doubt not they would do, a just view of their functions—would look to the advance of academic interests as a whole; they would not seek the sole advantage of the central University, but would watch and work for the interests of the collegiate institutions they represented—whether in London or the provinces—as well.

*Boards of Studies.*

Delegations from the faculties should be intrusted with the duty of watching the examination work and advising the Senate thereupon. This they would do in two ways: (1) they would consider from time to time all alterations necessary in the schedules so as to keep the examinations as closely as possible in touch with the best teaching; (2) they would review the conduct of the examinations, though without in any way interfering with the examiners. It would be their duty to consider the papers set, and criticize them if necessary, and they would consider and report on any apparent variation in the standard as evidenced by any sudden change in the percentage of passes and rejections.

*Reform of the Senate.*

I think it is generally admitted that the time has come when some change in the constitution of the Senate is advisable. At present it is an assembly of notables appointed for life. Many of them never attend, and some, appointed apparently on purely political grounds—and these are not always the least competent—never perhaps have attended. On the whole, the Senate, though individually eminent, is, it must be confessed, ill-informed on educational matters. As I have already hinted, it is apt in consequence to be somewhat timid and irresolute when it ought to act with decision; it is equally apt, I am afraid, to act with precipitancy when it ultimately realizes the necessity of moving at all.

The Senate must, however, remain the supreme governing body with whom the final decision must always remain in matters of importance. This being so, it seems not too much to ask that it should be an efficiently constituted body, and that the members should attend to their duties. Tenure of office for life it would seem desirable to abolish, and prolonged absence from attendance, say for a year, should *ipso facto* vacate a seat. As for the Crown nominees, who are in great part statesmen of high rank, it would be on obvious grounds unwise to dispense with them, if they took, as many of them do, sufficient interest in the work to attend with some regularity. Where the Senate needs strengthening is in experts in academic education; and it appears to me that the faculties, if constituted as above, might be intrusted with the duty of selecting these members of the Senate from their own ranks. On the whole, it might be convenient to constitute the Senate something on the lines of the Hebdomadal Council at Oxford: a third to be appointed by the Crown, a third to be appointed by the faculties, and a third by Convocation.

*Higher Teaching.*

There is still, however, one direction in which the University of London might even more closely associate itself with actual teaching, and so far become in actual fact a teaching University. This was pointed out in 1872 by the late Registrar, Dr. Carpenter, in his evidence before the Royal Commission on Scientific Instruction. He said (Minutes of Evidence, 10,925), "I think it very important that the State should provide for the carrying on of those higher researches, and that higher teaching, which are not provided for in any shape at present." Again (10,926), "I think that a body like the University of London might very advantageously be empowered to take up such higher and more special teaching. At present the University of London has nothing to do with teaching. The principle of the University is to recognize existing institutions. I do not think that it would be at all the function of the University to interfere or compete in any way with the institutions which it recognizes. But I should myself be very glad to see the University empowered to carry out courses of instruction of a higher and more special kind than are given in any of the institutions affiliated to it." The scope of this higher teaching was brought out more clearly in a subsequent part of Dr. Carpenter's evidence in answer to a question of Prof. Henry Smith's (10,953). He asked, "The Senate might at some future time endeavour, might they not, to have such lectures given in connection with the University of London as are now given in the Collège de France?—Yes, more of that character."

Such lectures would serve for the post-graduate study, provision for which seems to me the great defect in University education as it exists in London. And the professorships themselves would be positions which could be filled by eminent scientific men whom it is difficult as things are to retain in the capital. To take biological subjects as an example, the continual draining away of men like Michael Foster, Burdon Sanderson, and Lankester seems to me a real loss to the intellectual life of London.

It is just possible that it may be objected that the proposal to have a superior professoriate attached to the University is in some degree a slight on the Colleges and their teachers. And it may be urged that, if there were any demand for post-graduate teaching, the Colleges are quite competent to provide it. It may be so; but in practice I do not believe it feasible. The working day is inelastic, and from what I myself know of the labour involved in what may be called systematic graduation courses, I do not believe that the same man can superadd the higher work as well. Besides, to be of any value, it must not be formal and perfunctory; the essence of the higher teaching is that it should reflect the research to which the occupant of each chair should be able to devote the whole of his time.

I do not think that such professorships will be founded as long as the University is under the control of the State. For this and other reasons I should gladly see the University cease to be a quasi-Government institution, and launch out on its own resources. It seems almost incredible, but it is a fact, that at the present time not the slightest alteration can be made in a schedule without the approval of the Home Office, or the slightest alteration in the amount of prizes without that of the Treasury. There is no inducement now to the public to provide endowments, because, as the University nearly pays its way, any public benefaction would only tend to create a surplus, which would have to be paid over to the Exchequer. But I can hardly doubt that if the University were cut adrift from the State it would receive endowments which would enable it from time to time to found useful and important chairs. These would form not an unwelcome addition to the too few prizes accessible to those who devote themselves to learning for its own sake.

I had it in my mind to say a few words about the very complicated but independent problem which medical University education in London presents. But this paper has already run to an intolerable length, and the subject is perhaps of limited interest to the readers of Nature. But I may say that I believe that the organization of a strong medical faculty would bring about the solution of all existing difficulties.

W. T. Thiselton Dyer.

Royal Gardens, Kew, May 18.

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A note in the last issue of Nature (p. 39) seems to assume that the present University of London is nothing but an Imperial Examining Board that has got wrongly named, and stands in the way of London possessing the educational advantages of a German University town.

I venture to offer some facts and considerations which may modify this view, and perhaps aid in forming a juster conception of the real nature of the University question than is commonly entertained.

Much more important matters are involved in the question than the maintenance or extension of existing institutions, though these are quite legitimate subjects of discussion and defence; and in the columns of NATURE it is only upon the broad ground of the advancement of science and learning that the question can be dealt with.

The epithet "Imperial" is intended to imply some unfitness on the part of the present University for other than "Imperial" functions, whatever these may be. But the University has not, and never has had, the least claim to any such title. It has never at any time held colonial examinations of its own motion. It has never at any time held any colonial examinations whatever in the faculty of science, or in the faculty of medicine, or for honours in any faculty, or for any of the higher degrees. What examinations it holds in any colony are held only at the request of the Governor of the colony, transmitted through the Colonial Office, and are practically confined to matriculation and the intermediate examination in arts. Occasionally, but very rarely, an examination in laws or for the Bachelor of Arts is held in some colony. In 1890, 16 candidates matriculated in the colonies, and 5 passed the intermediate examination in arts out of a total of some 5000 candidates. Not a single degree examination was held in any colony. In fact, these colonial examinations, which, few as they are, yearly diminish in number, never formed part of the University scheme. They were instituted about 1864 at the request of the colony of Mauritius, but were extended and have been maintained chiefly to facilitate the award of the scholarships at the disposal of the Gilchrist Trustees. Not only is the University of London not an Imperial University, but it is even less British in character than probably either of the older Universities. Very few of its candidates come from Scotland, fewer still from Ireland, and my strong impression is that the great majority come from midland and southern England. I should not be surprised even to find that a considerable majority are now drawn from an area having London for its centre with a radius of not more than 100 miles. The probable establishment, at no very remote period, of provincial Universities will practically give a still more exclusive sense to the name Unisity of London.

It may next be asked what precisely is meant by a "teaching University in and for London," the creation of which is constantly put forward as the principal educational need of the metropolis. Is the proposed University to be "for" London in some sense in which the University of Oxford is not "for" Oxford, or that of Edinburgh not "for" Edinburgh? I know of no University, British or German, which is "for" the particular town or district in which it has its local habitation. Or is the proposed University to be "for" London in some sense in which the existing University is not "for" London as well as the rest of the country? The words seem mere surplusage, unless intended to impose local limitations which no University has ever yet imposed upon itself.

The expression "teaching University," too, stands in need of exacter definition. The University of Edinburgh is a teaching University, so is that of Dublin, so are the German Universities. Oxford and Cambridge are only in part teaching Universities; the greater part of the teaching is done by the Colleges. The Victoria University is not, in fact, a teaching University at all; the teaching is the work of its Colleges, and the proposed 'teaching University in and for London" would, as far as actual teaching is concerned, resemble the Victoria University rather than a Scotch or German University. At this point the *crux* of the whole question reveals itself. The really distinguishing feature of the new University as contrasted with the University of London would be the examination of collegiate candidates (and those only) by their teachers in alleged conformity with the principle that examination should follow teaching. But it may be admitted that teaching ought to be adapted to examination, or examination to teaching, without admitting any advantage in the system of teachers settling the examination of their own students, collegiate or not. The combined teacher-examiner system is not wholly trusted by its supporters. At the older Universities the examiners are by no means usually the teachers of the candidates; at the Victoria University one of the examiners is always an "external" one. I am not quite sure how the matter stands at the Scotch and Irish Universities. To assert that such partial or semi-partial modes of testing knowledge are superior to disinterested and independent methods is merely to make an assumption, announce an opinion. What comparison of the working of both systems proves any superiority on the part of the first-mentioned of them? Do the pass degrees of Scotch or Irish Universities, or even of Oxford or Cambridge, stand higher than those of London?

Further, is it not misleading to characterize the University of London as a mere Examining Board? Of the three functions of such a teaching University as that of Edinburgh, it performs two. It directs teaching by syllabuses and regulations (prepared with extreme care, and not without ample reference to the best authorities on all matters of special knowledge), and it tests teaching by absolutely impartial and disinterested examinations, but it does not—without space, funds, and appliances it could not—pretend to teach. Nothing, however, in its nature or essence forbids its development, alone or in union or conjunction with other institutions, into what would be an ideal University of the non-residential order, neither coercive nor exclusive—one that should offer proper University instruction to all comers, and, at the same time, confer degrees upon open examinations independently (save for obvious reasons in relation to medical degrees) of place or mode of instruction.

The part the existing University of London has played in the advancement of learning may be indicated by the fact mentioned by the Vice-Chancellor in his Presentation speech, that during the last thirty years—that is, since its examinations were thrown open—the number of degrees conferred by the University has increased tenfold. This, however, is only one of the ways in which its influence is shown; the great advance in scientific education the last fifty years have witnessed is almost wholly due to the stimulus and example of the University of London. But the subject is too large a one to be dealt with on the present occasion, and indeed, from its nature, scarcely lends itself to treatment capable of doing full justice to the University.

The work of a University should not be confined to the education of graduates. Its crowning function is the exposition and illustration of the higher learning along the whole line of advance. Such is the task so admirably accomplished by the Sorbonne and the Collège de France, and to the world of science and learning in London the University of London is peculiarly well adapted, by its independence and impartiality, to render similar services. Some years ago an attempt was made to work out a scheme having this end in view, but, in deference to reasons that no longer exist, it was found necessary to abandon its further prosecution. Its resumption has now become, or may shortly become, simply a question of means, and the time is at hand when a strong effort ought to be made to afford scholars and men of science in London some of the advantages their brethren have so long enjoyed in Paris.

Richmond, May 19. F. VICTOR DICKINS.

### Co-adaptation.

WRITTEN letters remain. It is for anyone who may read this correspondence through at one time to judge on which side lie the "valid" distinctions, and on which the "invalid" confusions—not to mention comparisons in respect of "verbiage" or mere personalities. But I am obliged to write once more to insist, for the fourth time, that my agreement with Prof. Meldola does *not* extend to the "conclusion as to the non-existence of co-adaptation," but only to stating that co adaptation must be proved not to exist, if "Mr. Spencer's argument" is to be logically met. And if, as Prof. Meldola now says, any such statement is to be found in his "review of Mr. Pascoe's book" (which, I repeat, merely reproduces "Mr. Wallace's argument" as to the *accumulation* of *adaptations*, without remarking that this has no relevancy to the argument from *co-adaptation*), it must be in that "language of their own" which the neo-Darwinians find "to be intelligible among themselves."

Christ Church, Oxford, May 15. GEORGE J. ROMANES.

### *A priori* Reasoning.

I SEEM to have failed to make my contention clear to Mr. Cockerell, and will try once more. What I maintain is this:

that it is unscientific—unphilosophical—to state an hypothesis or formulate a theory, and much more so to make a categorical statement, when no antecedent facts are given nor any subsequent verification attempted. Thus, Mr. Cockerell asks the question, "Why is it that plants growing on exposed sea-shores have a tendency to lie upon the ground or otherwise *to evade the violence of the winds*" (my italics)? Now, what evidence has he to bring forward that the *purpose* of lying down is to evade the violence of the winds? So far, it is nothing more than his private opinion—an *a priori* assumption. It is true that he adds a reason, but it is also drawn from his own consciousness, and not from nature: "When a plant is growing among others, it has to compete with them in raising itself into conspicuousness." But do not dwarf plants ever compete? My experience of the South Downs, where plants are for the most part considerably dwarfed, is that the struggle between them is a severe one. Yet their flowers and foliage are fully exposed to sunlight and insects, as well as to severe gales of wind. Mr. Cockerell also appears to forget that what is true for one plant is true for another, and each must try to overtop all the others.

I would venture to warn our younger naturalists most earnestly against this *facilis descensus* of *a priori* reasoning without facts or verification. It has been the bane of metaphysics; and when a scientific man like Dr. Weismann puts forth, in the name of science, most deplorable illustrations of it in his late attempt to apply his theory to plants, it is time that some one should venture to protest.

In reply to his request, I would refer Mr. Cockerell to M. Verlot's pamphlet "Sur la Production et la Fixation des Variétés," in which he describes his method of creating and fixing dwarf plants by sowing seed late in the season. Also to M. Roujou's experiments in selecting the smallest seeds of plants (*Journ. d'Hist. Nat. de Bordeaux et du Sud-Ouest*, 1884). Mr. McNab also raised dwarf rhododendrons by using pollen from the smallest stamens. Want of space forbids me adding more on the subject. GEORGE HENSLOW.

### The Natural Selection of Indian Corn.

IN a former letter I had occasion to mention that *Zea mais* varies in its period of maturing, and that at certain altitudes and latitudes, only some of the varieties (*i.e.* the early maturing) are able to mature at all, the rest being absolutely eliminated by natural selection in a single generation. A few days ago I received, through the kindness of Mr. James Fletcher, the new (1891) Report on Experimental Farms for 1890, published by the Canadian Government, in which are numerous statistics of experimental planting in different parts of the Dominion. On p. 296, Mr. T. A. Sharpe gives an account of the result of planting twenty-nine different varieties of Indian corn at Agassiz, British Columbia, which perhaps deserves notice, as illustrating the above-mentioned facts in a particularly clear way. Of the varieties planted (all exposed to the same kind of environment), the majority did not form any ears at all. Some formed very small ears, and others reached various stages of maturity, but only a very few actually matured.

For example, I will quote some of them:—

No. 1. Moore's Early Concord, corn matured, one of the best.
No. 3. Early Adams, corn matured to glazing stage.
No. 6. Mitchell's Extra Early White Flint, produced some matured ears.
No. 11. Marblehead Sugar, matured corn, ears very small.
No. 12. Narraganset, sweet, corn did not fill to tips of cob.
No. 14. Chester Co. Mammoth, no corn formed.
No. 21. Golden Dent, no ears formed.

T. D. A. COCKERELL.

3 Fairfax Road, Bedford Park, Chiswick, W., May 10.

### The Soaring of Birds.

IT seems a great pity that the simpler form of this question—wherein birds soaring steadily rise, in a gentle breeze, over a large plain—is needlessly complicated by the flight of sea-birds over waves.

We shall get the solution best by taking the former and less complicated case, wherein the pelicans, adjutants, cyrus, vultures, &c., slowly rise, by soaring alone, to great heights, under conditions where up-rushes of air are quite out of the question.

Upper Asam is a dead level, some 60 miles wide by 200 long, and over this area, wherein these birds rise by soaring alone, the air-drift is almost invariably from north-north-east, or else south-west, and at about 5 to 10 miles an hour. They do not seem to rise in a dead calm, nor yet in stormy weather, and I take it the desideratum is a slow air-drift, or gentle *steady* breeze.

That there are no up-rushes of air, I have fairly good proof in the small tufts of cotton, from the *Bombyx malabaricum*, which cross the field of my telescope when examining the Noga Hills at 10 to 20 or 30 miles; these are always beautifully horizontal at elevations from 200 to 2000 feet, coming from the plains and hills north-east of us.

So that out here there is no complication of the case by vertical movements of the air, as at sea. The question is not how large birds sustain themselves (without flapping their wings) in a wind, when there are rising and falling and strata of "different velocities"; but how large birds like the cyrus, adjutant, pelican, and vulture can rise from 300 to 3000 feet, in a *steady* breeze, without flapping their wings.

It is not mere flotation; they have to *raise* 20 or 30 pounds some 2000 feet, in addition to what the albatross does.

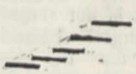
Surely this is the major question, at once simpler to see, and more difficult to answer.

In NATURE (vol. xxiii. p. 10) I drew attention to this, and sent a small diagram, to show how I thought it was done. I have frequently observed the phenomena since, and see no reason to modify my views.

Firstly, these large birds do not soar in a dead calm, or a storm, or during high winds. They prefer a steady breeze.

Secondly, they rise from the ground, by flapping the wings, and continue this till they are 100 or 200 feet up, and then begin to soar, in right or left hand spirals, 100 or 200 yards across. At each lap they rise 10 or 20 feet, and make as many yards leeway, drifting slowly *with the wind*, and continue thus to rise until out of sight above.

With a good telescope a bird can be easily followed after a little practice, and the only motion which can be seen is slight and occasional movement of the tail, in steering.

The legs (of the waders) are extended at full length behind, the neck thrown on the back, and beak projecting over the breast . The tips of the primary wing-feathers are always well separated in different planes, , and strongly curved up, thus,  evidently under great strain.

The lifting power is evidently applied to them mainly, and the plane of the outspread wings is not horizontal, but forms part of an obtuse, inverted cone, as though a little centrifugal force was implicated.

The speed of the bird is always greater than the breeze, and the resistance is unequal on opposite sides of the loop of the spiral; least when it travels with the breeze, and greatest when on the opposite half, meeting it.

It seems to me the solution is that, when going with the wind, the bird gathers momentum by going down a slight incline, and when it turns and meets the breeze, this extra momentum is used in lifting the bird and carrying it over a *shorter* course. Thus it starts the next lap at a *slightly* higher level, but some 20 yards to leeward. Variation of the speed of the wind at different levels is here quite out of the question; the bird, too, keeps to its steady spiral, and as steadily ascends at each lap.

I feel sure that Prof. Tait, Sir W. Thomson, and Lord Rayleigh will find the case I state a more profitable one to study than the erratic flight or floating of sea-birds. The telescope I use to watch and follow these birds when soaring is a 3·5″ O.G. power 50, with long tripod legs, and on a mattrass below I find no difficulty in keeping a bird in the field, if at 1000 feet up. My own idea is that all these birds go up there to sleep or doze.

Sibsagar, Asam, March 30. S. E. PEAL.

## ON SOME POINTS IN THE EARLY HISTORY OF ASTRONOMY.[1]

### III.

WE now come to the important point for our present inquiry—the direction in which the temple is built, or, technically, its orientation. Confining ourselves for the moment to Karnak, is there any meaning in the direction of that line, some 500 yards long, which is obviously the main feature of the building, and to which all parts are accessories?

How can we instrumentally determine this? I have the necessary apparatus here, and the question may be answered in a few minutes; we have simply to determine either the azimuth or the amplitude (and as we have seen one of these gives the other) of the point of the horizon towards which this long line is directed.

The azimuth compass is an instrument familiar to most of you. It consists of a magnetic needle fastened

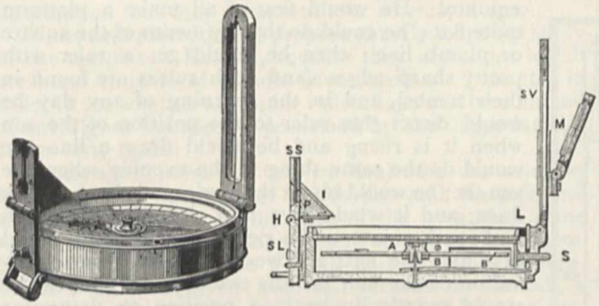



FIG. 11.—Azimuth compass. SV, direction pointer; M, sun shade (for observations of sun); I, reading prism; A B, graduated card with attached compass-needle.

to a card carrying a circle divided into 360°, which can be conveniently read by a prism when the instrument is turned toward any definite direction marked by a vertical wire.

A theodolite armed with a delicately hung magnetic needle which can be rotated on a vertical axis will do equally well; it has first of all to be levelled; there is a little telescope with which we can see along the line.

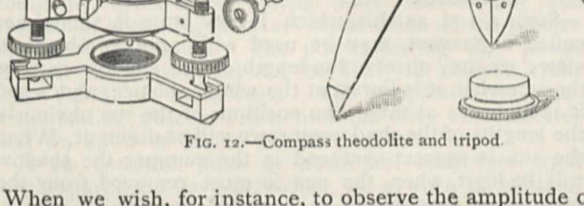


FIG. 12.—Compass theodolite and tripod.

When we wish, for instance, to observe the amplitude of a temple, the theodolite is set up on its tripod in such a position that we can look along the temple wall or line of columns, &c., by means of the telescope. We then get a magnetic reading of the direction, after having unclamped the compass; the compass showing the angle made between the line and the magnetic north (or south), as in the azimuth compass.

Having made such an observation as that I have described, the next thing we have to do is to determine astronomically the real north, which is the only thing of value. There are two ways of getting this *astronomical* bearing or azimuth.

It is sad to think how much time has been lost in the investigation of a great many of these questions, for the reason that the observations were made only with reference to the magnetic north, which is vastly different at different places, and is always varying; few indeed have tried to get at the astronomical conditions of the problem. Had this been done either by the French or Prussian Commissions to which I have referred, it is perfectly certain that years ago the solstitial orientation of Karnak and other temples which I shall have to demonstrate to you would have been long known to all scholars.

If the magnetic variation has been determined for the region we may use a map. Such a map as that shown in Fig. 13 gives us the lines along which in the British Isles the compass variation west of north reaches certain values. From such a map for Egypt we learn that in 1798 a magnet swung along a line extending from a little to the west of Cairo to the second cataract would have had a variation of 12° to the west; in 1844 of 8° to the west; and at the present time the variation is such that observations made along the same part of the Nile valley will have a variation closely approximating 5° to the west. By means of such a map it is quite possible to get approximately the astronomical bearings of all temples which were observed by the French in 1798 or by the Germans in 1844, or which can be observed in the present day.

If we are not fortunate enough to possess such a map, the theodolite will enable us to observe the direction in which the sun culminates at noon. This gives us the south point astronomically. From observations of the pole star at night, the astronomical north can be determined. From either of these observations the magnetic variation is obtained without any difficulty.

This being premised about the method, we next come to the results. The amplitude of the point to which the axis of the great temple at Karnak points is 26° N. of W., which we learn from the table already given is precisely the amplitude of the place of sunset at the summer solstice. The amplitude of the point to which the axis of the small temple points is 26° S. of E., exactly the position of sunrise at the winter solstice.

There is more evidence of this kind. Abydos, one of the oldest temples in Egypt, built, according to tradition, by the servants of Hor, is now, it is true, a heap of ruins, the brick walls best showing its direction; but it is possible to gather the orientation of it by these guiding lines. It is 27° N. of W.—as it should be, being in a higher latitude than Karnak—and evidently was oriented to the solstice.

At Abydos, then, as at Karnak, we get exactly, within a degree, the amplitude shown in the tables for the sun in the Nile valley at sunset at the summer solstice. So that the Egyptians who were employed in building those temples must have known exactly what they were going to do, and what they did was to build a temple such that the sun at setting should, at the summer solstice, pour its light along the axis of the temple. If Maspéro and the great authorities in Egyptian archæology are right—namely, that the Abydos temple was founded before 4000 B.C.—and if we can depend upon the French figures, we are driven to the conclusion that we have in this temple a building which was orientated to the solstitial sunset place in the valley of the Nile. The Nile valley holds other solar temples besides those we have named, but it is best to fully study Karnak; instead of being a mere heap, the orientation of which is obtainable only by the general lie of the remains, this temple is still in such

[1] Continued from p. 11.

preservation that the Germans, in the year 1844, could give us an infinite number of details about it, and locate the position of the innumerable courts. Its orientation to the solstice we can claim as an early astronomical observation. So it is quite fair to say that, many thousand years ago at all events, the Egyptians were perfectly familiar with the solstices, and therefore more or less fully with the yearly path of the sun.

But so far we have only dealt with solstices. Did the Egyptians know anything about the equinoxes? Certainly. Nothing is more remarkable than to go from the description and the plans of such temples as we have seen at Abydos and Karnak to regions where, apparently, the thought is totally and completely different, such as we find on the Pyramid Plains at Ghizeh; the orientation lines of the German surveyors show, beyond all question, that these structures are just as true to the sun-rising at the equinoxes as the temples at Abydos and Karnak were to the sun-rising and setting at the solstices,


Fig. 13.—Map of British Isles showing the magnetic variation.[1]

and the Sphinx was merely a mysterious nondescript sort of thing which was there watching for the rising of the sun at an equinox, as the Colossi of the plain at Thebes were watching for the rising of the sun at the winter solstice.

The observations which have been made in Babylonia are very discordant among themselves, and at present it is impossible to say, from the monuments in any of this region along the Euphrates valley, whether the temples indicate that the solstices were familiar to the Babylonians; but no doubt some of the temples were as perfectly squared to the equinox as some walls at Memphis or the Pyramids at Ghizeh; and certainly there is no doubt that as early as Solomon's time the temple at Jerusalem was orientated to the east with care. We find there that the direction of the axis of the temple shows the existence of a cult connected with the possibility of seeing the rising of the sun on the day of an equinox, possibly at the time which we now call Easter.

All the doors being opened, the sunlight would penetrate over the high altar, where the sacrifices were offered, into the very Holy of Holies, which we may remember was only entered by the High Priest once a year.

Have we any other evidence except the evidence afforded by temples? Yes. It has been stated that we have no temple evidence from China, but there is a good deal of written evidence, and there is no doubt that in China the solstices and the equinoxes were perfectly well known 1100 years B.C. Was it difficult to obtain this knowledge? Did it indicate that the people were great astronomers? Nothing of the kind; nothing is so easy as to determine a solstice or an equinox.

We know from the Egyptian tombs that their stock-in-trade, so far as building went, was very considerable; they had squares, they had plumb-lines, they had scales, and all that sort of thing just as we have. Suppose an Egyptian wished to determine the time of an equinox. He would first of all make a platform quite flat; he could do that by means of the square or plumb line; then he would get a ruler with pretty sharp edges (and such rulers are found in their tombs), and in the morning of any day he would direct this ruler to the position of the sun when it is rising and he would draw a line; he would do the same thing in the evening when the sun set; he would bisect the angle made by these two lines, and it would give him naturally the north and south points, and a right angle to those would give him the east and west. So that from observation of the sun on any two days in the year he would practically be in a position to determine the position at which the sun would rise and set at the equinox.

There is another way of doing it. Take a vertical rod. Suppose that the sun is rising, let the rod throw a shadow; mark the position of the shadow; at sunset we again note where the shadow falls. If the sun rises exactly in the east and sets exactly in the west, those two shadows will be continuous and we shall have made an observation at the absolute equinox. But suppose the sun not at the equinox, a line joining the ends of the shadows equally long before and after noon will be an east and west line.

It is true that there may be a slight error unless we are very careful about the time of the year at which we make the observations, because when the sun is exactly east or west at the time of rising or setting it is moving most rapidly. So it is better to make the above observations of the sun nearer the solstices than the equinoxes, because the sun changes its declination most quickly at the equinoxes.

Such a rod as this, which I may state is sometimes called a *gnomon*, may be used with another object in view: we may observe the length of the shadow cast by the sun when it is lowest at the winter solstice, and when it is highest; at these two positions of the sun obviously the lengths of the shadows thrown will be different. When the sun is nearest overhead in the summer the shadow will be least, when the sun is most removed from the vertical the shadow will be longest.

The day on which the shortest shadow is thrown at noon will define the summer solstice; when the shadow is longest we shall have the winter solstice.

This in fact was the method adopted by the Chinese to determine the solstices, and from it very early they found a value of the obliquity of the ecliptic.

It may be said that it is only a statement, and that the record has been falsified; some years ago anyone who was driven by facts to come to the conclusion that any

[1] For Figs. 11–13 I am indebted to the kindness of Mr. Stanley, Great Turnstile, Lincoln's Inn Fields.

very considerable antiquity was possible in these observations met with very great difficulty. But the shortest and the longest shadows recorded (700 years B.C.) do not really represent the true lengths according to recent knowledge. If anyone had forged these observations he would state such lengths as people would find to-day or to-morrow, but the lengths given were different from those which would be made to-day. Laplace, who gave considerable attention to this matter, determined what the real obliquity was at that time, and proved that the record does represent an actual observation and not one which had been made in later years.

The solstices and the equinoxes were therefore in all probability thoroughly known to the Egyptians 4000 years B.C., perhaps even 5000. We are then justified in considering that the temples at Abydos and at Karnak are really solar temples. The Egyptians marked the solstices and the equinoxes not only by their temples but in their calendars, which these temples enabled them to construct. The Chinese had also this knowledge, but we have no information that they possessed it at so early a date.

In the next place, then, I propose to make a special study of the temples at Karnak, because they are those which are most capable of minute investigation. I do this in order to see whether any other indications can be obtained of any higher knowledge possessed by the Egyptians of those early times.

I must again point out that we deal with the solstices in the case of the temples at Abydos and Karnak, and with the equinoxes in the case of the pyramids, some mounds in Babylonia, and the Temple at Jerusalem. Since the labours of the French and Prussian Governments who have given such full records of Karnak, a memoir on the temples has been published by Mariette, which gives us not only plans, but precious information relating to the periods at which, and the kings by whom, the various parts of the temples were constructed or modified.

We may begin by the general plan of Thebes. We find there a perfect nest of temples. No doubt those which are still traceable form only a very small portion of those which once existed, but however that may be, I have now only to call attention to one or two among them. In the general plan we see indications that on both sides of the Nile there were temples pointing to those special amplitudes which I have before referred to. What we have first to do is to refer to the solstitial temples, those which point to 26° N. or S. of E. or W., in which we have undoubtedly indications of the early attempts to observe, or to worship, the sun at sun-rising and at sun-setting, at the critical times—the solstitial times of the year.

The first point that I wish to make is that these temples—whatever views may be entertained with regard to their worship or the ceremonial in them—were undoubtedly constructed among other reasons for the purpose of obtaining an exact observation of the precise time of the solstice. The priests having this power at their disposal, would not be likely to neglect it, for they ruled by knowledge. The temples were, then, astronomical observatories, and the first observatories that we know of in the world.

If we consider them as horizontal telescopes used for the purpose I have suggested, we at once understand the long axis, and the series of gradually narrowing diaphragms, for, the longer the beam of light used, the greater is the accuracy that can be obtained.

It is worthy of note that the direction of the temple at Karnak is quite independent of the locality, it has nothing to do with the presentation of the temple to the Nile or to any other particular part of the landscape, and that point, I think, is absolutely settled by the consideration that we have temples at the same amplitude in different localities up and down the Nile Valley, where, although they are parallel to each other, their presentation to the river in the different localities is very various.

What then was the real use of these pylons and these diaphragms? It was to keep all stray light out of the carefully roofed and darkened sanctuary; but why was the sanctuary to be kept in darkness?

Independently of ceremonial reasons—there is a good deal to be said under that head—it is quite clear that the darker the sanctuary the more obvious will be the patch of light on the end wall, and the more easily can its position be located. It was important to do this on the two or three days near the solstice in order to get an idea of the exact time at which the solstice took place. We find that a narrow beam of sunlight coming through a narrow entrance some 500 yards away from the door of the Holy of Holies would, provided the temple were properly orientated to the solstice, and provided the solstice occurred at the absolute moment of sunrise or sunset according to which the temple was being utilized, practically flash into the sanctuary and remain there for about a couple of minutes, and then pass away. The flash would be a crescendo and diminuendo, but the whole thing would not last above two minutes or thereabouts, and might be considerably reduced by arrangements of curtains. Supposing the solstice did not occur at the precise moment of sunrise or sunset, and provided the Egyptians by any means whatever were able to divide the days and the nights into more or less equal intervals of time, two or three observations of the sun-rising at the solstice on three different mornings, or of the sunset at the solstice on three different evenings, would enable a careful observer to say whether the solstice had occurred at the exact moment of sunrise or at some interval between two successive sunrises, and if the latter, what that interval was.

I now come to my next point, which is that here we have the true origin of our present means of measuring time—that our year as we know it was first determined in these Egyptian temples and by the Egyptians. We have seen that it did not require any great amount of astronomical knowledge to determine either the moment of the solstice or the moment of the equinox. I think you will agree with me that the most natural thing to begin with was the observation of the solstice, for the reason that at the solstice you can watch the sun day after day getting more and more north or more and more south until it comes to a standstill. But for the observation of the equinox, of course, the sun is moving most rapidly either north or south, and therefore it would be more difficult to determine in those days the exact moment, so that I have little doubt that what they attempted in the first instance was to mark the absolute moment of the solstice. If that be so, and if Maspéro is right that Abydos was built before Menes, then we know definitely that the Egyptians could and did observe the solstices, and knew what they were doing, 7000 years ago.

Before I say anything more about the use of these temples in determining the year, it is worth while to note how very different the treatment of this subject was in Egypt to what it was in Chaldæa and Babylonia and among the Jews. We do of course in the Egyptian inscriptions read of the moon, but in Chaldæa it would seem that the moon was the chief thing worshipped, and it was thus naturally the chief means used for measuring time, and, as far of course as months were concerned, this was quite right. In Chaldæa, where they were not dependent upon the rising of the Nile, and where much desert travel had to be undertaken at night, the moon and the month were the points considered, and the sun was hardly regarded at all from that point of view. An interesting point connected with this is that, among any of these ancient peoples, the celestial bodies which gave them the longest period of time by which they reckoned were practically looked upon in the same category.

Thus, for instance, in Egypt the sun being used, the unit of time was a year; but in Chaldæa the unit of time was a month, for the reason that the standard of time was the moon. So that when people began speaking about periods of time it was quite easy for one nation to conceive that a period of time was a year when really it was a month, and *vice versâ*. It has been suggested that the years of Methuselah and other persons who are stated to have lived a considerable number of years were not solar years but lunar years—that is, properly, lunar months. This is reasonable, since if we divide the numbers by 12 we find that they come out very much the same length as lives are in the present day.

The Egyptians, taking the sun as their measurer of time, began very early with a year of 360 days. For some reason or other they divided these 360 days into months, probably with some lunar connection, so that they had 12 months of 30 days. Now, we know that that is not the true length of the year, and it is clear that any nation which uses such a year as that will find its festivals going through the year. Further, such a year as that is absolutely useless for the agriculturist or the gardener, because after a time the same month, to say nothing of the same day of the month, will not mean reaping-time, will not mean sowing-time, or anything else. So that this 360-day year did not last very long; so long as it lasted, however, they knew that they got the seasons back to months of the same name in a period of 70 years.

This method led to complications, which possibly may have had something to do with the building of these temples. Egypt being exclusively the gift of the Nile, you can quite understand that their earliest calendar would be connected with the Nile, and so one finds it. We and other peoples occupying the zone in the north divide the year into four seasons; the Egyptians divided it, and still divide it, into three: they have four months of the flood of the Nile, then they have four months after the Nile has retired, in which they do their sowing, and then they have other four months which they call their summer, in which they gather their harvest.

We began, then, with a year of 360 days, and, having 360 days instead of 365¼, we had a cycle of 70 years, and during that cycle each day of the year meant something different with regard to the advance of the seasons, and with regard to the work of the agriculturist and the gardener to what it had meant in the preceding year. But this state of things did not last long. The 1st of the first month fell at the summer solstice on June 20, and the reason that it fell then was, that the inundation of the Nile reached Memphis on that day. Whether with the help of the temples or not, they soon got very much nearer, and changed the year of 360 for one of 365 days, which is, roughly, within a quarter of a day of the truth. They had still their 12 months of 30 days, and then they added an extra month of 5 days. With their perfectly orientated temples they must have soon found that their festival at the summer solstice—which festival is known all over the world to-day—did not fall precisely on the same day of the new year, because, if 365 days had exactly measured the year, that flash of bright sunlight would have fallen into the sanctuary just as it did 365 days before. But what they must have found was, that after an interval of four years it did not fall on the first day of the month, but on the day following it. They at once faced this, and found out that 365 days did not exactly make a year, but that they had to do with a quarter day in addition. What the Chinese did was this: every fourth year, instead of adding 5 days to their 360, they added 6 days, and in that way they practically brought the calendar right.

Theory indicated that retaining the 365-day year, the 1st of the first month would come back to its exact relationship to the inundation of the Nile after a period of 1460 years, the 1460 years of course depending upon the quarter being added ($365 \times 4 = 1460$).

This was known in Egypt to the priests alone. They would not allow the year of 365 days, called the *vague* year, to be altered, and so strongly did they feel on this point that every king had to swear when he was crowned that he would not alter the year. We can surmise why this was. It gave great power to the priests; they alone could tell on what particular day of what particular month the Nile would rise in each year, because they alone knew in what part of the cycle of 1460 years they were, and in order to get that knowledge they had simply to continue going every year into their Holy of Holies one day in the year as the priests did in Jerusalem, and watch the little patch of bright sunlight coming into the sanctuary. That would tell them exactly the relation of the true solar solstice to their year, which was supposed to begin at the solstice, and the exact date of the inundation of the Nile could be found by those who could determine observationally the solstice, but by no others.

In reading books on Egypt we come across another cycle which is supposed to be a very mysterious one; in fact it is one which, I think, has not yet been sufficiently investigated, and it is very well worth the trouble of anybody who will give the time. They begin with a year of twelve months, each of which has thirty days, thus giving 360 days; this was found not to work. They then tried 365 days, but that also would not work, because then the first day of Thoth (their first month) would only indicate the inundation of the Nile one year out of 1460; and then the priests interpolated the other day and got the cycle right, but it was not yet quite right. In the time of Hipparchus 365·25 did not really represent the true length of the solar year; instead of 365·25 we must write 365·242392—that is to say, the real length of the year was a little less than 365¼ days.

Now the length of the year being a little less, of course we should only get the absolute coincidence of the 1st of Thoth with the inundation of the Nile in a longer period than the 1460 years cycle; and, as a matter of fact, the 1460 years had to be expanded into 1506 to fit the months into the years with this slightly shortened length of the year; so we have a period which is called *sothic*, of 1460 years; and a period which is called *phœnix*, of 1506 years.

There is a great wealth of interest connected with the uses of the temples from the point of view of worship, but that does not concern us here, except that it is intimately connected with the next part of the subject, for I have next to point out that it necessitated in Egypt, Chaldæa, and elsewhere contemporaneous observations of the stars. I therefore now pass from the sun to the stars.

J. Norman Lockyer.

(*To be continued.*)

## *FORESTRY IN NORTH AMERICA.*

In continuation of the notes under the above heading which appeared in Nature last January, I wish to refer to a splendid paper[1] recently read by Sir Dietrich Brandis, F.R.S., to the Natural History Society of Bonn. It consists chiefly of a compilation from Dr. Mayr's book, "Die Waldungen von Nord America" (Munich, 1890), and from works by Prof. Sargent Bernhard Fernow, the present Chief of Forestry at Washington, and some other authors, as well as from the Agricultural Reports of the United States.

Dr. Mayr is the son of a Bavarian State forest officer, and, after studying forestry and botany at Munich, he was sent, at the expense of the Bavarian Government, to observe in their native forests, at different ages, certain important

[1] "Der Wald in den Vereinigten Staaten von Nord America," von Dr. D. Brandis in Bonn, 1891. (Sonder Abdruck aus den *Verhandlungen des Naturhistorischen Vereins*, 47 Jahrg.)

North American forest trees, experimental plantings of which have from time to time been made in Germany. After spending seven months on these researches, and extending his tour through Japan, Java, Ceylon, and Northern Hindustan, Dr. Mayr returned to Germany in 1888, and was shortly afterwards appointed Professor of Forestry and Forest Botany at the College of Agriculture and Forestry at Tokio in Japan. The present writer had the great pleasure of accompanying him in January 1888 for about three weeks through some of the coniferous and oak forests of the North-Western Himalayas and the subtropical forests of the lower hills near Dehra.

After leaving Germany a second time for Japan, Dr. Mayr had a further opportunity of visiting North America, and thus has twice traversed the length and breadth of the country between the Dominion of Canada and Mexico.

Mayr treats of the demands of the most important North American trees as regards climate and soil, with a summary account of their anatomical structure and of the physical and technical qualities of the most important woods, and his book contains numerous illustrations. He also gives lists of destructive fungi and insects observed by him on the different species.

Brandis has some criticisms to mete out for a few somewhat rash generalizations made by Mayr. These are that evergreen broad-leaved (not coniferous) forest requires a higher winter temperature than deciduous forest, and that deciduous forest vegetation is always absent in tropical countries on account of the uniformity of the climate throughout the year. Brandis shows clearly, from a comparison with the deciduous forests of teak and other species in India, Burma, and Java, that this statement will not hold wherever there is a prolonged dry season, which renders the trees leafless for a certain period of the year.

Another statement of Mayr's controverted by Brandis is that conifers never grow in tropical countries except where the altitude renders the climate non-tropical, and that in North America they have longer needles, supply heavier timber, and contain the more resin, the nearer they grow to the tropics. The latter statements may be true for *Pinus australis*, the pitch pine of the Southern States of North America, but do not hold good in India, where the *Pinus longifolia* of the Himalayas has the longest needles and probably yields as much resin as the tropical pine (*P. Merkensii*), which, however, has the heaviest wood of all the Indian pines, and grows in latitude 17° N., in Tenasserim, at about 600 feet above sea-level, in an absolutely tropical climate.

Mayr's statement that oranges will only grow to perfection in a hot dry climate is also not true for India, as oranges of splendid flavour are grown in enormous quantities in the damp lower hills below Cherapunji, in Assam, where the rainy season lasts for eight months, as well as in the dry regions near Delhi, and the comparatively dry country near Nagpur, in the Central Provinces of India.

Apart from these criticisms and an interesting discussion on the origin of prairies, we find in Brandis's paper a most complete account of the distribution of North American forest trees.

Forest vegetation is much richer in North America than in Europe, containing about 412 species, distributed as follows:—

| | |
|---|---|
| Atlantic region ... ... ... ... ... | 176 |
| Pacific region ... ... ... ... ... | 106 |
| Common to both ... ... ... ... ... | 10 |
| Central region on and surrounding Rocky Mountains ... ... ... ... ... | 46 |
| Tropical species near the coasts of Florida ... | 74 |
| | 412 |

as against 158 species in Europe.

At least six North American species of forest trees, according to Brandis, are also indigenous in Europe, being—

| | | |
|---|---|---|
| *Cercis canadensis* | = | *Siliquastrum* |
| *Diospyros virginiana* | = | *Lotus* |
| *Celtis occidentalis* | = | *australis* |
| *Platanus occidentalis* | = | *orientalis* |
| *Ostrya virginica* | = | *carpinifolia* |
| *Castanea americana* | = | *vulgaris.* |

All these species now grow naturally in Europe south of the Alps, and since many American forest genera existed in Europe in Tertiary times, whilst only five European forest genera (Ceratonia, Laburnum, Olea, Syringa, Laurus) are not found in America, it is possible that other species formerly common to both countries were destroyed in Europe north of the Alps by the Glacial epoch.

It would take too long to describe each region in detail, and I must here merely glance at them in the briefest manner.

A small outlier of the West Indian tropical flora extends into the south of Florida, and is followed by a broad zone of evergreen broad-leaved forest, of which *Magnolia grandiflora* is the chief representative. We then get the pitch pine forests on the sandy formations of Florida, Georgia, North and South Carolina, extending westwards to Alabama and Mississippi. The wood of the pitch pine (*P. australis*) is the best coniferous wood in the world, but the forests are being utterly ruined. They are tapped in the most wasteful manner for turpentine, 8,000,000 dollars being the estimated local value of the annual return. More wood is burned than is utilized, and, according to Mayr, already wide belts of white sterile shifting sands border both sides of the railways of the Gulf States, showing what the poorer tracts of the country will come to, if the farmers do not give up their pernicious habit of burning thousands of square miles of forest every year.

Another tree of the Southern Atlantic zone is the swamp cypress (*Taxodium distichum*), growing on annually inundated land, and presumably safe from fire, if not from ill-regulated and wasteful felling.

The valuable pencil cedar (*Juniperus virginiana*) also flourishes at its best in the Southern Atlantic region, but grows almost everywhere in the United States and British America, from latitude 54° southwards. To the north and in the prairies it has, however, only a stunted growth. Hardly any sound wood of this species is now procurable, as I learned last year from Messrs. Faber and Co. at Nuremberg. Next to this zone comes the description of the broad-leaved deciduous forest of the temperate region, containing many oaks, walnuts, hickories, and the tulip tree (*Liriodendron tulipifera*). The heavy seeded trees are found chiefly in the south, and lighter seeded ones, as maples, birches, and elms, more to the north.

There is a long account by Brandis of the prairie region, and the region of thinly-stocked forest bordering on it; and it appears that here, as cultivation extends, and the fires do not sweep over such vast extents of land as they did formerly, woods of Mesquit bean (*Prosopis juliflora*), and other trees are spreading by seed or coppice shoots, in Western Texas, and also in Wisconsin, Illinois, Iowa, and other States. Much has been done in the prairie region by plantations, and these succeed admirably wherever the climate is sufficiently moist; but in the central and western parts of Kansas all planting has hitherto failed, owing to the extremely dry climate.

In the northern pine zone of the Atlantic forest region, *Pinus Strobus*, the Weymouth or white pine is the most important species, and formerly covered enormous tracts from the Gulf of St. Lawrence to North Georgia, and beyond the sources of the Mississippi. At present, the only considerable supply of white pine is in Canada, and in the lake districts of the States of Michigan, Wisconsin, and Minnesota. The timber operations in the white pine forests have only one object, which is to bring as much timber as possible out of the forest in the shortest possible time, and to make money. Only the best trees are felled, and the rest burned. A forest after a timber gang has left it presents a remarkable appearance: between the standing blackened and partially charred stems of the broad-leaved and other trees which have not been felled are the stumps of the felled pines, whilst the ground is covered with wood, which would not have paid for its removal, and rots, or is burned by the annual fires.

In 1880, there were in the three lake districts 7000 million cubic feet of standing white pine timber, whilst in the last ten years 6205 millions of cubic feet have been felled and exported, 750 millions in 1889 alone. There is, therefore, little more left than can be exported in a single year. Many of the large saw-mills have already been obliged to stop work, or get timber from Canada. Chicago, which owes its rapid rise to the timber trade, imports yearly 166,000,000 cubic feet of white pine timber. This is about three-fourths of the whole forest yield of Prussia, the produce of 6,750,000 acres or 10,547 square miles of forest. Besides the Weymouth pine, *Pinus Banksiana*, the grey pine, and *Pinus resinosa*, and various broad-leaved trees are found. The sub-Arctic region of Alaska and British North America is poor in species; *Picea alba* and *nigra*, the white and black spruce, being characteristic trees.

Merely glancing at the North Mexican forest region, with forests of *Prosopis juliflora*, and grassy tracts containing gigantic cacti, and *Yucca baccata*, a palm lily, attaining 40 feet in height, we come to the Pacific forest region, where the Douglas fir, *Pseudotsuga Douglasii*, is the most important tree, and yields, in suitable localities, perhaps the greatest quantity of timber per acre of any known species.

We finally come to the red wood forests of the Pacific coast, where *Sequoia sempervirens* prevails, its congener *Sequoia gigantea* only occurring over a limited area. Unregulated fellings also prevail in the Douglas and red wood forests, and their supply cannot last much longer.

Besides the wholesale destruction of forests which goes on in America, and has already driven the United States to remove all duty from Canadian wood, the most appalling destruction is now being annually caused by the floods which pour down the slopes of the mountains, bringing down boulders, stones, and gravel on the cultivated lands below. Mayr has seen standing trees covered with mud up to a height of 15 feet in some of the Southern and Central States, whilst hundreds of magnificent trees lay uprooted in the full vigour of their growth. This can clearly be traced to the destruction of the hill forests.

How long will rulers of the United States shut their eyes to the appalling waste of the resources of their country which is still rampant! Brandis hopes that private capitalists may invest their money in forests, tempted by the rapid rise in the price of wood, and may manage them properly; but all European experience points to the necessity of State forests, and a trained State Forest Service to manage them, as the only efficacious remedy against the impoverishment of the soil and natural resources of America.

W. R. FISHER.

## DAILY INTERNATIONAL WEATHER CHARTS.

AT the meeting of the Meteorological Congress at Vienna in September 1873, General Myer, the Chief Signal Officer of the United States Army, submitted the following proposal:—

"That it is desirable that, with a view to their exchange, at least *one* uniform observation of such a character as to be suitable for the preparation of synoptic charts be taken and recorded daily and simultaneously at as many stations as practicable throughout the world."

Although various suggestions had been made before, and synoptic charts had been previously constructed for large areas, this proposal was a bold step in advance, as the charts hitherto published—those of the English Meteorological Office excepted—were mostly synoptic only, but not strictly synchronous, whereas the plan now proposed was to treat the whole observational area of the globe as a unit, and to represent the actual conditions existing at the same instant of physical time.

The proposal was well received, and on January 1, 1875, General Myer was able to publish his daily International Bulletin, and to supplement this, on July 1, 1878, by the daily International Weather Map. These publications were continued until the end of March 1884, after which time the daily Bulletin was discontinued, but the chart was issued on an enlarged scale, containing data referring to pressure and wind direction and force at all reporting stations in the northern hemisphere and over the northern portions of the Atlantic and Pacific Oceans, and this has been published up to the end of December 1887. We have before referred to the ability with which this great undertaking has been carried out by the Signal Service. The necessity of obtaining strictly simultaneous observations was generally acknowledged after the discovery of Buys Ballot's law of the relation between wind force and barometric pressure, about the year 1857, and it is almost entirely due to the construction of synoptic charts over large areas that so much progress has been made in weather prediction in the last quarter of a century. This progress would hardly have been possible while each country dealt exclusively with its own area, notwithstanding the great advance made over the old system of dealing with means of observations by the publication of telegraphic weather reports and weather charts. But notwithstanding the progress already made, we are still unable to foresee what may occur for more than a day or so in advance. Much more research is required, and the thousands of observations now taken on land and sea over the globe should be plotted at least once a day. We should therefore much regret the discontinuance of such work as that now before us, which deals with nearly half the globe.

To take one or two of the facts shown by the charts themselves: the very severe gale which visited these islands on December 8 and 9, 1886, in which about the lowest barometer reading on record was observed, will be remembered in connection with the capsizing of the Southport and St. Anne's lifeboats near Formby, resulting in the loss of twenty-seven lives out of twenty-nine which constituted the two crews. In a paper upon this storm, read before the Royal Meteorological Society on April 20, 1887, by Mr. C. Harding, it is stated, after a careful examination of the materials then available, that "the Atlantic was in such a disturbed condition at this time that it is not possible to track the passage of the storm across the Atlantic with any certainty." The daily International Charts, however, show the position of the storm day by day, and also that it did actually cross the Atlantic from shore to shore, and was central over the Gulf of St. Lawrence on December 3.

Another instance of remarkable weather, it will be remembered, occurred in June 1887—the Jubilee year; the weather was remarkably dry and fine in this country, there being an extraordinary drought of about thirty days. The charts for that period show that similar anticyclonic conditions also embraced a very large part of the eastern portion of the Atlantic, and extended abnormally over a portion of Europe; while the travelling disturbances are plainly shown to be confined to the American side of the ocean.

It is only Government organizations that can undertake the laborious work of producing such charts; but when they are published, the matter should not be left there: the meteorologist should make use of the materials provided for him, and endeavour to solve the problems which underlie weather changes and the general movements of the atmosphere.

## JOSEPH LEIDY, M.D.

THIS well-known American naturalist was born on September 9, 1823. He very early in life showed a fondness for collecting and observing insects, one of his first contributions being a paper on the mechanism which closes the membranous wings of the genus Locusta, published in 1845 in the Proceedings of the Academy of Natural Sciences of Philadelphia. Having taken his degree in medicine, he devoted himself more and more to the study of natural history, and few men of any nation have left behind them a longer list of work done than this distinguished man, whose death we announced in a recent number. Leidy was gifted with great powers of observation, he possessed a correct eye and steady hand for the delineation of whatever objects he was observing, he was endowed with a faculty for work; and as he had also an excellent memory, one reflects upon his half-century of work with less of surprise than admiration. To give an account of his writings would be to write a volume, to give but their titles would be to fill many of our columns, so that it must suffice to call to mind rather the subjects about which he wrote than the writings. Commencing with a study of entomology, and working more at the anatomy than at the general morphology of insects, he quickly passed on to the study of the entophytic worms, his "Flora and Fauna within Living Animals," published as one of the Smithsonian Contributions in 1852, having made its mark at the time. Then he took up the fresh-water Polyzoa, his labours on which will be understood only when a monograph on this group as inhabiting America comes to be published. Leaving for a time the study of invertebrate forms, he next entered on the field of research among the fossil vertebrates, describing in quick succession a number of remarkable fossil reptiles and fish, and he was the author of the first volume of the quarto series of reports issued by the United States Geological Survey of the Territories, under the title of "Contributions to the Extinct Vertebrate Fauna of the Western Territories." It was during his journeys to the Western Territories, that, not content with investigating the fossil vertebrates of the district, he worked very diligently at the study of the microscopic forms of life which inhabit the waters met with therein, and these researches, so far as one group of animals is concerned, were happily published by the United States Geological Survey in 1879, in one large quarto volume, "The Fresh-water Rhizopods of North America," which is illustrated by forty-eight coloured plates after Leidy's own drawings. This work on its appearance was received with great enthusiasm, and is still a worthy model for a monograph. During all these years, and amid so many and so varied labours, Leidy still discharged his duties as Professor of Anatomy to the University of Pennsylvania, and also of teacher of natural history to the classes of boys and girls at the Swarthmore College. No doubt many of these latter pupils will now call to mind the warm personal interest their master always took in their labours. In one of his books he tells us that since he was fourteen years of age the study of natural history was to him a constant source of happiness; but that on this joy a shadow was constantly cast when he thought how few, how very few, of those around him gave any attention to intellectual pursuits of any kind, and it saddened him to feel that the command "that man shall not live by bread alone" remained so unappreciated by the great mass of even so-called enlightened humanity. The results of Leidy's intellectual pursuits will long remain to testify to the manner of man that he was.

## THE SCIENCE MUSEUM.

THE discussion on this all-important question continues in the press. The Whitsuntide holidays have prevented any questions being asked in the House of Commons, where the feeling is very strong against the action of the Government.

As before, we reprint the most important items in the discussion. These consist of letters from Sir H. Roscoe and Profs. Armstrong and Ayrton to the *Times*. We commend to our readers the reference by the latter to Mr. Goschen's treatment of the deputation, and also their judgment as to the present position of science in this country, and the teaching of it in London, as compared with Göttingen and Zürich. No one can speak with greater authority than Profs. Armstrong and Ayrton on this subject.

Our administrative system, however, is such that the present question, which is acknowledged to be of such high importance, is being settled exclusively by officials who are quite ignorant of science. This is not said to their disparagement: it is only a statement of fact. The letters run as follow:—

ONE cannot but feel much sympathy for Ministers, on the one hand pressed by the advocates of scientific and technical education, and on the other nervous at the prospect of not securing the gifts of the munificent but somewhat *exigeant* art donor. But the question is so vitally important from the point of view of science that I feel sure no excuse is necessary if I urge most strenuously that an irrevocable step be not taken without full and careful consideration; and, further, that a definite scheme for providing for the science collections and Science School be formulated before what many of us believe to be a most unwise interpolation of an art gallery, on land which when bought was universally believed to have been acquired for scientific ends, is finally decided on.

At the present moment it is impossible to say under which thimble the scientific pea is housed, and it was no doubt due to this that the discussion which the deputation had with the Chancellor of the Exchequer and Lord President of the Council on Tuesday last was to some extent abortive.

The Chancellor of the Exchequer, in reply to myself on March 18, said:—

"It would be possible to make adequate provision for chemical and physical laboratories on the land between the Imperial Institute Road and the Technical Institute. This site adjoins the east galleries, and it is in these galleries, together with the west and southern galleries, and a proposed cross gallery joining the east and west galleries, that the science collections may ultimately be housed."

But by April 15 the impracticability of the scheme of putting part of the Science School at the south end of the eastern gallery seems to have been discovered. For on that day Mr. W. H. Smith, in reply to Mr. Mundella, propounded another scheme for the Science School, while leaving the collections to be housed in the east and west and cross galleries. He said:—

"A portion of these vacant lands" (facing the Imperial Institute) "can be utilized for the extension of the College of Science and for future growth of the science collections. Additions to the College of Science must in any case take the form of a separate building, divided from the present building by Exhibition Road; and, as access to the lands mentioned above from Exhibition Road will be secured by means of a corridor, the interposition of the Gallery of British Art need have no more serious effect than to increase by some 60 yards (which will be under cover) the distance between the two portions of the Science College."

By the former plan a portion of the Science School would no doubt have been in immediate contact with the splendid picture galleries in which the science objects were to be housed; but it would be far removed from the other part of the school—the Exhibition Road thus becoming a school of peripatetic philosophy. By the latter scheme the two parts of the school would be brought somewhat closer together—less of Exhibition Road and more of covered corridor—but then both portions would be entirely separated from the science collections—two roads to cross, and a walk of half a mile, or thereabouts, to the further part.

When receiving the deputation on Tuesday last, a third scheme was suggested, if not distinctly enunciated, by the Chancellor of the Exchequer, that the Science School extension and the Science Museum should be built on the other side of the plot given to the Art Gallery, but both on the ground recently acquired facing the Imperial Institute.

The two earlier projects having as it were blown themselves up, it is only necessary for me to deal with the last.

It has been argued that the recent Committee on the science collections, of which I was a member, only asked for 90,000 square feet of exhibiting space, and that more than that area can be obtained on the vacant ground opposite the Imperial Institute. But it must be remembered that, as stated by our Committee, this space did not provide for offices, workshops, &c.—a considerable item; that it did not in any way provide for the extension of the Science School; and that it was made some time before an immense impetus was given to technical education by the Technical Instruction Acts and the grants under the Customs and Excise Act of last year.

Now, the vacant ground recently acquired—omitting the strip part of which has already been sold, and the remainder of which is going to be sold for private dwelling houses—is about one-third of the land devoted to the Natural History Museum, and almost exactly of the same area as that already covered by the Natural History Museum buildings, which are shortly to be enlarged.

Is it unreasonable for the scientific man to urge that this vacant land is not too much to provide for the whole range of sciences other than those accommodated in the Natural History Museum; for a proper Museum of Machinery and Inventions; for a large extension of the Science School; and possibly for the collections from the Jermyn Street Museum? Surely there can be but one answer to this question.

Why—and we have never yet obtained an answer to this inquiry—will not the munificent donor be satisfied with another site? Why are the existing physical laboratory and scientific class-rooms to be removed, to allow an art gallery to be interposed between portions of the school?

Even if it be maintained that the ground south of the Imperial Institute Road will provide for the immediate wants of the Science School and collections, is it too much to ask that we should look a little ahead, and not now initiate another hugger-mugger arrangement of the collections and schools at South Kensington, which all will lament in a few years?

10 Bramham Gardens, S.W., May 15. HENRY E. ROSCOE.

NOTWITHSTANDING that the recent deputation to the President of the Council and the Chancellor of the Exchequer was headed by Sir William Thomson—the man of science whom we in this country regard as first among all others, both on account of his individual achievements and on account of his occupying the representative position of President of the Royal Society—not one single word was said by Mr. Goschen in explanation or justification of the course which he has adopted; we therefore venture, with all respect, to assert that the Royal Society has just cause to complain when one of its Fellows—for Mr. Goschen is one of us—thus treats representations urged by its President.

Where the science collections are to be lodged, where the extensions of the Science Schools are to be placed, are in themselves all-important questions; but a still graver issue remains—whether a weight of opinion of the magnitude represented by the memorial recently published in your columns is to be entirely set aside because an anonymous donor has offered £80,000 *plus* a collection of pictures, valued at another £75,000. That a Government which has at its head a Prime Minister whose interest in science is so marked, should thus disregard the opinion offered by so representative a body of men, is one of those things which even an Englishmen can scarcely understand: in no other country in Europe would such action be possible.

We cannot help thinking that a mistake has been made in calling public attention too exclusively to the housing of the science collections—the extension of the Science Schools appears to be a far more important matter. Attention has often been called of late to what is going on abroad, especially in Germany; to the unremitting attention that is being given to scientific instruction, and to the effect that is being produced on manufacturing industries of all kinds by the high development of science and of the application of every kind of scientific requirement. Unfortunately, in this country such matters have not yet entered into the domain of practical politics. But in the opinion of many among us there cannot be a question that almost superhuman efforts are necessary if this country is to regain the position which it has given away to foreigners by its neglect to apply the highest developments of chemical and physical science to industry.

The accommodation at present afforded by the Royal College of Science laboratories is not only inadequate, but beneath contempt in comparison with that to be found in Continental cities, such even as Göttingen and Zürich, for example; and those of us who have some knowledge of modern requirements know full well that every inch of space on the Imperial Institute Road side of the block of land on which stands the Natural History Museum will before long be required for the purposes of the Royal College of Science. The intrusion of an art gallery into this space would have a most disastrous effect by irretrievably preventing the proper and natural expansion of the Royal College of Science laboratories. This expansion must necessarily be rapid, for science is developing throughout the civilized world at a compound interest rate, and the grants recently made by the Chancellor of the Exchequer in aid of technical instruction must lead even this country to fully appreciate the value of experimental studies, and to insist on proper laboratory accommodation being provided.

Surely the munificent donor will accept for his gallery some other site equally good for art, and not insist on striking a blow at science by taking a piece of land already set apart for laboratories.

HENRY E. ARMSTRONG,
Secretary of the Chemical Society.
W. E. AYRTON,
President of the Physical Society.

It seems probable that, as the discussion goes on, some side light will be thrown upon the motives of those who have the "munificent donor" in hand. Although we have not room for the whole of a letter from Mr. Marshall of Edinburgh, the general drift of it may be stated as follows:—

Mr. Marshall's main point is that, according to the statements made by Sir Frederick Leighton in his speech at the Royal Academy banquet, the new gallery is to be used as "a worthy home for the permanent display of the works of contemporary native artists"—which "means, being interpreted," says Mr. Marshall, "a worthy home for the works of Royal Academicians and their friends." The object for which Sir James Linton, Sir J.C. Robinson, Mr. Orrocks, and others have been contending is that there should be adequate "recognition throughout its whole range, both as regards masters and mediums of work, of the artistic triumphs of the masters of our English school." What these gentlemen have urged and incontestably proved is that while foreign art, and especially early Italian art, is fully if not excessively represented in our National Gallery, and while a few of our great native artists (notably Turner and Constable), and many of our small ones, are represented far beyond what is necessary or even desirable, our native water-colour art is practically not recognized at all, and many of the very greatest of our masters in oil, who were (most of them) masters in water-colour also—Cox, Miller, Barret, De Wint, Crome, Cotman, Stark, Vincent, and others—are either conspicuous by their absence, or miserably represented as regards quality or quantity or both. If the public wants a "permanent display of the works of contemporary native artists," and if a generous millionaire is willing to provide "a worthy home" for such productions, the thing can be done. "But I object," continues Mr. Marshall, "to our astute Academicians, with the accomplished President at their head, calmly stepping in and absorbing a movement at the very moment of its success, diverting it from its legitimate purpose, and, after having stoned the prophets of English art while they lived, now endeavouring to steal the stones that others have quarried and hewn for the martyrs' monument in order to erect with them another comfortable mansion for themselves." Mr. Marshall is of opinion that "provincials" have opportunities more than enough of seeing contemporary art. Their wish now is to have a chance of studying fine specimens, authoritatively selected, of the acknowledged masters of our English school.

The possible existence of such special motives as those here suggested among the persons who are attempting to get a grant of land for the carrying out of their so-called national objects should form an additional inducement to men of science to redouble their efforts.

## *NOTES.*

THE general programme for the Cardiff meeting of the British Association has now been arranged. The first meeting will be held on Wednesday, August 19, at 8 p.m., when Sir Frederick Abel, K.C.B., will resign the chair, and Dr. William Huggins, President-elect, will assume the presidency and deliver an address. On Thursday evening, August 20, at 8 p.m., there will be a *soirée;* on Friday evening, August 21, at 8.30 p.m., a discourse on "Some Difficulties in the Life of Aquatic Insects," by Prof. L. C. Miall; on Monday evening, August 24, at 8.30 p.m., a discourse by Prof. T. E. Thorpe, F.R.S.; and on Tuesday evening, August 25, at 8 p.m., a *soirée.* On Wednesday, August 26, the concluding general meeting will be held at 2.30 p.m.

THE arrangements for the International Congress of Hygiene and Demography are nearly complete, and the programme, corrected up to May 1, has been issued in the form of a pamphlet. It has been definitely fixed that the opening meeting, at which the Prince of Wales is to preside, shall be held on Monday, August 10, at 3.30. The sections (of which there are ten) will meet on the four following days from 10 to 2. The six medical and scientific sections will meet in the rooms of the Royal and other learned Societies at Burlington House. The University of London will give the use of its large theatre to the section for the hygiene of infancy and childhood, and two examination halls to the sections for architecture and engineering. The division of demography will meet in the Theatre of the School of Mines, Jermyn Street. Much attention is being given to the necessary social preparations; and there is already a long list of proposed entertainments and excursions.

A GENERAL meeting of the Federated Institution of Mining Engineers will be held in London on Thursday, the 28th inst., at 12 noon, and on Friday, the 29th, at 10 a.m., in the rooms of the Institution of Civil Engineers, 25 Great George Street, Westminster. Various works will be visited on the 29th inst.

THE Committee of the Cardiff Naturalists' Society have put on foot a petition in favour of Mr. Pease's "Bill to Amend the Wild Birds' Protection Act, 1880." They are appealing to other scientific societies to join with them in order to make the petition as effective as possible.

AT Mowbray, a suburb of Cape Town, Mr. Cecil Rhodes has bought for £16,000 land on which, it is understood, the proposed University is to be built.

THE death of Prof. Carl Wilhelm von Nägeli, the eminent botanist, is announced. He died at Munich, on the 10th inst., in the 74th year of his age, and will be buried at Zürich, in accordance with a wish expressed before his death. Prof. von Nägeli was a Foreign Member of the Royal Society. We hope on a future occasion to give some account of his scientific labours.

THE Australian papers announce the death of Dr. Richard Schomburgk, brother of the late Sir Robert Schomburgk, and for many years Director of the Botanic Gardens at Adelaide, South Australia. Dr. Schomburgk was associated with his brother in the Boundary Demarcation Commission of British Guiana in 1840, and, some years later, settled with another brother in South Australia as a farmer and wine-grower. On the death of Mr. Francis, in 1866, he was offered, and accepted, the post of Director of the Adelaide Botanic Gardens, which he held with much distinction until his death. He was an enthusiastic horticulturist, rather than a botanist—that is to say, as an author; and his services in connection with the establishment he directed were very highly appreciated, as the sketches of his career testify. Indeed, so long ago as 1883, a large number of his admirers subscribed the funds to procure his portrait for the Museum of Economic Botany, founded by himself. His literary work commenced, we believe, with his "Reisen in Britisch Guiana in den Jahren 1840–1844," the third volume of which is devoted to a "Versuch einer Flora und Fauna von Britisch Guiana," in which Schomburgk had the assistance of several other botanists. This work has not yet been superseded, though its usefulness is unfortunately much limited by the publication of a large number of new names without descriptions. In 1876, Dr. Schomburgk supplemented this work by his "Botanical Reminiscences of British Guiana." But his most valuable literary work relates to the botany, to the agricultural and horticultural capabilities of his adopted country, and especially to the Botanic Garden, of which he was to a great extent the creator. His name will long be remembered in connection with this establishment, which is, it is asserted, the "most complete paradise of flowers in the southern hemisphere."

ACCORDING to the Calcutta correspondent of the *Times*, the Miranzai Expedition, under Sir W. Lockhart, has obtained much valuable geographical information about places which, although within a few miles of the frontier, have been hitherto unvisited by Europeans. The surveys effected by the Kuram field force during the Afghan war have been carried on to the Kurmana Valley.

A RUSSIAN scientific expedition, under the command of Captain Bartshevsky, has left Samarcand for the exploration of Southern Bokhara, the Pamir district, and Kafiristan.

ON Saturday, May 30, at the Royal Institution, Prof. A. H. Church, Professor of Chemistry in the Royal Academy of Arts, will begin a course of three lectures on the scientific study of decorative colour.

THE Rev. H. N. Hutchinson has undertaken to write for Messrs. Swan Sonnenschein and Co.'s "Introductory Science Text-books" a manual of physical geology. A second edition of Dr. Hatch's "Petrology" in the same series, reviewed in our columns last week, has already appeared.

MESSRS. WHITTAKER & CO. have in preparation a "Library of Popular Science." Among the works to be included in it are "Astronomy," by G. F. Chambers; "Light," by Sir H. Trueman Wood; "Chemistry," by T. Bolas; "Mineralogy," by Dr. F. H. Hatch; "Electricity and Magnetism," by S. Bottone; "Geology," by A. J. Jukes-Brown; "Botany," by G. Massee.

MR. J. ALLEN BROWN has expounded in the *West Middlesex Standard* an excellent scheme—now printed separately—for a technical institute and museum for the Ealing Parliamentary division of Middlesex. This division comprises Ealing, Acton, and Chiswick, and Mr. Brown's proposal is that a technical institute and museum should be established in whatever position may be most convenient for these localities. An essential part of his plan is that the instruction shall be imparted by specially qualified teachers and lecturers, and that their duties shall be "migratory or peripatetic," so that classes may be conducted or lectures given in any part of the division, and on any of the subjects contemplated under the Technical Instruction Acts. We commend Mr. Brown's scheme to the careful attention of the Middlesex County Council, which will soon have to decide as to the distribution of the funds placed at its disposal for technical instruction. There can be no doubt that the proposed institutions would be of immense advantage to the three districts, for Mr. Brown has a very enlightened conception of the true nature of technical instruction. What he wishes is that the young workman shall acquire "a knowledge of the scientific or artistic principles which are applicable to his trade or industry," and that by the development of his powers of observation and insight into the laws which govern all things "he may afterwards be enabled to effect improvements and excel to a greater extent than heretofore in the work he desires to accomplish."

THE Göttingen Society of Sciences has recently offered the following prize in physics for September 30, 1893:—From the researches of W. Röntgen and A. Kundt on variation of the optical properties of quartz in the electric field, there appears to be a close connection between the electro-optic phenomena and the elastic deformations which that piëzo-electric substance shows under the action of electrostatic forces. An extension of these inquiries to a series of piëzo-electric crystals with various properties of symmetry seems highly desirable. The investigation should also be directed to determining whether the electro-optic phenomena in piëzo-electric crystals are caused exclusively by the deformations occurring in the electric field or, besides, by a direct action of the electrostatic forces on the light-motion. Prize, £25.

THE German Society for the Encouragement of Industry offers the following (among other) prizes: (1) How far is the chemical composition of steel, and especially the amount of carbon present, a measure of the usefulness of cutting tools? Prize, a silver medal and £300; date, November 15, 1891. (2) A silver medal and £150 for the best chemical and physical investigation of the most common iron paints. Date, November 15, 1894. (3) A gold medal and £150 for the best work on the magnetism of iron. This should comprise a critical comparison of previous observations; also personal observations on steel and wrought iron bars of the most various chemical composition possible, examination being made both of the strength of temporary magnetization with absolutely measured and varying magnetizing force, and the strength of permanent magnetism and its durability with regard to temperature-changes and vibrations. Date, November 15, 1893. (4) Investigation of the trustworthiness of the usual methods of determining the carbon in iron. Prize, a silver medal and £150; date, November 15, 1892).

THE extraordinary collection of mummies, papyri, and other objects of antiquarian interest recovered last February at Deir-el-Bahari is now safely housed in the Ghizeh Museum. According to the Cairo correspondent of the *Times*, all the objects are in good condition, although some anxiety was caused by the protracted journey by boats from Luxor. The correspondent says that the mummies mostly belong to the 21st Dynasty, and, though styled Priests of Ammon, are supposed to be the corpses of generals and other official dignitaries who bore ecclesiastical besides other titles. The 163 mummies and the 75 papyri are not yet unrolled, and it is difficult to form an estimate of their archæological value, as many of the sarcophagi bear different names on the outer and inner casings, whilst others have the names usually inscribed on the outer casings intentionally effaced. M. Grébaut thinks that, owing to this circumstance and the magnitude of the collection, some time will be required before any important communications can be made to the scientific world.

A SERIES of experiments has been lately made by Herr Rubner (*Archiv für Hygiene*), with regard to the familiar fact that not only dry high temperatures are more easily borne than moist, but dry cold causes much less discomfort than moist cold. Dogs, fasting or fed, being observed in an air-calorimeter, it appeared that, in all cases, moist air increased the loss of heat by conduction and radiation. For every variation of the air-moisture 1 per cent., heat was parted with to the extent of 0·32 per cent. In a previous investigation, Herr Rubner demonstrated the lessened yield of water by evaporation from animals where the air-moisture is increased, involving lessened loss of heat. Here, then, are two antagonistic influences. He is disposed to regard the increased radiation and conduction in moist air as the primary action, and the diminished evaporation as secondary. The colder feeling of moist cold than dry is readily explained by the increased heat radiation. In moist heat, with the sense of oppression it brings, this factor passes rather into the background. The degree of temperature, and some other influences, of complex nature, also affect the amount of radiation.

THE Meteorological Council have issued a publication containing the hourly means obtained from the self-recording instruments at their observatories for the year 1887. This work constitutes a new departure in the use made of the records of the self-registering instruments, and one which we think will be of much practical use to meteorologists. The publication of the hourly observations *in extenso*, at the request of a number of scientific men, began with the year 1874, and was continued until 1880, in a lithographed form, and the daily means were added in 1879; from the year 1881 to 1886 they were issued in a printed form. The Council, after careful consideration, have now come to the conclusion that it is preferable, for a time at least, to publish mean values only; hitherto no hourly means had been published by the Office, but in the present work these have been grouped into five-day and other periods, in a convenient form for discussion, and the necessity for dealing with an excessive number of values has thereby been obviated, while many useful tables not included in the old series have been added. It is proposed to calculate the means similarly for earlier years, while the original records will be carefully preserved, and will be available, should they be needed, for any special research.

THE Annual Report of the Director of the Royal Alfred Observatory, Mauritius, for the year 1889 shows that the island has again enjoyed immunity from storms; the greatest hourly velocity of the wind was 31 miles. The almost total absence of tropical cyclones in the South Indian Ocean during the year is considered by Dr. Meldrum as another confirmation of the law that these cyclones are fewest in number and least intense in the years of least solar activity. The mean temperature was 0°·7 below the average for the last fifteen years, and below the average in every month except July and October. The maximum shade temperature was 93°·1 on March 27, and the minimum 52°·4 on June 18. The rainfall was 8·56 inches above the average; the greatest fall in one day was 3·88 inches on March 11, although this amount was much exceeded in other parts of the island. On January 1, a waterspout burst on the Pouce Mountain; Port Louis was flooded, and some persons were drowned. The collection of observations made at sea is actively carried on; 324 log-books were received, and the observations duly tabulated. The Report also contains observations made at the Seychelles and Rodriguez.

IN a paper recently published in the *Meteorologische Zeitschrift*, Prof. Hellmann, of Berlin, shows, from observations taken at different British, Continental, and American stations, at which barographs are used, that there exists a close coincidence in the daily range of the monthly extremes and in that of the hourly values of the barometer. He finds that the hours of occurrence of the highest and the lowest readings of the barometer during a month agree almost completely with the times in which the normal daily range has its maxima and minima, both curves being so similar in shape that it may be possible to judge of the general character of the daily range of the barometer from knowing only the hours at which the monthly extremes mostly occur. Hence, as the lowest readings of the barometer are accompanied by cloudy and stormy weather, during which the effect of the solar radiation upon the surface of the earth and the heating of the lower strata of the atmosphere are quite insignificant, Prof. Hellmann concludes that Prof. Hann and others are right in assuming that the normal daily range of the barometer is chiefly an effect of the *absorption of the solar rays in the upper strata* of our atmosphere. Prof. Hann has applied the harmonic analysis to the numbers furnished by Prof. Hellmann, and, by combining several stations in a group, has found the coefficients of the periodic formula to be practically the same as those for the normal daily range. We should, however, like to see a further confirmation with respect to the coincidence of the lowest readings and the diurnal minima, since the lowest readings occur so frequently during the passage of a severe storm, which can scarcely be said to have any agreement with the ordinary diurnal fluctuation.

THE first paper in the last volume of Transactions of the Seismological Society of Japan is by Mr. Bertin, and describes the double oscillograph and its employment for the study of rolling and pitching. It traces curves automatically, showing the motion produced in a floating body by waves. The second paper is on the "Seiches" of lakes, by Dr. F. A. Forel, of Geneva, and discusses those variations in the level of the water of lakes with the investigation of which the author's name has been associated for some years past. Prof. John Milne describes the remarkable instrument invented by him for measuring and recording the oscillatory movements of railway trains. Mr. Mason contributes a paper, accompanied by carefully compiled tables, demonstrating the importance of elaborating some uniform system of timekeeping for the purposes of seismological observations. Prof. C. G. Knott, in his paper on earthquake frequency, explodes two of the time-honoured delusions of the popular mind in regard to earthquakes, viz. that they are more frequent during the night than the day, and that their periodicity is connected with lunar culminations. Mr. Otsuka gives an interesting account of the great earthquake that visited Kumamoto in July 1888; and Mr. Pereira contributes a carefully compiled record of all the earthquakes noted by him in Yokohama from March 1885 to December 1889. Mr. W. E. Forster writes on earthquakes of non-volcanic origin, caused, it is suggested, by the displacement of masses of land beneath the ocean. The volume concludes with various reports and papers by Prof. Milne, such as diagrams of earthquakes recorded in Tokio, a report on earthquake observations made in Japan during the year 1889, and an essay on the connection between earthquakes and electric and magnetic phenomena, which is full of matter of an interesting and suggestive kind.

ACCORDING to the *Colonies and India*, Mr. Alexander McPhee, a West Australian bushman, who has steadily been earning fame lately by his explorations in the central regions of Australia, started inland from Roebourne in July last on another tour of discovery, taking back at the same time an albino aboriginal whom he found and brought to Melbourne a couple of years since. News has been received from which it appears that Mr. McPhee, with the albino, Jun Gun, and a "black fellow" named Timothy, went along the coast some 250 miles to a station called Yinadong, when the party turned inland in an easterly direction. After travelling about 350 miles, Mr. McPhee came upon another albino, a boy of fourteen years, whom he describes as the most extraordinary specimen of humanity he ever saw. One old man in this camp told Mr. McPhee that when he was a boy he heard of a party of whites and horses dying a long way inland. The old fellow could give no particulars about this party, but Mr. McPhee feels certain, owing to his acquaintance with the habits and customs of the blacks, and being thoroughly conversant with their dialect, that a party of white men perished about forty years ago somewhere in the interior. He heard of Warburton's party, and saw a native who told him that he guided them to water. He also heard of two parties of whites who had lately been in the desert, but turned back. From his turning point to the coast of La Grange Bay, Mr. McPhee reckons he was about 250 miles in a south-east direction from that bay. He found the natives very friendly, and on no occasion was it necessary to keep a watch. The country is described as very poor. The only birds observed during the journey were an odd crow and a few sparrows about the water; not a track of a kangaroo or emu was seen.

SOME satisfactory statements as to the growth of collegiate education are made in the last official report on public instruction in the North-West Provinces and Oudh. Of individual colleges, Agra, at which the numbers in 1885 had fallen as low as 45, has increased within the last two years from 97 to 175, or by over 80 per cent., and the percentage of increase last year was in no case less than 20. The number of matriculated students, indeed, is rising so rapidly that the existing accommodation is said to be barely adequate; it will, the Government resolution says, become a question of urgent importance whether the increasing number of students should be provided for by additions to the staff and buildings at the colleges now in existence, or by the creation of new colleges, or by the strengthening of the college classes at high schools and adding to their number. "Government," it is added, "will necessarily be guided to a great extent by the nature and direction of the local demand, as indicated by the willingness of the residents of the principal towns to contribute to the increased burden of expenditure." On its present basis, at all events, the higher education of India has received a fair share of Government support. But if it is satisfactory, says the *Pioneer*, to find that collegiate education in its present form is making decided progress, and that it is becoming possible to throw the cost of the advance on private shoulders, it is a distinct disappointment that not a word is said, as not a step has been taken, in those new directions of educational activity where other provinces have not only started, but made appreciable progress. There may be two opinions as to the extent to which, or the means by which, it is possible to introduce technical education, but there can be no question that some movement is desirable. It may be hoped that the omission is due, not so much to a failure to estimate the importance of the subject, as to a desire to give it fuller treatment on a future occasion.

THE amount of apparent flattening of the vault of the heavens Prof. Reimann has lately attempted to measure by noting the point which seems to bisect an arc extending from the zenith to the horizon. From 83 observations at Hirschberg he found that this point was $21^{\circ}\cdot47 \pm 0\cdot08$ above the horizon. This indicates a ratio of the vertical axis to the horizontal of $1 : 3\cdot66$. This apparent flattening has an annual period, and is dependent on cloud. The highest position of the bisecting point was assigned in autumn ($21^{\circ}\cdot98$), the lowest in spring ($20^{\circ}\cdot42$). The vault seems flatter the more the cloud. It seems least flat with a misty horizon; and the flattening seems less by night than by day. Curiously, several other persons whom Prof. Reimann got to make the same determination all gave higher values for the angle.

THE settlement of a purely philological question (that, namely, as to the position of the French accent), by a physical method, has been recently attempted by Dr. Pringsheim, of Berlin (*Naturw. Rdsch.*) The instrument used was König and Scott's phonautograph, into which a number of Frenchmen were required to speak; the measurement of the record being afterwards made by means of a tuning-fork curve running parallel with it. This instrument renders possible a determination of the duration, pitch, and intensity of each syllable, and Dr. Pringsheim discusses its indications. As a preliminary result, he finds that two-syllable words have the vowels pronounced with equal length and strength. Noteworthy differences appear in the curve of a word according as it occurs in the middle or at the end of a sentence. In the latter case, there is added to the characteristic word curve, a terminal curve with declining pitch and strength, which is nearly the same for different words, and corresponds to the sinking of the voice before a pause. The vowels and consonants show characteristic curves; and notably long wave-lengths occur with *n*, *l*, *b*, and *d*. The duration of syllables varies between 0·1 and 0·5 second; and between the syllables of a word there are often pauses of 0·03 to 0·2 second. The shortest syllable *é* in *été*, with rather slow pronunciation, consisted of 22 vibrations; yet the ear is capable of not only hearing the tone, but of detecting fine shades and differences in the mode of pronunciation. Further experiments in this direction, with an improved apparatus, are contemplated.

THE *Perak Government Gazette* states that a portion of an ethnographical collection formed by Signor G. B. Cerruti, in the island of Nias, has been recently acquired by the Government of Perak for the museum. Pulo Nias is one of a chain of islands bordering the south-western coast of Sumatra. The population is said to be numerous and of one race, though divided into many tribes under independent chiefs. Head-hunting is as common with them as it used to be in Borneo, and most of the houses have skulls hung up in them. Their weapons consist of iron-headed spears, mostly barbed, knives of two patterns, somewhat resembling the Kadubong Achi, with shields of two distinct types. No bows and arrows or blow-pipes seem to be known, nor are throwing sticks applied to their spears; boats also are not used by them, though rafts are sometimes made to cross the rivers on. The ironwork of their weapons is fashioned by themselves, and the upright double cylinder bellows is used to supply wind to their forges—the same in every respect as those used by the Semangs of Upper Perak, and the far away Malagasy. Helmets of black ijoh fibre are worn, somewhat similar to the cocoa-nut fibre ones of the Sandwich Islanders. Woven body armour is in use, in the shape of thick coats made of what appears to be the fibre of *Hibiscus tiliacens*. Buffalo hide armour is also said to be used, but is not represented in this collection. Attached to the sheaths of some of the knives are four or five animals' teeth, such as tigers, rhinoceros, &c., also a small carved wooden idol, and one or more bamboo boxes containing stones. In those examined there were twelve pebbles in each box. These stones are supposed to have been taken from the spot on which a man had been slain. All these charms are tied up into a bundle with red cloth, and bound with string on the upper front part of the sheath of the knife.

A COMPREHENSIVE study of the influence of forests on the daily variation of air-temperature has been recently made by Prof. Müttrich (*Met. Zeits.*), the data being from stations in Germany and Austria. *Inter alia*, this influence is greater in May to September or October than in the other months. In pine and fir woods it rises gradually from January to a maximum in August or September, then falls more quickly to a minimum in December; but in beech woods a minimum occurs in April, then there is quick rise, till the maximum is reached in July. The daily variation itself is greatest in May or June, both in forest and open country. The influence of the forest is to lower the maxima and raise the minima, and the former influence is in most months greater than the latter; in December and January, and occasionally in neighbouring months, it is less. The influence on the maxima in summer is greatest in beech woods, less in pine, and least in fir. The absolute value of the influence in woods of a given kind of tree is affected by the degree of density of the wood, being higher the denser the wood. The character of the climate (oceanic or continental) also affects the results. From daily observations in forest and open country, every two hours in the second half of June, it appears that, soon after 5 a.m. and 8 p.m., the air-temperature in the wood was equal to that in the open; that the maximum was about 0°·9 lower in the wood, and the minimum 0°·6 higher; that in May to September the difference sometimes reached 2°·7; that the maximum in the wood occurred about half an hour later, and the minimum a quarter of an hour earlier, than in the open; and that the daily mean air-temperature was about $\frac{1}{3}$° less in the wood.

THE *Revue des Sciences Naturelles de l'Ouest* gives an account of the life of Mathurin Rouault, one of the pioneers in the geology of Brittany. Rouault was born in 1813, of a very poor family. At the age of ten, while engaged as a shepherd, he became interested in "stones" and "rocks," and began to make a collection. By the death of a relative he obtained possession of a small hairdresser's shop, where he worked on Saturdays and Sundays, spending the rest of his time in hunting for rocks. Although Geoffroy Saint-Hilaire visited his collection of specimens, and was much interested in them, nothing would have been done for the poor young geologist—who lived upon something like five centimes a day—if it had not been for General de Tournemine, who, stationed with the garrison in Rennes, had been attracted by him. It is said that one day he went into the shop, and, seeing an antique pistol which Rouault had bought for a few centimes to kill himself with, the general remarked, "That is just the pistol I am after: I want it for my collection." And without waiting for an answer he took the pistol, and gave the young man 100 francs. M. de Tournemine went still further. He revised a memoir which the illiterate geologist had written. This was read in the Academy of Sciences, and met with so much success that the author became well known. The town of Rennes gave him 800 francs a year to help him to live in Paris, and afterwards he was appointed Director of the Geological Museum of Rennes. But he was dismissed on account of quarrels with some unintelligent bureaucrat, and died in 1881. Before his time only five or six fossils were known in Brittany: afterwards they numbered 500 or 600. He spent two years or more in making up *Trinucleus Pongerardi* out of over 2000 fragments.

AN important paper upon the atomic weight and position in the periodic system of the rare element lanthanum is contributed by Dr. Brauner, of Prague, late of the Owens College, Manchester, to the current number of the *Berichte*. In his recent work upon the reduction of oxides by metallic magnesium Prof. Winkler advanced the view that lanthanum is a tetravalent element of atomic weight 180, instead of, as has hitherto been accepted, a trivalent element belonging to the boron vertical group of the periodic system, with an atomic weight of 138·5. If lanthanum were indeed tetravalent with atomic weight 180, it would probably be the missing element between ytterbium and tantalum on the one hand, and cerium and thorium on the other. Further, Prof. Winkler expresses the opinion that the old values of Rammelsberg, Zschiesche, and Erk, for the equivalent of lanthanum, are correct. These experimenters obtained the round number 45 for the equivalent, and this number multiplied by 4 gives Prof. Winkler's suggested atomic weight 180. If, however, multiplied by 3, the atomic weight 135 is arrived at, and Prof. Winkler argues that even if the element were trivalent its atomic weight would not be 138·5 but 135. Against these views Dr. Brauner brings forward the following experimental facts. In the first place, Hillebrand (working under Bunsen) found the specific heat of Bunsen's pure lanthanum to be 0·04475. No impeachment has ever been brought against this result, and Dr. Brauner sees no reason why it should not be accepted. Making use of Dulong and Petit's generalization and multiplying this number by 138, a normal atomic heat of 6·18 is arrived at, whereas if multiplied by 180 the abnormal value 8·07 is obtained. Again, an element of atomic weight 180 should possess a density of 8·2, whereas that of lanthanum is only 6·48, a specific gravity corresponding to an atomic weight of 138. Considering therefore the position of lanthanum in the trivalent boron vertical group assured, Dr. Brauner brings forward a redetermination of its atomic weight of his own in order to decide between 138·5 and 135. His experimental method consisted in converting known weights of the oxide into sulphate. The material employed was obtained by a lengthy process of fractionation with ammonium nitrate, the oxide eventually obtained containing the most positive of the cerite earths (lanthanum oxide) and showing no traces in the spectrum of any others. His value thus obtained is 138·2, a number closely agreeing with those of Cleve and Bettendorff. The earlier and lower values of Rammelsberg and others are shown to be probably due to the presence of yttria, which was not detected by these observers, inasmuch as the work of Thalén and Bunsen upon the spectrum of yttrium had not then been published. Hence lanthanum of atomic weight 138·2 retains the place in the trivalent group of the periodic system marked out for it by its well-known basic properties.

THE additions to the Zoological Society's Gardens during the past week include a Striped Hyæna (*Hyæna striata* ♀) from India, presented by Mr. B. T. Ffinch, C.M.Z.S.; two Hairy-rumped Agoutis (*Dasyprocta prymnolopha*) from British Guiana, presented by Mr. H. Barrington; two Brent Geese (*Bernicla brenta*), a Pintail (*Dafila acuta* ♂), two Wigeons (*Mareca penelope* ♂ ♀), a Common Sheldrake (*Tadorna vulpanser* ♀), two Golden Tench (*Tinca vulgaris*, var.), nine Golden Carp (*Carassius auratus*), British, presented by Mrs. Atkinson; eight European Tree Frogs (*Hyla arborea*) from the South of France, presented by Mr. Clifford D. Fothergill; a Crested Porcupine (*Hystrix cristata*) from India, a Tibetan Crossoptilon (*Crossoptilon tibetanum* ♀) from Western China, deposited; two Swinhoe's Pheasants (*Euplocamus swinhoii* ♂ ♀) from Formosa, two Japanese Pheasants (*Phasianus versicolor* ♀ ♀) from Japan, two Amherst's Pheasants (*Thaumalea amherstiæ* ♀ ♀) from Szechuen, China, a Black-necked Stilt Plover (*Himantopus nigricollis*), a Cayenne Lapwing (*Vanellus cayennensis*) from South America, purchased; a Wild Swine (*Sus scrofa* ♀) from Persia, received in exchange; two Indian Desert Foxes (*Canis leucopus*), born in the Gardens.

## OUR ASTRONOMICAL COLUMN.

THE PHOTOGRAPHY OF FAINT NEBULÆ.—In the Journal of the British Astronomical Association for February, Dr. Max Wolf, of Heidelberg Observatory, contributes a note on a nebula surrounding ζ Orionis, the third star in the belt, which he has discovered on photographs taken with a 4-inch portrait lens. Some reproductions submitted to the Association show a large amount of nebulosity south-west of ζ, also nebulous ground around ζ, and a nebulous star north of ζ. Dr. Wolf's note is important, inasmuch as it indicates that the 4-inch portrait lens used at Heidelberg gives results which compare favourably with those obtained at Harvard with a much larger instrument, viz. the Bache equatorial of 8 inches aperture and 44 inches focal length. With regard to the use of portrait lenses for celestial photography, Dr. Wolf makes a few succinct remarks. In photographing the stars, the intensity of the image depends only upon the area of the lens employed, and an instrument of 20 inches diameter therefore requires 25 times less exposure than one 4 inches in diameter having the same focal length, in order to obtain the same number of stellar images. But it is a different thing with comets, nebulæ, and the like—bodies having a finite area. The intensity of the image at the focus then varies as the fraction $\left(\frac{d}{f}\right)^2$, where $d$ is the diameter of the object-glass, and $f$ its focal length. If, therefore, the intensity of the light received with an aperture of 20 inches and focal length of 100 inches be expressed by 0·04, that of a portrait lens of 4 inches aperture and 12 inches focus is 0·11. This shows that in order to photograph the same faint nebula, the instrument of 20 inches aperture requires an exposure about three times as long as the 4-inch portrait lens.

Another paper having the same purport is contributed by Dr. Holden to vol. iii. No. 14, of the Publications of the Astronomical Society of the Pacific, from which it appears that from 80 to 100 minutes' exposure with the 33-inch Lick telescope will give about the same number of stars as 205 minutes' exposure with Mr. Roberts's 20-inch reflector. When, however, the amount of nebulosity depicted is considered, the advantage is considerably in favour of the short-focus reflector, a comparison of the results obtained with the two instruments indicating that 15 minutes' exposure with the reflector is about as effective in showing the nebulosity of Orion as 60 minutes' with the refractor.

VARIATIONS IN LATITUDE.—Prof. H. G. van de Sande Bakhuyzen extends our knowledge of this subject in a paper contained in the March number of the *Monthly Notices* of the Royal Astronomical Society. The conclusions deduced from the investigation of observations of Polaris made between 1851 and 1882, and the interesting researches of Mr. Thackeray (Memoirs R.A.S., vol. xlix. p. 239), may be summed up as follows:—(1) The monthly discordances in the zenith distances of Polaris are, for the greater part, not caused by a real variation of latitude, but chiefly by an effect of temperature. (2) It is not possible to explain those discordances by an error in the indications of the exterior thermometer, or by an influence depending only on the exterior temperature. (3) The discordances can be explained, for the greater part, by a cause depending on the difference of the exterior and interior temperatures. (4) Probably that cause is a refraction in the observing-room, and its effects are sensibly proportional to those differences of temperature. (5) The discordances corrected for that refraction are about the same for both culminations, and can be explained by a real variation of latitude.

An investigation of the mean North Polar distances of Polaris in both culminations observed at Greenwich between 1883 and 1889 leads to the conclusions: (1) that it is probable that the observations of Polaris at Greenwich confirm the variations of latitude observed elsewhere in 1884–1885 and 1889–1890; (2) that there is a very strong probability that the variations in these years had an exceptional character, and do not agree with the annual variations, deduced from the observations of Polaris at Greenwich during the period 1851–1882.

RE-DISCOVERY OF WOLF'S COMET (1884 III.).—*Astronomische Nachrichten*, No. 3033, contains the information that Wolf's periodical comet was observed on its return by Prof. Barnard, of Lick Observatory, on May 3·9792 G.M.T. The following ephemeris is from one given in *Edinburgh Circular* No. 15, by Prof. Berberich. The brightness of the comet at re-discovery has been taken as unity.

*Ephemeris for Berlin Midnight.*

| 1891. | Right Ascension. h. m. s. | Declination. ° ′ | Brightness. |
|---|---|---|---|
| May 23 | 23 16 31 | + 17 47·1 | 1·44 |
| ,, 27 | 25 42 | 18 42·8 | 1·54 |
| ,, 31 | 35 0 | 19 37·7 | 1·65 |
| June 4 | 44 26 | 20 31·4 | 1·77 |
| ,, 8 | 53 59 | 21 23·9 | 1·90 |
| ,, 12 | 0 3 40 | 22 14·9 | 2·03 |
| ,, 16 | 13 30 | 23 4·0 | 2·18 |
| ,, 20 | 23 26 | 23 50·8 | 2·33 |
| ,, 24 | 33 32 | 24 35·2 | 2·50 |
| ,, 28 | 43 45 | 25 16·9 | 2·68 |
| July 2 | 54 5 | 25 55·4 | 2·88 |
| ,, 6 | 1 4 34 | 26 30·3 | 3·08 |

The comet will pass perihelion on September 3·3199 Berlin mean time. It is near α Pegasi at the present time, and may therefore be seen just before sunrise. The motion is towards Andromeda.

## THE PARIS OBSERVATORY.[1]

THIS report opens with the address delivered by the Director, Admiral Mouchez, before the Council of the Observatory on February 24 last; the following is a brief summary of the most important points touched upon.

After referring to the successful completion of the building for the large *equatorial coudé*, in which the instrument is now being erected, and to the formation of a special service for spectroscopy, over which M. Deslandres has been put in charge, he enters on the question of the formation of a branch establishment outside Paris. "The demands of modern science," he says, "the extreme smallness of the quantities on which the astronomy of position depends, and the extreme faintness of the objects that physical astronomy studies in order to penetrate more and more deeply into the knowledge of the universe, admit indeed of new processes of observation of such delicacy that they are altogether incompatible with the turmoil and disturbances of all kinds in a populous city. The instruments with large optical power lose nearly all their superiority, because they magnify the defects of an impure and disturbed atmosphere at least as rapidly as the images of the stars."

This is by no means the first time that this question of a branch establishment has been raised, but it looks very much as if it might now be taken up seriously. It seems that a proposal has been made to extend the railroad from Sceaux-Limours in the interior of Paris to Médicis and Cluny, where it would join the metropolitan; if this project was carried out, trains would run as close to the Observatory as 150 metres, thus affording the assistants at the Observatory an interesting amusement in calculating the distances of these trains by the vibrations set up in the various instruments.

A committee of inquiry, presided over by M. Chauchat, has been formed to inquire into the situation, and the unanimous opinion of all the astronomers questioned on the subject was that "the Observatory would be almost lost if this project was carried out according to the present conditions."

Of the other arguments put forward by Admiral Mouchez in favour of the branch establishment, the following may be mentioned. The lighting of the surrounding streets by means of the electric light. This, as he says, would obliterate all stars above the 12th magnitude, and perhaps even above the 11½th, to say nothing of the minor planets, nebulæ, and some comets. And with regard to photographing the heavens with moderate exposures, it would become nearly impossible owing to the fogging of the plates before the images are formed, the gas from the street lamps even now producing this effect on the sensitized plates. Referring to the opening and enlarging of the Rue Cassini, he points out, that at no remote date, houses will be constructed from 20 metres to 25 metres in height at a distance of 100 metres, and just in the direction of the meridian line of the instruments; these, besides completely blotting out from view many of the circumpolar stars at their lower culmination, will render the observation of those that remain difficult on account of the smoke from the chimneys.

Following Admiral Mouchez's address are the reports, from each of the heads of the various departments, of the work done during the past year. With the meridian circle no less than 14,374 stars have been observed, exclusive of the 432 observations of the planets made with the same instrument. Observations which were commenced in the month of April with the *equatorial coudé*, have been regularly pursued, and at present the results have been highly satisfactory. Not only "do we believe that we have settled in every detail the most precise rules for the application of the new method, but also we have obtained the constant of aberration with an exactness which surpasses all researches made up to the present time."

The three equatorials have been used by M. Bigourdan, Mdlle. Klumpke, and M. Boinot respectively, and with them observations have been made of comets, double stars, nebulæ, eclipses of Jupiter's satellites, occultations, planets, and double stars.

M. Paul Henry, who is chief of the photographic department, has been busily engaged among other things in making large *clichés* of different regions of the sky, several of which were prepared at the request of foreign astronomers.

The most important addition to the Observatory for the year was the special service for stellar spectroscopy, which, as we have mentioned before, is superintended by M. Deslandres. This branch, when in full working order, should be of the utmost value to science, and the results obtained will be looked forward to with interest. With regard to this branch Admiral Mouchez has given an extract from M. Deslandres' report on the installation of the apparatus and the results obtained.

After a short description of the meteorological work carried on, together with the various other reports usually inserted in this pamphlet, Admiral Mouchez concludes with a brief reference to the Observatory School at Montsouris, of which also he is Director. This school was organized under the patronage of the Bureau of Longitudes, in order to supply a want long felt in France of a school for practical astronomy, where "marine officers, explorers, professors of science, and others could come and accustom themselves to make observations." Since the year 1877 the Observatory has been freely opened to anyone, the only conditions being that those who go should have sufficient scientific knowledge to understand what is taught, and that their work should be regular. To give an idea of the range of the subjects that form the syllabus of instruction we cannot do better than condense the methods of organization as given in the report.

With regard to astronomy, both theoretical and practical lectures are given twice or three times a week. M. Boitel delivers a course on electricity and magnetism which extends over four months, during which time he conducts the officers over all the large electrical manufactories in Paris. Lectures on meteorology are delivered by M. Moureaux, who concludes them with practical instructions for the determination of the magnetic elements. M. Thoulet treats of ocean geography in a course that is of interest and use to sailors. The regulation of the compass, so important to-day on account of our iron ships, forms the subject of a number of lectures by M. Caspari, while photography is studied for two months under the superintendence of M. Guenaire.

From this syllabus it will be seen that a good, practical, and sound course is open to all those who wish to take advantage of it, and in the list of explorers who have figured in the principal missions during the last fifteen years the majority will be found to have served at any rate a short period at the Observatory School.

In concluding his remarks, Admiral Mouchez, after referring to the school that was started in 1879, and which was suppressed some years after for reasons of economy, points out the necessity of giving every encouragement to the one that is doing such good work at Montsouris. W. J. L.

[1] "Rapport Annuel sur l'État de l'Observatoire de Paris pour l'Année 1890." Présenté au Conseil par M. le Contre-Amiral Mouchez. (Paris: Gauthier-Villars et Fils, 1891.)

## NOTE ON THE PHYSIOLOGICAL ACTION OF CARBON-MONOXIDE OF NICKEL [$Ni(CO)_4$].[1]

BY the kindness of Mr. Ludwig Mond, we have had the opportunity of examining the physiological action of carbon-monoxide of nickel, a substance of unique chemical composition, represented by the formula $Ni(CO)_4$. The general results of our investigation are as follows:—

(1) $Ni(CO)_4$ is a powerful poison when injected subcutaneously into a rabbit weighing 1·5 kilo. even with a dose of 1/30th c.cm.

(2) The vapour of $Ni(CO)_4$ in air, even to the extent of 0·5 per cent., is dangerous.

(3) The symptoms are those of a respiratory poison, and are similar to those caused by carbonic oxide.

(4) The spectrum of the blood of an animal poisoned by $Ni(CO)_4$ is that of carbonic-oxide-hæmoglobin, and it is not reduced by sulphide of ammonium.

(5) When the substance is injected subcutaneously it is probably in part dissociated in the tissues, as there is evidence of the existence of nickel in those tissues, but the nickel also finds its way into the blood, and is found there.

(6) The substance produces a remarkably prolonged fall of temperature even when given in small quantities. In several instances, with lethal doses, the fall was from 2° to 12° C. This may be accounted for by the hæmoglobin being prevented to a large extent from supplying the tissues with oxygen. Nico, as we may, for convenience, call this substance, makes it possible to give graduated doses of carbonic oxide, and thus reduce temperature

[1] By John G. McKendrick, M.D., F.R.S., and William Snodgrass, M.A. M.B., Physiological Laboratory, University of Glasgow.

by directly interfering with the respiratory exchanges occurring in the tissues. The objections to its use as an antipyretic are that, owing to its poisonous properties, it is difficult to inject it subcutaneously in sufficiently small doses, while it is not easy to obtain a solution in any menstruum in which decomposition will not take place. If a convenient method of dissolving it could be devised, $Ni(CO)_4$ might become a valuable antipyretic, the *modus operandi* of which is intelligible.[1]

## SOCIETIES AND ACADEMIES.

### LONDON.

**Chemical Society**, April 2.—Mr. W. Crookes, F.R.S., Vice-President, in the chair.—The following papers were read:—Citraconfluorescein, by J. T. Hewitt. Lunge and Burckhardt have shown that maleic anhydride is capable of yielding a fluorescein; the author has obtained the corresponding fluorescein from citraconic anhydride, by the action of resorcinol in the presence of sulphuric acid. Citraconfluorescein is easily soluble in alcohol and glacial acetic acid, fairly soluble in water; the aqueous solution is yellowish-brown and shows a green fluorescence.—Ethylic thiacetacetate, by Dr. C. T. Sprague. Hübner obtained ethylic thiacetacetate by the action of sulphur monochloride, $S_2Cl_2$, on ethylic acetacetate. It has since been obtained by Delisle by the action of sulphur dichloride, $SCl_2$, on ethylic acetacetate; by Schönbrodt by the action of sulphur on the copper derivative of ethylic acetacetate; and by Michaelis and Phillips from thionyl chloride and ethylic acetacetate. Buchka proposed the formula $S(CH.Ac.CO_2Et)_2$, but an alternative formula, $S(O.C:CH.CO_2Et)_2$, was suggested by Delisle. The author describes the preparation of the substance and the products of its interaction with hydrazines; and shows that it behaves towards phenylhydrazine in the same manner as ethylic acetacetate. The results are in accordance with the formula proposed by Buchka.—The function of chlorine in acid chlorides as exemplified by sulphuryl chloride, by H. E. Armstrong. A number of experiments carried out during recent years in the author's laboratory show that sulphuryl chloride, $SO_2Cl_2$, acts on benzenoid compounds simply as a chlorinating agent. Sulphuryl chloride is easily formed by the direct union of sulphur dioxide and chlorine in the presence of a catalyst, such as camphor, charcoal, or acetic acid; it is a highly mobile liquid of low boiling-point, and is acted on with extreme slowness by water and alkaline solutions. It is an inert substance possessed of properties by no means such as are usually regarded as characteristic of acid chlorides. The chlorine is apparently but loosely held, and is easily withdrawn by a compound having an affinity for chlorine, such as naphthalene. On warming a mixture of this hydrocarbon and sulphuryl chloride, $SO_2$ is evolved and naphthalene tetrachloride is produced. The author doubts whether the chlorine in acid chlorides is possessed of special activity, and is inclined to the view that the activity of acid chlorides is conditioned by the oxygen rather than the chlorine; this view being supported by the observations of Wagner and Saytzeff, and the later ones of Pawlow (*Annalen*, clxxxviii. 104). The author also discusses the action of $SO_3HCl$, and the analogous compound $SO_3.EtCl$, and points out that pyrosulphuryl chloride, $S_2O_5Cl_2$, behaves much as if it consisted of $SO_3$ and $SO_2Cl_2$.—The action of nitric acid on the lignocelluloses, by C. F. Cross and E. J. Bevan. Dilute nitric acid attacks the ligno-celluloses when heated with them at 60°, with the formation of a bright yellow derivative of lignone and nitrous acid. On further interaction, large quantities of nitrous oxide, $N_2O$, are evolved, together with carbonic anhydride and a small proportion of nitric oxide. A sensible quantity of hydrogen cyanide is also produced, the proportion being increased by increase of temperature. The observations point to the entrance of the NOH residue into the lignone molecule; its interaction with nitrous acid being finally the displacement of $H_2$ by O. The reaction is probably general for compounds containing the NOH residue, and the authors suggest that attention be paid to the gaseous products of the interaction of nitric acid and carbon compounds, as calculated to elucidate their mechanism.—The Chairman, Mr. Crookes, gave a short verbal account of observations on the volatilization of metals *in vacuo* under the influence of an electric discharge.

[1] This investigation was carried on during last winter. It appears that M. Hanriot made a communication of the subject to the Société Chimique on February 27. He found the substance to be more poisonous than CO, and that the blood gave the spectrum of carbon-monoxide-hæmoglobin.

April 16.—Prof. A. Crum Brown, F.R.S., President, in the chair.—The following papers were read:—Studies on the formation of substitution derivatives, by H. Gordon. The following experiments were undertaken with the object of throwing further light on the laws which govern substitution in the case of benzenoid compounds. *The action of bromine on diorthonitrophenol.*—When bromine is added to an acetic acid solution of diorthonitrophenol at ordinary temperatures, the normal product, namely parabromdiorthonitrophenol, is obtained. However, if the mixture be heated at 100° for a short time, a mixture is obtained consisting of parabromdiorthonitrophenol and orthobromorthoparadinitrophenol. And if the heating be prolonged, and small quantities of bromine added, the mixed product is converted into orthobromorthoparadinitrophenol. Parabromdiorthonitrophenol is therefore completely converted by the action of heat, and bromine into the isomeric orthobromorthoparadinitrophenol. The same isomeric change takes place under the influence of nitric acid. An acetic acid solution of parabromdiorthonitrophenol, when heated with a few drops of nitric acid at 100°, is completely converted into the isomeric orthobromorthoparadinitrophenol. Experiments were then undertaken with the corresponding chloro-compounds. It was found that chlorine had no action on diorthonitrophenol when dissolved in acetic acid at 100°, even in the presence of iodine. Chlorination, however, takes place when chlorine is passed into a solution of diorthonitrophenol in antimony pentachloride at 105°, and only the normal product parachlorodinitrophenol is formed. *Action of bromine on parachlordiorthonitrophenol.*—Experiments to ascertain whether isomeric change could be effected by the action of bromine on parachlordinitrophenol only gave negative results, the normal product, parachlororthobromorthonitrophenol, being obtained in every case. The author considers that in the case of the chlorine compound isomeric change does not take place, because the chlorine is more firmly held than bromine. *Action of sulphuric acid on orthoparadichlorphenolorthosulphonic acid.*—The combined action of heat and sulphuric acid on orthoparadichlorphenolsulphonic acid gave no indication of any isomeric change taking place, although the reaction was investigated under a great variety of conditions of temperature, &c. The corresponding dibromphenol also gave negative results, but as several secondary reactions set in, such as the formation of tribromphenol, this reaction was not further investigated. *The chlorination and bromination of phenol.*—Phenol when chlorinated in the ordinary manner yields a mixture of para- and ortho- chlorphenol. The author finds that a similar mixture is obtained when $SO_2Cl_2$ is employed as the chlorinating agent. He has also investigated the action of bromine on phenol under the conditions described by Hübner and Brenken (*Ber.* vi. 170), and finds that the product is practically pure parabromphenol. *The sulphonation of the nitrophenols.*—Orthonitrophenol and paranitrophenol are, according to Armstrong, both readily acted upon by $SO_3HCl$; the former yields the well-known sulpho-acid; the latter yields a product which is decomposed by water, and was supposed by Armstrong to be the sulphate, and this the author finds to be the case. The author considers that the initial action in both cases is the same; but that the sulphate formed from orthonitrophenol at once undergoes isomeric change, whereas the sulphate from paranitrophenol is more stable. The author did not succeed in obtaining any sulpho-acid by heating the sulphate from the paranitrophenol at 100°. But he obtained a fair yield of sulpho-acid by heating the nitrophenol with two molecular proportions of $SO_3HCl$ at 100°. Hence, there is little doubt that the paranitrophenolsulphonic acid is formed by the sulphonation of the sulphate. Metanitrophenol resembles the para-compound in being converted into sulphate, but not into the sulpho-acid even by the action of heat.—Compounds of dextrose with the oxides of nickel, chromium, and iron, by A. C. Chapman. The nickel compound is obtained by adding a solution of nickel hydrate in ammonia to a solution of dextrose in 90 per cent. alcohol. It is a green amorphous substance, insoluble in water and alcohol, of the composition $C_6H_{12}O_6.2NiO + 3H_2O$. The chromium compound, which appears to have the composition represented by the formula $C_6H_{12}O_6.Cr_2O_3 + 4H_2O$, is prepared by dissolving an excess of dextrose in an aqueous solution of chromic chloride, and pouring this solution into cold strong ammonia. The precipitated hydrate partly dissolves on standing, and on pouring the purple solution so obtained into 90 per cent. alcohol, the chromium dextrosate is obtained as a lilac-coloured precipitate. The iron compound, $2C_6H_{12}O_6.3Fe_2O_3$

$+ 3H_2O$, is obtained by adding a slight excess of ammonia to a solution of ferric chloride containing an excess of dextrose; on standing, a deep red solution is obtained, which when poured into 90 per cent. alcohol yields the dextrosate of iron as a red flocculent precipitate. The moist compound dissolves easily in water to a red solution, is decomposed on boiling, but is not decomposed by ammonia, potassic ferrocyanide, or potassic thiocyanate. The dry compound is insoluble in water.—A rapid method of estimating nitrates in potable waters, by Dr. G. Harrow. The method depends on the reduction of nitric to nitrous acid by means of zinc dust and hydrochloric acid, in a very dilute solution, in the presence of α-naphthylamine and sulphuric acid; the estimation is made by comparing the depths of the pink azo-coloration developed in the solution with that arising on similar treatment of standard nitrate solutions. When nitrites are present, the amount is estimated in a similar manner prior to the addition of zinc dust, and due allowance is subsequently made. A number of comparisons with the Crum method show that very satisfactory results are obtainable.—The "gravivolumeter," an instrument by means of which the observed volume of a single gas gives directly the weight of the gas: a preliminary note, by F. R. Japp, F.R.S. The author describes a method of constructing a gas apparatus, by means of which, with an ordinary graduation in cubic centimetres, any required single gas may, without observation of temperature or pressure and without calculation, be measured under such conditions that each cubic centimetre represents a milligram of the gas. The author describes the apparatus in detail and the method of using it, and he anticipates that it will, at least, give results sufficiently accurate for technical purposes.—Mr. de Mosenthal exhibited one of Lipmann's coloured photographic negatives.—The action of acetic acid on phenylthiocarbimide, by J. C. Cain and Dr. J. B. Cohen, Owens College. The authors show that the product of the action of pure glacial acetic acid on phenylthiocarbimide is not diacetanilide, as stated by Hofmann; but that two compounds are formed—namely, diphenylurea and acetanilide. At low temperatures diphenylurea is mainly formed, at higher temperatures acetanilide. The reactions may be expressed by the following equations:—

$$2C_6H_5NCS + 6C_2H_4O_2 = (C_6H_5NH)_2CO + 3(C_2H_3O)_2.O + 2H_2S + CO_2$$

and

$$(C_6H_5NH)_2CO + 2C_2H_4O_2 = 2C_6H_5NH_2 + (C_2H_3O)_2O + CO_2.$$

—The action of aluminium chloride on benzenoid acid chlorides, by R. E. Hughes, Jesus College, Oxford. The author has examined the action of aluminium chloride on cinnamic and hydrocinnamic chlorides, in the expectation that pentamethylene derivatives might result. The experiments, however, afforded negative results. The chloride was either dissolved in or mixed with light petroleum, and aluminium chloride then added; action set in at 80–90° in the case of cinnamic, and at 50° and more briskly in the case of hydrocinnamic, chloride. The chief product in both cases was an ill-characterized substance, which has not been examined. The author also describes the following compounds: hydrocinnamic chloride, hydrocinnamide, and hydrocinnamanilide. It is noted that benzoic and cinnamic acids may be readily separated by treating the mixture with phosphorus pentachloride and distilling the product under reduced pressure; the portion passing over below 95° under 10 mm. contains the benzoic chloride.

### PARIS.

**Academy of Sciences,** May 11.—M. Duchartre in the chair.—Essay on graphical dynamics, with reference to the periods of motion of hydraulic motors, by M. H. Léauté.—On the lowering of the surface of water in a horizontal cylindrical vessel, by M. Haton de la Goupillière.—On the boundaries of the littoral zones, by M. Léon Vaillant.—Observations made at Marseilles Observatory of the asteroid (308) discovered on March 31, by M. Borrelly. The observations for position extend from April 6 to April 30.—Elements of the orbit of Borrelly's new asteroid (308), by M. Fabry.—Provisionary elements of Borrelly's asteroid deduced from observations made at Marseilles Observatory on March 30, April 8, 18, and 26, by M. Esmiol.—Solar observations made at the Royal Observatory of the Roman College during the first quarter of 1891, by M. Tacchini.—On the movement of the moon's perigee, by M. Perchot.—On limited permutations, by M. C. A. Laisant.—On a class of complex numbers, by M. Markoff.—On a registering manometer applicable to pieces of ordnance, by M. P. Vieille.—An "elastic" theory of plasticity and fragility of solid bodies, by M. Marcel Brillouin.—On the wave-surface in crystals, by M. C. Raveau.—On the determination of the dielectric constant of glass by means of very rapid electrical oscillations, by M. R. Blondlot. The author has made some experiments which support Prof. J. J. Thomson's conclusion that the specific inductive capacity of glass is very nearly equal to the square of the index of refraction, and has least value when a slow frequency of vibration is employed.—On a new compound of oxygen and tungsten, by M. E. Péchard.—Thermic study of bibasic organic acids with simple functions, by M. G. Massol.—Remark on the preceding note, by M. Berthelot.—On the fourth primary amyl-alcohol, by M. L. Tissier.—On the diffusion of fresh water into sea-water, by M. J. Thoulet.—On the theory of M. Tschermak's felspars, by M. K. de Kroustchoff. A description is given of a new triclinic felspar having a composition very similar to oligoclase, but distinguished from it by several peculiarities.—On the genital organs of some Tristomidæ, by M. G. Saint-Remy.—On the constitution of the sexual nuclei of plants, by M. Léon Guignard.—On the groups of the genus *Clusia*, by M. J. Vesque.—The parasitic fungus of the larva of the cockchafer, by MM. Prillieux and Delacroix.—The parasite of the cockchafer, by M. Le Moult.—On a remarkable inversion of strata termed *pli couché* observed near Toulon, by MM. Marcel Bertrand and Zurcher.—On the permanence of the orogenic effort in the Pyrenees during the geological periods, by M. Roussel.

### AMSTERDAM.

**Royal Academy of Sciences,** April 24.—Prof. van de Sande Bakhuyzen in the chair.—Mr. van der Waals dealt with a formula for electrolytic dissociation, which may be deduced from his theory of a mixture. This formula accounts for the facts (1) that ions may combine with absorption of heat; (2) that the parameter of electrolytic dissociation varies with the medium which holds the salt-molecules in solution; (3) that the quantity of free ions may diminish when the quantity of salt-molecules increases.

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