THURSDAY, MARCH 11, 1886

## THE SCIENTIFIC RELIEF FUND

A TIME in which so many heartrending calls are being made upon the benevolent seems but ill-adapted for pressing any schemes not immediately connected with any specially urgent distress. There is, however, at present an appeal being made by the Royal Society to all who are in any way interested in science, which, though on behalf of a permanent fund, and not connected with any ephemeral distress, has long been felt to be necessary, and which peculiar circumstances render it imperative now to press forward without delay.

For some years past the Scientific Relief Fund administered by the Royal Society has been found insufficient properly to meet the numerous claims made upon it, and the Committee in charge of the Fund has been frequently hampered in its action in consequence. Of course the demands upon it are variable in their character and extent, being chiefly dependent on what may be called the chapter of accidents; but there are few years that pass without some one of greater or less scientific repute being carried off by an untimely death without having made due provision for those whom he leaves behind him; and perhaps fewer years still in which some earnest worker is not laid low by sickness, and finds himself reduced by an unforeseen chain of circumstances to a condition in which a grant from a fund administered by his scientific brethren is of inestimable value both materially and morally.

The income of the Scientific Relief Fund has not hitherto exceeded some 250*l.* per annum, its capital at the close of last year being about 7500*l.* With the view of increasing this capital sum to at least 20,000*l.* a most munificent offer has been made by Sir William Armstrong, who was among the original founders of the Fund. It is that he will present 6500*l.* to the Fund provided certain conditions are fulfilled, the principal of which is that an equal amount be raised from other sources within the current year. We earnestly hope that there may be no difficulty in accomplishing this, and, with the view of making the scope and character of the Fund more fully understood, we annex a few explanatory particulars.

It was in the year 1859 that the idea of creating a Scientific Relief Fund occurred to the minds of the late Mr. Gassiot and some other Fellows of the Royal Society, and the regulations under which it was to be administered were carefully considered, and, having been adopted by the Royal Society, have remained practically unchanged until the present day. The object of the Fund is defined to be for the aid of such scientific men or their families as may from time to time require and deserve assistance. Cases, however, can only be entertained on the recommendation of a President of one of the chartered Societies, it being understood that he consults the Councils over which he presides as to the person whom he intends to recommend for relief. The Committee administering the Fund, though Fellows of the Royal Society, are not members of its Council, and the senior member of its body retires annually, another Fellow being appointed in his place. It will thus be seen that every reasonable precaution has been taken for the impartial and judicious administration of the Fund; and since its foundation nearly a hundred recipients of well-timed grants have had reason to bless its existence.

It may perhaps be said that men of science ought to be able to foresee what is coming, and be prepared to meet all the changes and chances of this mortal life, and possibly as a class they are more than usually exempt from those reverses of fortune to which all are liable. It is, however, the unforeseen that constantly occurs, and the temporary aid which, by means of such a Fund, a man can receive from a committee of his fellow-workers may be accepted with all gratitude, and without that humiliation which would be felt did the relief proceed from any more ordinary source of charity.

But although men of science may as a rule be prudent, it can hardly be said that they are as a class rich. Their researches may aid others in the acquisition of material wealth, but the cases are exceptional where the pursuit of science has brought large pecuniary gains to the student. Where it has, we are confident that others besides Sir William Armstrong will aid those less fortunate in this particular than themselves by contributing liberally to this Fund.

There is, however, another and important class of persons whose ample fortunes have been the result of the judicious application of science in economic pursuits, and we may be sure that the wants of the Fund have only to be brought under their notice to assure ready and efficient aid in raising the amount necessary to meet Sir William Armstrong's munificent offer. Already we see on the list a noble subscription of five hundred guineas from Mr. Ludwig Mond, and we venture to hope that by thus giving publicity to what is at present being done to enlarge the scope of this most useful Fund, others directly and indirectly interested in science may be led to follow so generous an example.

Looking at the extent to which the material welfare of this country has now for many years been intimately connected with its scientific progress, the debt due from all to the workers in science must at once be appreciated, and a more practical manner of acknowledging this indebtedness can hardly be suggested than that of contributing to the Scientific Relief Fund.

## THE BOTANY OF THE ROCKY MOUNTAIN REGION

*Manual of the Botany of the Rocky Mountain Region, from New Mexico to the British Boundary.* By John M. Coulter, Ph.D., Professor of Botany in the Wabash College. (New York and Chicago: Ivison, Blakeman Taylor, and Co, 1885.)

THE object of this manual, as stated in the preface, is to do for the range of country in which the plants it describes are found, what has for a long time been so admirably done for the North-Eastern States of the Union by Asa Gray's manual. It hence affords a means of comparison between two distant areas, each of such considerable dimensions as to throw much light on the flora of temperate North America. And not only with the flora of the Eastern States does it compare, for, the botany of the great Western area included in California having also been recently worked out, the three floras together enable a fairly accurate estimate to be formed of the nature and extent of the vegetation of the middle regions of temperate North America from ocean to ocean. Thus Gray's manual takes in the States between the Atlantic and Mississippi, which lie north of Tennessee and North Carolina; that is, approximately between lats. 36½° and 46° N., and is essentially the vegetation of a wooded region with high-lands towards the coast. The Rocky Mountain manual occupies a rather smaller area, comprising the States of Colorado, Wyoming, Montana, Western Dakota, Western Nebraska, and Western Kansas. Its southern and northern limits are on the same parallel as those of Gray's "Flora," and its eastern limits (the 100th meridian) is nearly parallel to and as long as Gray's western, and distant from it about 400 miles. Its western frontier is a very irregular one, following the north-western and south-eastern direction of the great mountain plateau; and borders the Pacific States of Washington and Oregon, and the interior ones of Nevada and Utah. It is essentially an Alpine and prairie vegetation; probably most or all of it is above 4000 feet of elevation, with mountains attaining a maximum of over 14,000 feet (Mount Gray). The Californian flora, again, does not occupy half the area of either of the others. It extends rather further south, to lat. 35°, and only to 45° N. Though only 200 miles in average breadth, it is infinitely richer than both the others combined, having a mountain flora—the Sierra Nevada—throughout the length of its eastern boundary, a coast flora along the Pacific, a hill flora along a coast range, and a valley flora between the latter and the Sierra Nevada.

Comparing the areas thus limited with that of the whole breadth of what may be regarded as temperate North America (in contradistinction to the cold British possessions to the north of 48° and the hot ones to the south of 35°), it will be seen that they together cover nearly four-fifths of the breadth of the continent, of which 1200 miles are in the limits of Gray's "Flora," 700 in Coulter's, and (though not in the same parallel) 200 in the Californian. The strips of country not included are, the wooded region west of the Mississippi, and the prairies between the latter and the 100th meridian, and the region of the Great Basin, between the Rocky Mountains and the Sierra Nevada. Neither of them can, however, be expected to add much to the sum of the three floras now under consideration, for on the one hand the western wooded region of the Mississippi cannot add much to Gray's flora or the eastern prairies to Coulter's; and on the other hand, as the Rocky Mountain flora takes in the plants of the western fringe of the Great Basin, and the Californian its eastern fringe, the intermediate region cannot add very much to what these two floras contain.

Dr. Gray[1] has in various essays admirably discussed the characteristics of the three longitudinal zones of North American vegetation, and clearly indicated their composition and relations. In no region of the northern hemisphere, nor perhaps anywhere on the globe, can three parallel meridional floras so different in their aspect and elements be found within such narrow limits, as the middle unforested zone of North America separating two heavily forested ones, the latter differing from one another by hundreds of genera and thousands of species.

The richness of the three floras as expressed by genera and species is, in round numbers—

| | Genera | Species | (European) |
|---|---|---|---|
| Rocky Mountain Flora ... | 500 | 1750 | (300) |
| Eastern United States ... | 660 | 2150 | (370) |
| Californian ... ... ... ... | 764 | 3786 | (225) |

A striking difference between the Pacific and Atlantic floras is in the relative numbers of the two primary groups of Dicotyledons: the Polypetalæ and Monopetalæ, which in California are as eighteen to ten, but in the Eastern States nearly equal. In both the Incompletæ amount to about one-third of the Monopetalæ, in which respect the Rocky Mountain flora assimilates to the Eastern one. But the most remarkable difference between the three floras is in the relative proportion of Monocotyledons and Dicotyledons, which is about 1 : 2·18 in the Eastern United States; 1 : 3 in the Rocky Mountains; and 1 : 4·9 in California. This is mainly due to the greater number of Junceæ, Cyperaceæ, and Gramineæ in the Rocky Mountains and Eastern States, and the abnormal excess of various Dicotyledonous families in California. So too with the proportion of genera to species: it is nearly the same in the Rocky Mountains and Eastern States (1 : 3·5 and 1 : 3·3), but in California, owing very much to the number of monotypic genera, it is nearly as 1 : 5.

Regarding the composition of the Rocky Mountain flora, the most remarkable facts are the number of Compositæ, amounting to one-fifth of the Phanerogams, thus far exceeding the proportions in the Eastern United States (about one-seventh), and in California (about one-eighth). Of this order nearly forty genera do not occur in the Eastern States. Leguminosæ come next in order of number of species, as they do in California, where, however, they are fewer in proportion to the whole flora. These, with Gramineæ, Cyperaceæ, Scrophularineæ, and Rosaceæ, which follow next in order of numbers (six families in all), embrace half the Rocky Mountain Phanerogamic flora. In the Eastern United States half of the flora consists of seven families, of which the five first are the same as in the Rocky Mountain flora; but Scrophularineæ are replaced by Ericeæ, and Ranunculaceæ are to be added. In California, on the other hand, species of no fewer than thirteen families must be added together to embrace half the flora.

But the most conspicuous difference to the eye between the vegetation of the Rocky Mountains and the Eastern States consists in the absence in the former of almost all the Coniferæ and Cupuliferæ of the latter, together with the whole Orders of Magnoliaceæ, Tiliaceæ, Juglandeæ, Platanaceæ, and Droseraceæ, and the rarity of Hypericineæ, Rubiaceæ, Lobeliaceæ, Ericeæ, Labiatæ, and Orchideæ, and of water-plants generally. On the other hand, there is in the Rocky Mountains a much greater abundance of Cruciferæ, Portulaceæ, Loasaceæ, Cactaceæ, Polemoniaceæ, Borragineæ, Solanaceæ, Chenopodiaceæ, Polygoneæ, and Nyctagineæ, — all show-

[1] "Statistics of the Flora of the Northern United States" (*American Journal of Science and Arts*, vol. xxiii.); "Remarks concerning the Flora of North America" (*American Journal of Science and Arts*, vol. xxiv.); "Vegetation of the Rocky Mountain Region" (*Bulletin* of the United States Geological and Geographical Survey, vol. vi. No. 1, 1881); "Characteristics of the North American Flora" (*British Association Reports*, Montreal, 1882, and *American Journal of Science*, vol. xxviii. 1884).

ing the flora to partake far more of that of the Pacific than of the Atlantic coast. It differs, however, notably from the Californian flora in the paucity of Papaveraceæ, Rhamneæ, Hydrophyllaceæ, Labiatæ, Cupuliferæ, Coniferæ, and Liliaceæ.

The distribution in the United States of America of the 480 indigenous European species,[1] which form an integral part of their flora, offers some very interesting matters for consideration. Of these there are indigenous to the

| | | |
|---|---|---|
| Rocky Mountain Flora | about | 300 |
| Eastern United States | „ | 370 |
| Californian | „ | 225 |
| Common to all | „ | 135 |

The following list of some of the Rocky Mountain species not found either in the Eastern Alps or Sierra Nevada of California is a very instructive one. No doubt some occur in the northern continuation of the Sierra Nevada, in Oregon, and Washington, &c.; such are therefore plants of the Pacific States, though not Californian:—

| | |
|---|---|
| Ranunculus hyperboreus, nivalis, pygmæus, and affinis | Adoxa Moschatellina |
| Thalictrum alpinum | Hieracium umbellatum |
| Papaver nudicaule | Campanula uniflora |
| Draba stellata, incana | Androsace Chamæjasme |
| Viola biflora | Gentiana frigida, glacialis, prostrata |
| Cerastium alpinum | Swertia perennis |
| Sagina nivalis | Lloydia serotina |
| Elatine triandra | Juncus triglumis, castaneus |
| Astragalus hypoglottis | Kobresia caricina |
| Potentilla nivea | Carex microglochin, rupestris, obtusata, frigida, ampullacea, nardina, gynocrates, incurva, stenophylla, elongata, leporina |
| Saxifraga Hirculus, flagellaris, cæspitosa, cernua, adscendens, punctata | Alopecurus alpinus |
| Chrysosplenium alternifolium | Deyeuxia lapponica |
| Epilobium latifolium | Catabrosa aquatica |

These it will be seen are for the most part Arctic plants, whose presence might be expected on any range of mountains of sufficient elevation in America; but they are absent both in the Sierra Nevada and the White Mountains of the Eastern States. On the other hand, it is not easy to account for the absence in the Rocky Mountains of an even greater number of European species which are found in the Eastern States or in California, or both, and of which some occur in most other meridians of the globe. Such are especially

| | |
|---|---|
| Caltha palustris | Trientalis europæa |
| Nuphar lutea | Euphrasia officinalis |
| Cardamine bellidifolia and pratensis | Prunella vulgaris |
| Arabis petræa | Armeria vulgaris |
| Drosera (all the 3 European species) | Myrica Gale |
| Arenaria peploides | Salix herbacea |
| Montia fontana | Betula alba |
| Oxalis Acetosella | Taxus baccata |
| Oxytropis campestris | Rhynchospora alba and fusca |
| Rubus Chamæmorus | Carex (24 species, chiefly boreal) |
| Circæa lutetiana and alpina | Tofieldia palustris |
| Lythrum Salicaria | Narthecium ossifragum |
| Lobelia Dortmanna | Luzula arcuata |
| Ericeæ (12 species, nearly all Arctic, and both European and Asiatic) | Hierochloe alpina |
| Menyanthes trifoliata | Aira atropurpurea |
| | Glyceria fluitans |

[1] In Gray's "Vegetation of the Rocky Mountain Region," cited above, will be found an exhaustive table of comparison of all the Alpine plants of the Atlantic, Pacific, and Rocky Mountain regions as then known. Coulter's "Flora," however, introduces considerable modifications in its details.

Neither the climate of the Rocky Mountain region, its elevation, its geological structure or its physical features, appear to present obstacles to the migration into it of these common plants of the colder north temperate zone from Canada or from the Eastern United States. It is still more difficult to understand how those that occur in all three adjacent American regions should have become excluded from the fourth, which they may be said to bound.

It remains to add that Dr. Coulter's manual is well got up, and though claiming in the preface to be no more than a compilation, it is really a very useful work. The only important omission observed, and it is a very serious one, is that of the elevations at which the plants grow. It is a great advance on the fragmentary "Synopsis of the Colorado Flora" by Porter and Coulter, which was welcome in its day; and though there is no doubt that a good many more species will be found in a future edition, and that some of the data extracted above will be thereby interfered with, there is no reason to suppose that these will clash with the main facts, which so amply confirm all the conclusions that Asa Gray drew years ago from the comparatively imperfect data then at his disposal.

J. D. H.

## *MORLEY'S "ORGANIC CHEMISTRY"*

*Outlines of Organic Chemistry.* By H. Forster Morley, M.A., D.Sc. (London: J. and A. Churchill, 1886.)

THE faults into which the writer of a text-book of organic chemistry for students is apt to be led are, as Dr. Morley points out in his preface, that of over-elaboration on the one hand, to which the material too readily lends itself, and that of unintelligent abridgment on the other. Dr. Morley adopts a satisfactory compromise by selecting for treatment a relatively small number of typical compounds, giving, however, "as much consideration to each compound as it would receive in a large treatise." Some departures from this rule are, as he candidly confesses, due to the necessity of taking into account "the requirements of students working for examinations."

Dr. Morley's arrangement of his subject is new—so far at least as it has not been anticipated in Prof. Remsen's "Organic Chemistry," which, however, did not appear until after the completion of the present work in manuscript. "I have endeavoured," he says, "to describe compounds in the order in which they may be synthetically produced, so that each compound should be a product of the one before and a producer of the one after." We must confess to having doubts as to how far, save in the interests of very young students, such an entire sacrifice of the symmetry of the ordinary classification as is involved in this arrangement is judicious.

Most text-books of organic chemistry contain, prefixed to each class of compounds, preliminary chapters dealing with the general modes of preparation, the properties and the reactions of the entire class. This arrangement finds no favour in the author's eyes. "Not until he (the student) finds that a series of reactions can be grouped together under some general expression should he be allowed to mention a general law." The principle is doubtless, within its proper limits, sound; but we should be sorry to lay down a hard-and-fast rule. The order of educational exposition is not necessarily identical with that of scientific discovery. Facts are more readily remembered if the principle connecting them is known beforehand. And if a teacher, by offering a few preliminary generalities, can peptonise a rather indigestible mental nutriment, why should he, on purely *doctrinaire* grounds, be forbidden to do so? Did Dr. Morley ever read a novel, and if so, did he never yield to the human temptation of anticipating the course of the narrative by looking at the end?

Dr. Morley has distributed the general reactions throughout the text, calling attention to them, however, by printing them in spaced type. The theoretical sections are as a rule brief, but to the point. An excellent account of the benzene theory is introduced; but we would point out that Hübner has given a simpler proof of the existence of the symmetrical *meta* pair of hydrogen atoms than that of Wroblewsky quoted by Dr. Morley: this proof is based upon reactions of the two nitro-salicylic acids.

The selection of facts is judicious, and, inasmuch as Dr. Morley's plan demanded that each compound should be treated of with considerable fulness, the student can hardly turn to any section of the book without learning all that is of real importance under that head.

Several inaccuracies have crept into the book, although they are not sufficiently numerous to interfere seriously with its usefulness. In his preface Dr. Morley acknowledges his indebtedness to Beilstein's treatise in his search for facts, and we fear that he has not always been sufficiently careful to ascertain how far Beilstein's statements are traversed by more recent experimental results.

Thus on p. 136 the author introduces glycerin ether—formerly dear to classification as the only example of the ether of a trihydric alcohol. But Tollens and Loë have shown that, whatever this compound may be, it is certainly not an ether of glycerin.

On p. 389 it is stated as a universal rule, that, in the conversion of diazo-compounds into substituted azo-compounds, "where nitrogen becomes attached to an atom of carbon in a benzene nucleus, the nitrogen takes up a para-position with regard to one of the groups already present." Mazzara, Witt, Liebermann, and Griess have shown that the nitrogen may also take up the ortho-position.

Following Beilstein the author has altered Wertheim's formula for conhydrine, $C_8H_{17}NO$, into $C_8H_{19}NO$. This has of course been done in order to bring Wertheim's statement, which Dr. Morley gives, that conhydrine may be broken up into water and coniine, into harmony with Hofmann's formula for coniine. But Hofmann has shown that Wertheim's conhydrine formula is correct, and that it is his experimental fact which is wrong: conhydrine does not yield coniine. In this connection it is strange that Dr. Morley makes no mention of Ladenburg's synthetic optically-inactive coniine ($\alpha$-isopropylpiperidine).

Under piperidine (p. 434) Königs's assertion that pyridine can be reduced to this compound by treatment with tin and hydrochloric acid is given. Dr. Morley must have overlooked Ladenburg's criticism of this work.

In the indigo group we find isatin (p. 386) represented as a lactam instead of as a lactim, and the so-called nitroso-oxindol (in reality isotoxim) formulated as a true nitroso-compound, instead of as an isonitroso-compound. It is of course conceivable that in these two cases Dr. Morley does not share the views put forward in Baeyer's later work on the indigo-compounds.

In the foregoing instances the information is, as already stated, merely not up to date. But there are one or two statements in the book, the source of which we are quite unable to trace. Thus we are told (p. 339) that "anthraquinone forms a compound with bisulphite of soda." If there is one thing that distinguishes anthraquinone from the quinones of the other hydrocarbons with complex nuclei—from phenanthraquinone, chrysoquinone, &c.—it is the fact that it does *not* form a compound with bisulphite of soda.

Again, under the head of ultimate analysis of organic compounds, we read:—

"Many mixtures have been suggested from time to time as substitutes for oxide of copper; the latest is a mixture of potassic chromate and precipitated binoxide of manganese proposed by Dr. Perkin."

Dr. Perkin would indeed have much to answer for if he had proposed such a substitute for oxide of copper. The mixture was proposed as a substitute for *reduced* copper, to absorb the oxides of nitrogen formed during the combustion of nitrogenous organic compounds.

The equation for the action of trichloride of phosphorus on acetic acid (p. 47) is an instance of the strange vitality which symmetrically-constructed and plausible but quite erroneous chemical equations sometimes display. We do not blame Dr. Morley for introducing the equation: it is given in all organic text-books, ancient and modern, from the time of Gerhardt to the present day, and will probably continue to be employed, translated into the notation of the distant future, at a time when our present formulæ have become as unintelligible as cuneiform inscriptions. The correct equation may however be found, by the curious in such matters, in a paper by Dr. Thorpe, (*Chem. Soc. Trans.*, 1880, p. 186), who was at the trouble to work out the reaction quantitatively.

An excellent feature, unusual in an elementary work of this kind, is to be found in the copious references, designed to encourage in students the habit of reading original papers for themselves.

F. R. JAPP

## THE SPRINGS OF CONDUCT

*The Springs of Conduct; an Essay in Evolution.* By C. Lloyd Morgan. (London: Kegan Paul, Trench, and Co., 1885.)

THIS is a thoughtful and extremely well-written little book on psychology and ethics, regarded from the standpoint of evolution. There is not much in it that is strikingly original; but the material is so well arranged, and the views so lucidly expressed, that the work constitutes a most interesting epitome of modern thought upon the subjects of which it treats. The author is a man well informed as to his facts, while his ability as an analyst may be remembered by the readers of this journal, in the pages of which it was well displayed a year or two ago in a criticism upon the work of the present writer. On that occasion Mr. Morgan took exception to the study of animal intelligence and mental evolution in animals, on the ground that it is impossible to obtain any verified knowledge of the psychology of brutes, seeing that we cannot directly interrogate them upon the nature of their feelings or mental states. The discussion which followed appears to have had the effect of somewhat modifying his original views; for these, as now stated in his book, are not so severely sceptical as they were when stated in these columns. That is to say, he now appears to recognise the possibility of comparative psychology as a science, although its subject-matter is necessarily restricted by the inadequacy of our "ejective" knowledge of animal intelligence.

We are in such full agreement with the whole essay that our only criticisms upon it refer to matters of comparative detail. These are as follows:—

Mr. Morgan gives it as his opinion that we cannot conceive of matter apart from motion (p. 94), for, in order to do so, we should require to conceive of matter as absolutely cold, "and of such absolutely cold matter we have no knowledge." The fact, however, that we have no knowledge of absolutely cold matter is no proof that we are not able to conceive of matter as absolutely cold. The so-called absolute zero of temperature surely admits of conception as definite as it would were it possible to take an actual reading of its occurrence.

Mr. Morgan's use of the word "instinct" appears to us equivocal. At one time instinctive actions are expressly affirmed to mean adaptive actions of an involuntary and unconscious kind (pp. 226-7); while at another time it is said "Mr. Darwin clearly shows that the satisfaction of any instinctive emotion carries with it a subdued form of pleasure; while, on the other hand, if those instinctive emotions be not satisfied, there results a still more marked feeling of uneasiness, which is a subdued form of pain" (p. 259). Now, clearly, there can be no such thing as an unconscious emotion, an unconscious form of pleasure, or a "still more marked [unconscious] feeling of uneasiness." Mr. Morgan thus appears to have fallen into the inevitable confusion which is the fate of all writers who fail clearly to distinguish between instinct and reflex action, or expressly to include the former term within the territory of consciousness. For these reasons we cannot follow the author's analysis where it leads up to the conclusion that volition is coextensive with consciousness (p. 226 *et seq.*). We may be conscious of the sudden anguish of neuralgia: can it be said that this consciousness is due to, or accompanied by, any act of volition? Mr. Morgan would answer that with the pain there arises a desire that it should cease (p. 229). But, in the first place, a desire is not a volition; and, in the next place, even the desire has here no time to arise before the pain is past.

In one place where Mr. Morgan refers to the views of the present writer, he represents them as differing from those of Dr. Bain, while in reality no difference obtains. First, he quotes the following passage from "Mental Evolution in Animals":—

"What is the difference between the mode of operation of the cerebral hemispheres and that of the lower ganglia which may be taken to correspond with the great subjective distinction between the consciousness which may attend the former, and the no-consciousness which is invariably characteristic of the latter? I think the only difference that can be pointed to is a difference of rate or time, which clearly implies that the nervous mechanism concerned has not been fully habituated to the performance of the response required. . . . Reflex action may be regarded as the rapid movement of a well-oiled machine, consciousness as the heat evolved by the internal friction of some other machine, and psychical processes as the light which is given out when such heat rises to redness. Consciousness is but an adjunct which arises when the physical process, owing to infrequency of repetition, complexity of operation, or other causes, involves what I have before called ganglionic friction."

Now, on this passage Mr. Morgan remarks that he does not consider such ganglionic friction so important a factor in the evolution of consciousness as is "the diffusion of nerve-disturbance" enunciated by Dr. Bain. But surely the former principle includes the latter. For it is only due to this internal friction that the diffusion of nerve-disturbance can be supposed to take place. If all the paths of nervous discharge were freely open, the nervous disturbance would course rapidly and easily along the habitual channels, with comparatively little diffusion as a result. It is only in cases where no one set of paths are more readily open than other sets that alternative directions are offered to the flow of nervous disturbance, with diffusion as a result. The resistances thus encountered—or the ganglionic friction thus created—finds its measurable expression in the delay of eventual response. But although ganglionic friction may arise from such "complexity of operation" (so leading to diffusion), it may also arise from "infrequency of repetition or other causes." Therefore the term ganglionic friction includes all that is expressed by the term diffusion, and differs from it only in being more comprehensive, or in recognising other conditions of cerebral action leading to consciousness, the occurrence of which is always expressed by delay.

George J. Romanes

## OUR BOOK SHELF

*Spectrum Analysis. Six Lectures delivered in* 1868 *before the Society of Apothecaries in London.* By Sir Henry E. Roscoe, F.R.S. Fourth Edition, Revised and Considerably Enlarged by the Author and by Arthur Schuster, Ph.D., F.R.S. (London: Macmillan and Co., 1886.)

This is a fourth edition of a well-known book, and the joint authors have evidently taken some trouble to bring the present edition up to date. To this end, the arrangement of the book, which is rather peculiar, lends itself very well. The peculiarity of the arrangement to which we refer is this. At the time that the lectures were first delivered, now nearly twenty years ago, the literature of the subject was so restricted that Prof. Roscoe found it easy and convenient to reinforce the subject-matter of each lecture by reprinting, immediately after it, the particular memoirs on which it had been based. Hence the first edition was a very precious boon to two classes of people: there was an excellent popular account of the new science, and there were the complete memoirs conveniently brought together for those who wished to go more deeply into the subject.

In the present edition an attempt has been made, as we have said, to bring the lectures more or less up to date, and considering the volume of the work which has been done since 1868, one can understand that this has been no easy task. When we pass, however, from the lectures to the appended memoirs so much cannot be said; indeed the interest of this part of the book is now chiefly antiquarian, if we except reprints of Dr. Schuster's own papers, which are given, we believe, *in extenso*, while Prof. Young's observations on the sun, now fifteen years old, is the latest information we get in the appendixes on any solar matter, English and foreign work being ignored with a magnificent impartiality. In the same manner Vogel's work on the spectra of stars, the most extensive which has been accomplished by any one single individual up to this time, is also passed over, as is also Birmingham's work on the red stars.

We give these as instances of the treatment adopted. No doubt, had the initial idea of the book been carried out in its entirety by the insertion of the most important parts of these memoirs, the size of the volume would have been greatly increased, and this perhaps may be one reason for the violently selective treatment adopted; but it may be urged on the other hand that the value of the book would have been increased much more than its size, and further, that space might easily have been gained for some of the best modern work by the omission of those papers which, as we have said before, are now purely of antiquarian interest.

There was one feature in the third edition which we also regret very much to see dropped in the present one. This was a bibliography running over twenty pages, in perhaps its most convenient form, namely, a list of authors and a complete reference to their memoirs, arranged under the larger groupings of the subject-matter.

*Trigonometry for Beginners, as far as the Solution of Triangles.* By the Rev. J. B. Lock, M.A., Senior Fellow of Gonville and Caius College. (London: Macmillan and Co., 1886.)

THIS book covers exactly the same ground as Pinkerton's, which we noticed in NATURE, vol. xxxi. p. 148. The two have many good points in common, and we should be well satisfied to use either of them as a text-book. Mr. Lock's great advantage is preceptorial skill in arrangement and exposition. On this score he deserves much credit indeed. There are very few points on which it is possible to suggest improvement. The retention of the expression "circular measure" in all its former importance, notwithstanding the introduction and constant use of the term "radian," is regrettable but not of much consequence: the mode, however, which he employs for indicating the word "radian," *e.g.* writing $\pi^c$ for $\pi$ radians, is most unfortunate, and we should hope altogether unacceptable. It is surprising too to find so skilled a teacher following the multitude in condescending to recognise those unnecessary nuisances, "tabular logarithmic sines," &c. Their existence, Mr. Lock says, is due to a typographic difficulty—a statement we hesitate to give assent to; but, be their history what it may, they serve no purpose nowadays whatever, except to roughen the learner's path. Writers require to give them a foolish name and a special symbol, to alter the formulæ for solution, and to burden the learner with additional cautions,—and all for less than nothing. It seems almost malicious indeed to force on a "beginner" such gratuitous absurdities as "natural sines," "logarithmic sines," and "tabular logarithmic sines," when the entities to be dealt with are simply *sines* and *logarithms of sines*. If Mr. Lock in a succeeding edition could see his way to inaugurate the necessary reform here, many teachers would be grateful to him.

*The Apparent Movements of the Planets and the Principal Astronomical Phenomena for the Year* 1886. Illustrated with Charts showing the Paths of the Eleven Principal Planets among the Stars. By William Peck, F.R.A.S. (Edinburgh: Archibald and Peck, 1886.)

BEGINNERS in astronomy will find this little compilation useful. Just the kind of information is brought together in it which persons interested, though not learned, in celestial phenomena want to be supplied with. Technical language, too, is as much as possible avoided, while sufficient exactness for the purpose in view is usually preserved. Not, however, invariably; the statements regarding the two solar eclipses visible in 1886 are so loose as to be misleading. Eleven miniature maps, showing the paths through the constellations during the present year of seven primary and four minor planets, are neatly executed, and ought to prove acceptable to casual observers. Exception must be taken to the introductory assertion that Copernicus swept away all the "complicated machinery of the heavens." His reform of the Ptolemaic system was by no means so complete as Mr. Peck's expression implies. The retention by the Frauenburg astronomer of the old hypothesis of equable circular motion necessitated, in fact, the employment still of no less than thirty-four circles, by which to make plain, as he said, "the entire structure of the heavens"—that is, the revolutions of the moon and of the six known planets.

## *LETTERS TO THE EDITOR*

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

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

### An Earthquake Invention

IN NATURE of July 2 last, p. 213, I was accused by Prof. Piazzi-Smyth and Mr. D. A. Stevenson of having attempted to appropriate an invention of Mr. David Stevenson. The invention referred to was the joint introduced by Mr. Stevenson beneath the lamp-tables in certain lighthouses in this country.

In my reply (NATURE, vol. xxxii. p. 573) I pointed out the fact that the aseismatic joint had been independently invented by several investigators of earthquake phenomena, and so far as I was aware Mr. Mallet had appeared to have the prior claim to this invention. My reason for attributing the invention to Mr. Mallet is that when speaking of Japanese lighthouses he says: "I was consulted by Mr. Stevenson as to the general principles to be observed, and these edifices have been constructed so that they are presumedly proof against the most violent shocks likely to visit Japan; not perhaps upon the best possible plan, but upon such as is truly based upon the principles I have developed" (Palmieri's "Vesuvius," p. 43). As the aseismatic joints were portions of the lighthouses especially designed to render them proof against earthquakes, I naturally assumed that Mr. Mallet might be the first inventor of the ball-and-plate joint.

The only occasion on which I have posed as *the* author of the aseismatic joint in question, was when Messrs. Stevenson and Smyth promoted me to that *quasi*-enviable position.

Had these gentlemen recognised the fact that they were only reading a *brief note* about ball-and-plate joints, intercalated in a collection of notes on other subjects, and had they been well acquainted with the recent literature relating to aseismatic tables, they would certainly have refrained from the objectionable accusations made on July 2.

On *more than one* occasion I have referred to Mr. Stevenson's work in Japan. As an example of such a reference, Messrs. Stevenson and Smyth may turn to the *Times* of May 26—*a date which it will be observed is prior to the date of their unwarrantable attack.* In that paper there is a long letter on "Buildings and Earthquakes" signed with my name. When speaking of my house on shot, I there say, "*This experiment was very similar to one carried out by Mr. David Stevenson with regard to the lamp-tables in several of the lighthouses on the coast of Japan.* For several reasons, among which were the movements produced by wind, I abandoned the balls, and now have my house resting at each of its piers upon a handful of cast-iron shot. These shot, which are about the size of buckshot, have so increased the frictional resistance to rolling, that the house is practically astatic, and the motion in the house is in most earthquakes only about one-tenth of what it is outside."

I make especial reference to the *Times*, first because it is a paper which many Englishmen have often the opportunity of seeing; and second, because the article in question occasioned considerable discussion.

With this reference to the relationship of my experiments to those of Mr. David Stevenson, and I will furnish others if required, I think it would only be just for Prof. Piazzi-Smyth and Mr. D. A. Stevenson to withdraw their accusation of July 2.

I must now refer to a second point which Mr. Stevenson has raised. In your issue of August 6 Mr. Stevenson appeals to the results of my experiments as showing the value of his aseismatic tables. All my experiments with aseismatic tables, whether used for seismographs or buildings, when approximating in form to Mr. Stevenson's joint were, for a variety of reasons, unsatisfactory. Under compulsion I have stated that Mr. Stevenson's joint itself, as applied to the lighthouses of Japan, has been unsatisfactory. I did so for the following reasons:—

(1) Shortly after erection the free motion of the tables occasioned so much inconvenience, that the European engineers then on the Japanese service had them clamped. For this reason Mr. Stevenson's arrangement was not adopted in lighthouses which were subsequently erected (see Brunton on "The Japan Lights," Institute of Civil Engineers, No. 1451, p. 9).

(2) I learn from the Lighthouse Department that in 1882, wishing to give Mr. Stevenson's tables another trial, several of them were put in working order. The result has been that on March 11, 1882, at Tsurgisaki, a number of lamp-glasses on the burners on the aseismatic tables were overthrown.

Some time afterwards a second shock produced a similar effect. At neighbouring lighthouses, of which there are several (two being within 8 miles), not provided with aseismatic tables, no damage was sustained.

The shock of March 11, 1882, which was felt for at least 300 miles along the coast, was severe, and its effects at Yokohama and Tokio, which are no great distance from Tsurgisaki, were carefully recorded. I am not aware that any small articles like lamp-glasses, bottles, vases, &c., were overthrown inside ordinary houses (see *Trans. Seis. Soc.*, vol. vii. part ii. pp. 41–44). The fact that no ill effects occurred at other lighthouses provided with Mr. Stevenson's tables, like those on the Inland Sea and near Kiushiu, must not be regarded as an argument in favour of the tables, inasmuch as the earthquake referred to was not felt in those districts.

I may here remark that several of the aseismatic tables are at places where earthquakes are almost as rare as they are in Britain. Mr. Stevenson tells the readers of NATURE (June 26) that his lighthouses suffered when the aseismatic tables were *not* in working order. I have shown that they suffered when they were in good working order.

Speaking generally about Mr. Stevenson's descriptions of his aseismatic arrangement, he invariably refers to it as a great success. Where it was applied, earthquakes have produced no effect, but where it was suppressed, evil effects have resulted. After inquiries at the Lighthouse Department, which is a branch of my own department, I find that the facts adduced by Mr. Stevenson are exactly the reverse of the facts which have been placed at my disposal; and from what I learn, and from my own experiments, I conclude that thus far Mr. Stevenson's tables have been a failure.

As a further support to my conclusions I will quote the following translation of a report from the chief lightkeeper at the Tsurgisaki Lighthouse:—

"Sir,—On October 15, 1884, at 4.16 a.m., very severe shocks of earthquake were felt. The aseismatic table was in working order, but the shocks were so violent that fifteen lamp-glasses out of twenty-one in use were upset and broken. The lamps thus stripped of glasses began to smoke. The milled heads of the wick-holders being shaken off, and besides the revolving machine being in motion, we had some difficulty in replacing the glasses promptly; however, we managed to put them all in proper order again by 4.21 a.m.—I am, Sir, your obedient servant, &c., &c."

The only form of aseismatic joint that I have found at all practical is one where something more like a layer of cast-iron sand rather than a bed of cannon-balls is used to break the continuity between a structure and its foundations. I arrived at this after spending much time and, I may add, a considerable sum of money, and although the method involves the same *principle* as Mr. Stevenson's tables, I hardly think he is justified in claiming my successes to back up what I cannot but feel have been his failures.

If the ways and means permit, I hope to make experiments upon a small building resting on a bed of sand or fine gravel. Should results of any value be obtained, surely Mr. Stevenson will not expect me to do more than I have done already—namely, to state the relationship which may exist between these experiments and those which he carried out at the expense of the Japanese Government.

If everything connected with earthquake investigation which involves the same *principles* as are involved in Mr. Stevenson's lighthouse tables are to be regarded as his creations, he cannot avoid claiming the rolling sphere seismograph, the rolling cylinder seismograph, the horizontal pendulum seismograph, the conical pendulum seismograph, and in short, a very large proportion of the work accomplished by the Seismological Society of Japan. To this I cannot assent. All that Mr. Stevenson can be accredited with is a particular *method* of applying a principle, and this method has to my mind been a failure.

The question of the *principle* involved in Mr. Stevenson's device is one that has been *repeatedly* discussed in Japan. As an example of these discussions I will refer to the *Transactions* of the Seismological Society of Japan, vol. iii. p. 9, where Prof. J. A. Ewing is speaking, amongst other things, about a rolling sphere seismograph, the invention of Mr. Thomas Gray. Prof. Ewing says that Mr. Gray's contrivance was an application of "the method of supporting a mass by a movable piece in such a manner that the connection with the earth was at the centre of percussion of the movable piece, the mass being at the corresponding centre of instantaneous rotation, while at the same time the supporting piece was arranged so that its movements did not introduce any disturbing force due to the action of gravity upon the mass. This kinetic property, common to all these instruments, he believed he might fairly claim to have introduced into seismometry."

If Messrs. Stevenson and Smyth see fit to comment upon these notes, I trust that they will distinctly state whether they yet consider that I have attempted "to get round the letter" of Mr. David Stevenson's invention, and whether they were justified in publishing the objectionable personalities about "a B.A. man" on July 2.

This is the main point at issue, and if they choose to neglect it, the discussion may be considered as ended.

In conclusion I may remark that it was not I who commenced this controversy. JOHN MILNE

Tokio, January 6

## On the Velocity of Light as Determined by Foucault's Revolving Mirror

A FEW years ago Lord Rayleigh raised an interesting question as to the quantity actually determined by our experiments and observations on the velocity of light. There can be no difficulty as long as the medium transmits different wave-lengths with the same velocity, but whenever the medium possesses the property of dispersion the velocity with which any one crest of a wave travels ($V$) is different from the velocity with which a group of waves is propagated ($U$); hence the question arises in each particular case, whether it is $V$ or $U$ or something depending on both quantities that we measure.

In his first article on the subject (NATURE, vol. xxiv. p. 382) Lord Rayleigh states that, in experiments with Foucault's revolving mirror, the group-velocity, $U$, is determined; but subsequently (vol. xxv. p. 52) he corrected this statement and gave $V^2/U$ as the quantity measured. A paragraph was added, however, in which the remark is made that, if a convex lens is interposed so that an image of the slit is formed on the fixed mirror, the rotation of wave-front, caused by the different velocities of different wave-lengths, and acquired on the outward journey, is neutralised during the return, so that in this case we should measure $V$.

Gouy (*Comptes rendus*, ci. p. 502, 1885) dissents from Lord Rayleigh's second view, and gives $U$ as the quantity determined; without, however, giving sufficient reason in support of his opinion.

Finally, Michelson has performed the experiment with bisulphide of carbon, and obtained a result in close agreement with $U$. In a discussion of Michelson's measurements in the *American Journal of Science*, by J. W. G., his result is said to give "no countenance" to the theory which would make the velocity observed $V^2/U$.

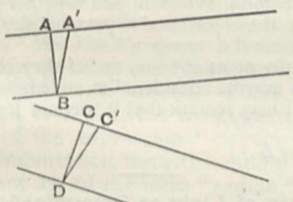
It is the object of this communication to support Lord Rayleigh's value $V^2/U$ for the arrangement considered by him, but to point out that, if only one revolving mirror is used, the experiment cann t be performed in this way. The lens spoken of in Lord Rayleigh's s.cond note is, then, a necessary condition of the experiment. When the lens is used it seems to me, however, that the result ought not to be a neutralisation of effects, but rather an equal rotation in the opposite direction. I have Lord Rayleigh's authority for stating that he agrees with this conclusion.

The method of Foucault's revolving mirror thus measures neither $V$ nor $U$, nor $V^2/U$, but $V^2/(2V - U)$. As $V$ and $U$ differ by a small amount only, the last expression becomes nearly equal to $U$, so that Michelson's experiment is in complete agreement with theory.

In Foucault's experiment, it is well known, the displacement of an image is measured which is due to the rotation of a reflecting mirror in the time of passage of the light from the mirror to a fixed mirror and back again. But in order that this quantity should be capable of measurement, it is necessary that a displacement of the mirror should not by itself alone cause a displacement of the image.

The following consideration will show what conditions this imposes on the arrangement of the experiment.

In the accompanying figure let A B be the position of a wave-front as it leaves the mirror (supposed fixed). Let C D be the position as it returns to the mirror. I have drawn the wave-surfaces plane, for the sake of simplicity, but the argument remains the same if they are curved. The point A may correspond to the point C on the returning front or to the point D. If now the mirror is displaced through a small angle, the wave-front takes a different position, A′ B. If this displacement shall not change the position of the image after reflection from the mirror, the returning wave must turn through the same angle and take up the position C′ D. But it is easily seen that, in order that this may be possible, the point A must correspond to D and B to C, for the optical length along any ray between two wave-




fronts remains the same; also, owing to the maximum-minimum property, we can still measure optical lengths in the displaced position along the original paths; and the length of the ray leaving A having been shortened by a distance A A′, the ray leaving B must be shortened by an equal amount, C C′—that is to say, the ray arriving at C must be the one leaving B, and the ray arriving at D must be the one which left A. We may then express the condition of a stationary image thus: The wave-surface must be inverted by the optical arrangement interposed, and the magnifying power of this optical arrangement must be equal to one. The last part of the condition is rendered necessary by the fact that the width of beam must remain the same; otherwise A A′ could not be equal to C C′. If both conditions are fulfilled, the image will remain distinct in the rotating mirror, otherwise it would be drawn out into a band.

But an arrangement is possible, at any rate theoretically, in which the wave-surface is not inverted. We might have a second mirror rotating in such a way as to neutralise the effect of displacement. For this purpose the second mirror ought to rotate twice as fast as the first, the light being sent on to this second mirror on its way back only, after reflection from the first revolving mirror. As the angular velocity of the two mirrors must have their velocity accurately adjusted, the experiment would be difficult to perform, but it is important to point out that it is theoretically possible.

Returning now to Foucault's arrangement, consider two successive wave-fronts as they leave the revolving mirror. The distances between them will be larger on the receding than on the preceding side of the mirror. That part of the wave-front which is on the receding side will thus be propagated more quickly in a medium having dispersion, and whatever happens to the wave-front on its journey, whether it is refracted, reflected, or inverted, that side of it which left the receding part of the rotating mirror will always gain on the other. When the wave-front returns to the revolving mirror, it will have rotated through the angle given in Lord Rayleigh's second note; but we have to consider the direction of rotation.

If that part of the wave-front which left the receding half of the mirror will return again to the receding half, the final rotation of wave-front will be in the same direction as the rotation of the mirror; and the displacement of the image which depends on the relative rotation of mirror and wave-front will be diminished. In this case, however, as I have shown, we want a second revolving mirror, otherwise the image of the slit will be drawn out into a band.

In the experiments hitherto performed, the wave-front is inverted an odd number of times between the two mirrors, and hence that part which left the receding side of the revolving mirror will now be on the preceding side. The relative rotation being increased, the observed displac ment of the image will be increased.

The total observed rotation will thus, with Lord Rayleigh's notation, be

$$\frac{4Dw}{V}\left(1 + \frac{d \log V}{d \log\lambda}\right),$$

or the velocity of light calculated will be $V/\left(1 + \frac{d \log V}{d \cos\lambda}\right)$. As $U = V\left(1 - \frac{d \log V}{d \log\lambda}\right)$, the apparent velocity becomes equal to $V^2/(2V - U)$. If $\frac{d \log V}{d \log\lambda}$ is small compared to unity, we may write the expression for the apparent velocity approximately

$$V\left(1 - \frac{d \log V}{d \cos\lambda}\right) = U.$$

In the case of bisulphide of carbon, J. W. G., in the passage quoted above, gives for the green rays $K/V = 1{\cdot}637$, $K/U = 1{\cdot}767$, $K$ being the velocity *in vacuo*. From this we get for the theoretical value of the quantity observed in Michelson's experiment 1·758, being identical with the value 1·76 actually observed.

If a second rotating mirror is used, as described, Lord Rayleigh's value $V^2/U$ remains true.

We have then as a final result, that, while the aberration of light measures $V$, and the eclipses of Jupiter's satellites and Fizeau's experiments measure $U$: Foucault's revolving mirror measures either $V^2/(2V - U)$, or $V^2/U$.

ARTHUR SCHUSTER

## Variable Stars

IN the last number of NATURE (p. 397) Prof. Seeliger, of Munich, is represented as thinking "it not improbable that the blazing forth of the *Nova* may have been due to a collision which caused an enormous development of heat and light." It appears to me that the collision hypothesis is not necessary, and that the variability of a star is a physico-chemical consequence of mere cooling. This conclusion is based on considerations relating to the formation of the chemical elements.

If we suppose a mass of primitive matter, say gaseous, to be cooling down, it will from a chemical point of view undergo a series of changes such as are indicated in the known transformations I to $I_2$, $NO_2$ to $N_2O_4$, &c.; in short, it will produce a succession of polymers. When, however, each stage of polymerisation is exactly reached, the formation of each new polymer being attended with an increase of density, will, from a physical point of view, lead to an evolution of heat. This evolution of heat will occur periodically as polymerisation goes on; and if the heat be sufficiently intense, there will be a corresponding periodic development of light. The number of such periods will doubtless depend on the nature of the primitive matter, and the difference between its temperature and that of its environment. I cannot recall an instance where, in the laboratory, more than three polymeric periods have been attained; but celestial events have a wider range and greater possibilities.

The evolution of heat which necessarily occurs at the point of polymerisation involves of course a partial reversal of the polymerisation. Elements therefore will be formed which may be regarded as derived from successive polymers by virtue of such reversal.

In the *Philosophical Magazine* for November 1884, and for February 1886, I have shown that the numerical value $y$ of the symbol of an element within the common system is given with great accuracy by the equation

$$y = 15p - 15(\cdot 9375)^t,$$

where $p$ is the number of the period, and $t$ is a whole number, easily approximated to, on the scale of celestial temperature. Our ordinary elements—that is to say, all that are known to us with the exception of hydrogen—are thus seen to correspond to what might be expected from a law of polymerisation affected by the inverse of Dulong and Petit's law of cooling. They appear to be comprised within sixteen periods only.

These considerations lead to the inference that a variable-star is one that is engaged in making "elements." All stars, therefore, either are, or must at one time have been, variable-stars.

Glasgow, March 1 EDMUND J. MILLS

### Do Young Snakes take Refuge in the Stomach of the Mother?

THE letters of Messrs. Middleton and Creole (NATURE, vol. xxxiii. pp. 176 and 269) in relation to the above-indicated reputed habit of serpents, directed my attention to some considerations and facts bearing upon the question at issue.

It has long been a popular belief—in relation to the European viper, as well as the American rattlesnake—that when the female is suddenly surprised she opens her mouth and permits her young to *run down her throat.* Some of the English viper-catchers deny that any such thing ever happens; and for a long time I was disposed to ascribe the origin of the popular opinion to the fact that, these serpents being *ovo-viviparous*, they are sometimes found with the young in the *oviduct;* which might lead persons to suppose that they were in the *stomach.* But the following account, given by the well-known French naturalist and traveller, M. Palisot de Beauvois, is so *direct* and *positive*, that my faith in the above explanation has been seriously shaken.

He asserts "that he saw a large rattlesnake, which he happened to disturb in his walks, coil itself up, open its jaws, and instantly receive five small ones which were lying by it and instinctively rushed into its mouth. M. de Beauvois retired and watched the snake, and, about a quarter of an hour after, he saw her discharge them. He approached a second time, when the young retired into its mouth with greater celerity than before, and the snake immediately moved off among the grass and escaped" (*vide* Rees' "Cyclopædia," vol. x., Article "Crotalus," cited from the *Transactions* of the American Philosophical Society). Testimony so clear and distinct from a scientific man is hardly to be gainsaid. JOHN LE CONTE

Berkeley, California, February 17

### The Coal-Dust Question

SIR FREDERICK ABEL has not fortified his statement by even one quotation from the writings of one of those workers "antecedent to and contemporaneous with" myself, who, according to his letter in the last number of NATURE (p. 417), have taken the variable specific heat of air into account in drawing comparisons between experimental effects obtained in practically open apparatuses and the corresponding effects to be expected in a great explosion taking place in the practically closed space represented by the workings of a mine.

Secondly, I am entirely at a loss to know what are the "very obvious facts" which forbid the conclusion at which I have arrived, namely, that coal-dust plays the principal part in most great explosions in mines. They have not yet been pointed out by any author so far as I have been able to learn.

Lastly, Sir Frederick's statements, to which I called attention in my letter in NATURE of December 31 (p. 197), were made for the most part before popular audiences, very few of whom will probably take the trouble to wade through the Report of the Royal Commission on Accidents in Mines in order to verify his concluding remarks for themselves. W. GALLOWAY

### Permanent Magnetic Polarity of Quartz

To my letter in NATURE for February 25 (p. 391) you have added an editorial note, quoting a passage from Tumlirz's paper which has no bearing on any of the points at issue.

In your original note you stated that Tumlirz has discovered a permanent diamagnetic polarity of quartz. To this I objected, saying that the permanent polarity was paramagnetic, not diamagnetic, and you reply by a quotation stating that quartz is a diamagnetic body showing permanent polarity, a very different statement indeed from your original one.

A permanent diamagnetic polarity could only mean that quartz placed between the poles of a magnet should show permanent north polarity at the end placed opposite the North Pole. Tumlirz is perfectly distinct and explicit that this is not the case, but that the permanent polarity acquired is in the same direction as it is in iron. There is no room for two opinions as to the meaning of Tumlirz's words.

ARTHUR SCHUSTER

[Our correspondent who furnished us with the original note informs us that Prof. Schuster was entirely right in the definition of the term diamagnetic polarity, and that his informant in Vienna had been misled in using the phrase permanent diamagnetic polarity for permanent polarity of a diamagnetic body. The sentence which we quoted from Dr. Tumlirz's now-published paper we quoted because it is the one in which he points out the essential novelty of his discovery, which we and our correspondent regarded, and still regard, as one of very great importance. Our thanks are due to Prof. Schuster for pointing out the ambiguity.—ED.]

### The "Muir Glacier" of Alaska

IN Mr. G. W. Lamplugh's interesting article on "The 'Muir Glacier' of Alaska," published in your issue for January 28 (p. 299), appears the erroneous statement that Glacier Bay opens into Chilcoot Inlet. Chilcoot Inlet is at the head of Lynn Canal, the approximate latitude and longitude being 59° 20′ N., and 135° 20′ W. Glacier Bay opens into Cross Sound (or Icy Strait) about latitude 58° 30′ N., and longitude 135° 50′ W. Lynn Canal is an extension of Chatham Straits, both running nearly north and south. Cross Sound connects the latter with the Pacific Ocean, and runs nearly east and west, entering Chatham Straits south of the southern limits of Lynn Canal.

Washington, February 25 CHAUNCEY THOMAS

## THE SURVEY OF INDIA[1]

IT has been well said that "l'exacte connaissance topographique qui est un facteur de l'avancement des sciences et de leurs applications pratiques, est aussi un élément constitutif du progrès national."[2] India is a country which, ever since the establishment of British dominion in the east, has been prolific of surveys of very various degrees of exactitude, ascending from the rough and rude reconnaissances which were needed for the speedy acquisition of some knowledge of the general geography, to, first, a fairly close representation of all topographical features, and, finally, to an exact delineation of the boundaries of all properties—of private individuals as well as of the State—in the richer and more densely populated portions of the British districts. Commencing at the coast lines, with the primary object of furnishing charts for the guidance of navigators, with a view to the rapidly-increasing traffic between India and Europe, they were extended inland, here and there, as different parts of the country became subject to British influence. Astronomical determinations of the latitude and longitude were employed in the first instance as a general basis for the geography, but not proving satisfactory, they were abandoned at the commencement of the present century, when the Great Trigonometrical Survey was originated, which has been of such value for geodesy, as well as geography. The survey work may be broadly classified as non-graphical and graphical, the former trigonometrical and geodetic, the latter delineative of the configuration of the ground and of whatever has been raised on its surface.

[1] "General Report on the Operations of the Survey of India Department, administered under the Government of India during 1883-84." Prepared under the direction of Col. G. C. De Pree, S.C., Surveyor-General of India.

[2] "Importance de la Cartographie officielle." C. D. Carusso. Genève, 1836.

For many years survey operations of various kinds were carried on independently by distinct agencies. Of these the most notable were the Revenue and Settlement Departments under the Governments of the Madras and Bombay Presidencies and of the several provinces of the Bengal Presidency, and the Great Trigonometrical, the Topographical, and the Revenue Departments under the Supreme Government. The three last were amalgamated, in 1878, into a single department, styled the Survey of India, the report of which for 1883-84 we are about to review.

The survey year in India is invariably divided into the two periods of the field season and the recess, which are mainly governed by climatic conditions, and vary greatly in different parts of the country according as the monsoons set in or terminate early or late. For the surveys under the Supreme Government it is held to commence on October 1, when most of the survey parties are preparing to leave their recess quarters and return to the field. The annual reports are prepared for the survey year, as distinct from both the calendar and the official year. This one has been issued somewhat tardily, fully a year after the close of the period it embraces.

The operations were mainly geographical, topographical, and cadastral.[1] The principal triangulation having been completed in 1882, it is now only necessary to construct occasional minor triangulations. A chain was carried along the coast of Orissa and Gaujam to establish beacons for the use of the Marine Survey. Another was contemplated in extension of the principal triangulation terminating below Mergui and Tennasserim, at the southernmost point of the British-Indian territories on the Malayan Peninsula, to connect the Straits Settlements and Malacca; but it had to be held in abeyance in view of financial exigencies; thus Singapore still remains unconnected geographically with India, though Bangkok, the capital of Siam, has been well connected by a chain of triangles carried eastwards from Tavoy. And now that Upper Burmah has been annexed to the dominions of the Queen-Empress, the triangulation to Singapore must give way to what is more urgently required for the geography of the newly-acquired provinces.

Some idea of the variety and extent of the topographical operations is afforded by the following statement of the out-turn of area surveyed on different scales during the year:—

| | | | | |
|---|---|---|---|---|
| 4034 | square miles | on the scale of | ¼ inch | = 1 mile |
| 3225 | ,, | ,, | ½ ,, | ,, |
| 7437 | ,, | ,, | 1 ,, | ,, |
| 12074 | ,, | ,, | 2 inches | ,, |
| 692 | ,, | ,, | 4 ,, | ,, |
| 110 | ,, | ,, | 6 ,, | ,, |

in addition to which areas of 875 and 310 square miles were surveyed on the 4-inch scale, the former to supply working plans for the Forest Department, the latter to enable village boundaries to be relaid in riverain tracts, where they had been swept away by floods. The topography was carried on simultaneously in widely distant regions, in Biluchistan and in Burmah, in the Andaman Islands of the Bay of Bengal, and in the Native States of Rajputana and Cutch in Western India; also in the Guzerat and Deccan provinces of the Bombay Presidency. It may excite surprise that these last, which are among the oldest of the British possessions in India, should now be under regular topographical survey, and for the first time; but as a rule more attention has been paid to the topography of our later than our earlier acquisitions of territory. Good maps were prepared for the Punjab and Oudh as soon as possible after their annexation, under instructions from Lord Dalhousie and Lord Canning, at a time when very inferior ones were forthcoming for the North-West Provinces and Bengal: and to this day some of our oldest possessions have no better topography than was acquired by reconnaissance on the ¼-inch scale early in the present century. These tracts are taken in hand as the requisite agency becomes available on the completion of surveys elsewhere.

[1] The term "cadastral" is applied in India to a field-by-field survey which gives an outline-map of all properties and statistical information relating thereto; it is derived from the French word "cadastre" = rental book, which comes from "capitastrum," a register for taxation of individuals.

Indian topography is entirely executed by the method of plane tabling on a trigonometrical basis, which, though well known on the Continent, is but little practised in England, and is not adopted by the Ordnance Survey. When a plane table is employed, all the details of the ground may be "fixed" by direction intersections laid off on the table, and all instruments for direct linear measurements may be dispensed with; this is a great advantage in surveys of tracts of mountains where chains cannot be conveniently employed, and in native States where they are objectionable for political reasons, raising a suspicion that lands are being measured with a view to annexation; and it has the further advantage of enabling the details of the ground to be drawn on the spot, whereby field-books are dispensed with and the rate of progress is much accelerated. In accuracy it cannot compete with the Ordnance Survey system of triangle-chaining; but it is better suited for ordinary topography in India, and best of all for rapid geographical reconnaissance everywhere; and it may be supplemented by chaining to any extent that may be desired for cadastral and other large scale surveys.

The out-turn of the work of the Cadastral Survey parties was as follows:—

| | Square miles | | Fields |
|---|---|---|---|
| In the North-West Provinces... | 1747 | comprising | 1,863,000 |
| ,, Burma ... ... ... | 1749 | ,, | 1,608,000 |
| ,, Central Provinces ... | 31 | ,, | 40,000 |
| ,, Assam ... ... ... | 228 | ,, | 148,000 |
| Total ... | 3755 | ,, | 3,659,000 |

Of the total area, 11 square miles were surveyed on the 32-inch scale, and the remainder on the 16-inch scale; the average areas of the fields ranged from ·12 to ·98 of an acre in different districts, the general average of all being two-thirds of an acre.

Cadastral Survey operations have hitherto been mainly carried on conjointly by the Survey and by the Settlement Departments, with the double object of furnishing correct maps of all properties, and records of the rights of every individual proprietor and tenant. Such maps and records are obviously of enormous value in the administration of the country; but they are necessarily costly, and therefore it has long been, and is still, a moot point whether they are absolutely necessary. Records of rights and liabilities to taxation were constructed at each of the successive periodic settlements of revenue under the Asiatic Governments anterior to the British, but no maps were made. During our first settlements no attempt was made to obtain accurate maps; some attempt was made to ascertain the areas of the fields with fair approximation by measures of lengths and breadths with poles and ropes of a length regulated by that of the forearm of the measurer, obviously a very variable and indifferent standard unit; and from these measures rude outline sketches were constructed which were serviceable as furnishing a graphical index to the record of rights of each village, but of course were not true maps showing all boundaries of property correctly. In the modern Revenue and Settlement Survey of the Bombay Presidency accurate map construction was deliberately set aside, lest it might interfere with the classifications of soils and the investigations of tenures and rights which were deemed of greater importance. In the Madras Presidency its advantages were recognised, and good maps were made from the first, but the results were costly. In Northern India there has been a continuous endeavour on the part of the settlement officers to improve the field survey and construct good maps; but it has been attended with varying success, indifferent at best; for though a theoretical knowledge of the first principles of surveying, which are very simple, is readily acquired, a practical knowledge of their application in the combination of a vast number of mutually interdependent measures, with all the desirable accuracy, is not so easy to acquire; it needs much skill and judicious organisation; the ground measurements are necessarily made by the cheapest agency procurable; many measurers have to be employed simultaneously, and they have to be systematically supervised in order to guard against both accidental mistakes and intentional falsifications. Thus in the Bengal Presidency the Survey Department has long been called on to co-operate with the settlement officers to some extent. All village or parish boundaries have been carefully surveyed, and plane-table sketches of the interior have been made, distinguishing the cultivated from the uncultivated lands; and the areas of the entire village, the cultivation, and the waste thus determined have been employed as a check on the gross areas derived from the field measurements of the settlement officers. The results thus obtained were long considered sufficient for practical purposes, though the maps were still far from accurate, for it was merely the areas that were checked, not the mapping of the fields and boundaries of property. But as the country grows in wealth and civilisation, the want of accurate cadastral maps is more and more felt; and the question has arisen, How are they to be obtained in the future? whether wholly by the Professional Survey, or by the Settlement Department, as formerly, but with greater assistance from the surveyors, by getting the latter to subdivide the village area into several accurately-outlined blocks, each no greater than the field-measurers can be expected to fill in with accuracy? The merits of the two rival systems are hotly contested; the Governments of the Punjab and the Central Provinces have declared in favour of the modified settlement system, whole those of the North-West Provinces and Burmah are in favour of the professional survey; and the surveyors and the settlement officers are each declared by their respective backers to be the fittest to survive in what has come to be regarded, in some quarters, as a struggle for existence.

There are three essential requirements for administrative purposes, a correct map, a classification of soils, and a record of individual rights, and at first sight it would seem desirable to have each of these performed by a separate class of experts. Thus, when the professional surveyors were called on to undertake the field measurements, it was intended that they should simply make a survey of the ground, and leave the business of classification and recording rights to others; but, though in most instances this would have sufficed to secure what was wanted, in very many it would have failed to do so, because several boundaries of property are either quite unmarked on the ground, or so faintly marked that they must be specially pointed out to the surveyor; while, on the other hand, many prominently-marked divisions of land are not boundaries of property. If, then, the surveyor simply made a survey of what he saw, assigning a number to each plot of land on his map for ready identification, the settlement officer following him had sometimes to break up, sometimes to combine, his plots, and institute a new series of numbers for the record of rights, all which caused trouble, expense, and delay, and might necessitate the construction of a second map. Thus, it was found that the surveyor had to survey not what he saw, but what was wanted by the settlement officer, and this necessitated his entering into communication with all the landed proprietors and tenants and squatters.

Now in India and Asiatic countries generally there is this standing objection to any invasion of a district by Government officials, more particularly when made with a view to inquiries about rights of proprietorship and occupancy, that the inhabitants invariably consider it a judicious precaution in their own interests to conciliate the officials and win their favour by substantial presents, even when they have no rival claimants to fear, and when they have, the conciliatory gifts are liable to merge into bribes of ruinous magnitude. Thus every district is invariably impoverished more or less by the passage over it, and still more the protracted residence in it, of a body of native officials, and the less it is subjected to such invasions the better for its welfare. In this respect there is a decided advantage in having the survey, the classification, and the record of rights executed simultaneously by the officials of a single department working under a single officer; and there is the further advantage that the presence of the villagers in attendance on the officials is required much less frequently when all three investigations are made simultaneously than when each is conducted separately. Thus in some of the districts in which cadastral operations are now being carried on, the survey officers have been called on to undertake, in addition to their ordinary duties, the classification of soils and the entry of all *undisputed* items in the record of rights, and to furnish a list of all disputed items, with the requisite information to enable the disputes to be settled either by an officer specially appointed to do so, or by one of the higher courts of justice. This new system has not been long on trial, but it is expected to be very satisfactory in being less costly to the Government and less oppressive on the people of the country. The Survey Department has the great advantage of a staff of European subordinates to control the accuracy of the field measurements; its system of operation is cheap native agency closely supervised; its officers can control the classification of soils and the preparation of the record of rights as well as the ground measurements; and close supervision is a *sine quâ non* wherever subordinate native agency is largely employed.

The question of the best system of cadastral survey is one of especial importance at the present time, for the Government of Bengal contemplates carrying out such a survey in the districts which were permanently settled by Lord Cornwallis in 1792. When that settlement was effected the Government ceased to have any direct interest in the land so long as the annual payments of revenue were made with punctuality. The land has greatly increased in value, and the lightly assessed revenue has been paid with ease and without demur; but meanwhile great changes have taken place in proprietary rights, and of these there is little official record; lands have changed hands, and the settlements of zamindars with ryots, or landlords with tenants, have not by any means been of the permanent and easy nature of the one made by the Government with the landlords; on the contrary, rents have been raised to the utmost, and disputes between the zamindars and the ryots are constantly coming before our Law Courts, which have no record of rights and no map to guide them to a correct decision. When the permanent settlement was effected, nearly a century ago, it was provided that a "patwari," or village accountant, should be maintained to keep the record of rights, and correct it up to date on all occasions of subdivision or clubbing of land; but no steps were taken to carry out this provision, and in Bengal the "patwari" has long become extinct, doubtless to the great advantage of the rich and the strong over the poor and the weak. And now the former are crying out that they do not want a survey, while the latter have not as yet commenced to ask for what they have not yet learnt to understand and appreciate. But many of the higher officials of Government think it imperatively necessary for the just administration of the country.

One of these officers maintains that there can hardly be two opinions as to the abstract desirability of a cadastral survey; that it would be difficult to conceive a greater boon to the province than would be the existence at the present moment of a complete series of cadastral maps, with their accompanying detailed records of possession and of title; that to measure the extent to which such a record would facilitate administration and promote economic progress, it is only necessary to realise the vast quantity of interminable litigation, more or less connected with the land, that burdens the civil and criminal courts, and drains the resources of the agricultural population. Out of the many criminal cases, true or false, that are brought to determine questions of title or possession in village fields—out of the concurrent and still more harassing civil litigation on the same subject—a very great proportion would certainly never have arisen at all, but for the lack of survey records, and in the remainder the same lack places equity and justice at an extreme disadvantage, and prevents the decisions arrived at from being accepted as definitive. The criminal courts decide at most the question of actual possession at the moment: the parties accept the situation for the time, and go away poorer, but not wiser, to renew the contest when opportunity and resources offer. The civil courts work in the dark, sending out "amins" to perform straggling fragments of mapping—the outcome of hearsay and village tradition rather than of any scientific process—which barely serve as precarious foundations for the court's decree, and do not secure the ready identification of the site when the litigation is in course of time reopened. There is nowhere any stand-point of knowledge or certainty, and every transaction in connection with land is either a litigation or a compromise, in which the strongest wins.

But the cleansing of this Augean stable is expected to be a work of extreme magnitude and difficulty. A Commissioner, who is entirely in favour of the experimental introduction of cadastral survey operations, reports that both zamindars who continue to levy rates which have been actually disallowed in courts of justice, and ryots who for years have taken advantage of the absence of a record to hold more land than they pay rent for, are interested in many instances in preventing the truth from being found out, and the appearance of the survey party in any estate will awaken all sorts of fraud and chicanery, all that procrastination, evasion, and quiet opposition at which both zamindars and ryots are such great adepts. To this must be added the opposition which will be offered by interested middlemen of all grades. The cadastral survey will be an opening up of all the sores of the country, a probing of old wounds, and an invitation to all and sundry to come forward and join in the great game of scrambling for rights; for in Bengal there is next to nothing to go upon.

Thus a cadastral survey will not be an unmixed blessing, and there are not a few of the higher officials who think it likely to be exceedingly mischievous, and deprecate its being undertaken. It is strongly opposed by all the more powerful zamindars. Still it is probably more alarming in prospect than it will prove to be in reality. With a view to the acquisition of practical experience on the subject, the Government has ordered a cadastral survey of the district of Mozuffurpur, which lies to the north of Patna, to be immediately undertaken as a tentative measure. J. T. W.

(*To be continued.*)

## *AËRIAL NAVIGATION*

THE account given in NATURE (p. 421) of the late experiments of the French Government with their "dirigible" balloon is very interesting and important, and in order to give it its full significance I will ask leave to offer a short explanation of the general state of the question.

In 1875 I had occasion, in writing an article on balloons for one of our leading Reviews, to call attention to the fact that some skilful and, so far as they went, successful attempts had been made not long before by two French engineers, MM. Henri Giffard and Dupuy de Lôme, to show the possibility of propelling and guiding balloons through the air.

At that time a general and strong opinion prevailed in England against such a possibility. This opinion was enunciated by various classes of people. In the first place, some writers, taking upon themselves to speak in the name of science, declared that the thing was physically impracticable. The Duke of Argyll, for example, the President of the Aëronautical Society of Great Britain, wrote:—[1]

"A balloon is incapable of being directed, because it possesses no active force enabling it to resist the currents of the air in which it is immersed, and because, if it had such a force, it would have no fulcrum or resisting medium against which to exert it. It becomes, as it were, part of the atmosphere, and must go with it wherever it goes."

Then another class of objectors were the aëronauts, who necessarily and properly commanded respect as experts in the practice of ballooning. The cleverest of these, Nadar, declared it was impossible to control the direction of balloons, on account of their lightness and large surface, and he laid down what he considered an important principle, that "pour lutter contre l'air, il faut être plus lourd que l'air." One of our most esteemed and experienced English aëronauts, Mr. Coxwell, held the same view; and another (now, alas! lamented) expert, Col. Fred. Burnaby, wrote in the *Fortnightly Review* of May 1884, an article on the "Possibilities of Ballooning," for the express purpose of asserting that the power of guiding them was not one of these possibilities. He professed to show that we were not "one whit nearer" the solution of this problem than when De Rozier and the Marquis d'Arlandes made the first ascent; he denied the truth of the French reports of what had been done; and he offered a present of 100*l*. to any one who, in a free balloon, would after travelling a certain distance return to his starting-point. And I may mention that so strong was the feeling in favour of Col. Burnaby's assertion, that the editor of the *Review* refused to insert a short and respectful remonstrance against it which I tendered to him. Then there were the host of writers in the general Press, the *Times* at their head, who argued that, as in the century since balloons had been invented nothing had been done, it was clear nothing could ever be done, and that the idea of guiding them must be a delusion, which was accordingly ridiculed unmercifully.

All this had an important practical effect; for our military authorities, who wished to make use of balloons in war, totally ignored all possibility of directing them, and confined their attention to using them captive for observing stations, as had been done in the battle of Fleurus nearly a century ago.

As it appeared to me that this opposition and incredulity were very ill-founded, and that the matter was worth more serious investigation, I sent to the Institution of Civil Engineers "A Study of the Problem of Aërial Navigation, as affected by Recent Mechanical Improvements," which they did me the honour to publish in the volume of their *Proceedings* for the session 1881-82. I attempted to show, in the first place, that the problem was perfectly amenable to mechanical reasoning, and that its successful solution involved nothing inconsistent with the teachings of mechanical science; secondly, I pointed out various reasons to account for the failure of early attempts to guide balloons; and thirdly, I showed that the result of the recent French experiments, when treated on ordinary mechanical principles, gave fair data for forming an approximate estimate of what might hereafter be done.

[1] "Reign of Law," London, 1868, p. 130.

They had sufficiently established the general practicability of the attempt, and they had obtained an actual speed through the air of about 6 miles an hour, and it was easy to argue that by suitable provisions this might be increased to 10, 20, or perhaps even 30 miles an hour.

Meantime the French, who had no insular prejudices to restrain them, continued their experiments. M. Gaston Tissandier, an eminent man of science as well as a skilled aëronaut, conceived the possibility of applying electrical power for balloon propulsion; he exhibited a working model at the Paris Electrical Exhibition of 1881, and afterwards made, at his own expense, a large balloon, with which in 1883 he obtained a velocity of 9 miles an hour. But the French military authorities (wiser in their generation than ours) here stepped in, and, with their greater resources, carried the trials still further. They commissioned two of their engineer officers, Messrs. Renard and Krebs, to construct a balloon with which the problem might be thoroughly worked out, and the result is now given. The experiment has been a perfect success; an independent velocity through the air has been attained of upwards of 13 miles an hour; the balloon has been managed, steered, and guided with the greatest ease, and it has, in defiance of the wind, been made to return to its starting-point, the test proposed by Col. Burnaby.

But the most valuable part of the communication to the Academy of Sciences has been the investigation of the bearing of the experiments on the scientific conditions of the problem. There were two points especially which, from the want of actual experience, had in former calculations to be estimated by analogy from water navigation: the resistance which a balloon would encounter at different speeds in its passage through the air, and the efficiency of the screw propeller in overcoming this resistance.

First, as to the resistance. M. de Lôme estimated this by the midship section, but in a vessel much elongated the length has also to be taken into account. Prof. Rankine has given a rule for ships according to the wetted surface, and also another dependent on the displacement. Adapting these to air, and making certain additions which M. de Lôme estimated as special to the balloon, I obtained for the resistance in lbs., the diameter and length being in feet, and the velocity in feet per second—

By the skin friction $R = 0{\cdot}0000477 dlv^2$;
By the displacement $R = 0{\cdot}0000886 (d^2l)^{\frac{2}{3}} v^2$.

Now, taking the proportion of MM. Renard and Krebs's balloon at $l = 6d$, these equations become—

$$R = 0{\cdot}000286 d^2v^2,$$
$$\text{and } R = 0{\cdot}000292 d^2v^2.$$

The result of the latest French experiments is, when put in English measures—

$$R = 0{\cdot}000320 d^2v^2.$$

This is a little higher than the estimation by the former methods, but it corresponds sufficiently well to give confidence in the general mode of inquiry.

Secondly, as to the efficiency of the screw propeller. This has been often investigated for water navigation, and it may be said that an efficiency of 70 per cent. is fairly borne out by experience. MM. Renard and Krebs obtain for their screw an efficiency of only about 50 per cent.

It is, I should think, highly probable that by further experience both these elements may be considerably amended; but even taking the facts as they are, they show the attainment of considerably higher speeds to be perfectly practicable. A balloon of 50 feet diameter, for example, would carry power sufficient to give a speed of upwards of 20 miles an hour, and still leave a considerable buoyancy disposable.

At any rate, let us hope that we may have no more quasi-scientific declarations of the impossibility of propelling and guiding balloons, and no more sneers at those who attempt to solve the problem. The capabilities of aërial locomotion of this kind must (as I have fully shown elsewhere) be necessarily limited, but its utility in certain situations would be incontrovertible. The President of the Institution of Civil Engineers, Sir F. Bramwell, speaking in January 1885, said:—

"There may undoubtedly be particular circumstances in which this mode of locomotion would be useful, such, for example, as the exploration of new countries, or as the present Egyptian campaign. I strongly suspect that if our lively neighbours, instead of ourselves, had been invading the Soudan, they would long before this have had a dirigible balloon looking down into Khartoum."

And we have now a curious comment on his words, as we know that at that very time there was, lying in its shed near Paris, a balloon which, though perhaps it could not have saved Gordon, might certainly have saved poor Burnaby, and otherwise have been of incalculable benefit to our military operations. WILLIAM POLE

## *MEDICAL STUDY IN OXFORD*

STATUTES for the regulation of the qualifications of Candidates for Degrees in Medicine and Surgery, and for creating a Faculty of Medicine in the University of Oxford, have after prolonged discussions been approved by Congregation in their definitive form. The Statute which places the medical studies of the University under the control of the new Board of the Faculty of Medicine recently received the final sanction of Convocation, and the other statutes will soon follow. The interest which these Statutes have excited could certainly not be attributed to the radical nature of the changes which they will initiate. It must be rather due to the circumstance that the establishment of the new Faculty is regarded as an indication that Oxford, which has hitherto stood alone as the only University in the United Kingdom which has no mdeical students, and in which there is no organisation for medical instruction, now intends to undertake this function.

It is well known that the Oxford Medical Degree is one of the most coveted professional distinctions, but it does not, like that of Cambridge or Edinburgh, mean that the possessor of it has been trained either in science or in medicine at Oxford. In future there is reason to hope that it will be otherwise—that the University will no longer confine itself to the giving of degrees, but will teach all those branches of medical knowledge which come within the range of University studies.

Chemistry, human anatomy, and physiology, are the three subjects which constitute the scientific foundation of medical education, the last-mentioned being itself founded on the other two. For the instruction of medical students in human anatomy, the University has lately imported from Edinburgh an accomplished and experienced teacher, Dr. Arthur Thomson, who has already as many pupils as he can find room for; and the completion of the new laboratory has rendered possible the development of practical work in physiology. But the mere providing of the means of instruction in these subjects is insufficient, unless the lectures and laboratory work are so systematised as to enable the student to learn all that he needs to learn within the limited time at his disposal, and at the same time each branch of teaching is sufficiently specialised to adapt it to his requirements.

The bearing of the new statutes on medical education in the University can be best understood in relation to the course of scientific training which an intending student of medicine will, if they are passed, be able to follow. Hitherto the Oxford graduate who has obtained honours in biology, has been in no better position as regards his medical curriculum than he would have been had he taken an ordinary pass degree. For the statute now in force, which we hope in a week or two will be replaced by a better, does not permit him to present himself for his medical examination in anatomy and physiology until two years after his degree. In future it will be possible for him to do this at the same time with his examination in the Honour School in the same subjects; the practical effect of the change being that two years will be saved, and that he will be enabled and encouraged *as an undergraduate* to study human anatomy and physiology in their relation to medicine.

In Oxford, as is well known, every man who intends to take a degree in Arts, which is a necessary precursor to that in Medicine, must have passed the examination in classics and mathematics, which is known as "Moderations," and inasmuch as he is not permitted to present himself for this examination until the fourth term after matriculation, it is obvious that he is practically precluded (supposing him to have Medicine in view) from beginning his scientific education until after this period. Consequently, of the four years which intervene between matriculation and his final examination in the Natural Science School, only three at most are at his disposal for the study of physics, chemistry, anatomy, and physiology.[1] Of these three years one at the very least is occupied in acquiring a sufficient knowledge of the four subjects to pass the very thorough and practical "Preliminary Examination." In the scanty remainder of time which is thus left to him he has to get through the most important part of his Oxford work. He has to acquire such proficiency in physiology as will enable him to obtain a class in the Natural Science School, in addition to a very detailed knowledge of human anatomy and a limited acquaintance with organic chemistry. Under present conditions this can only be accomplished by men of exceptional power of work. The rest find it to their advantage to defer their anatomy and chemistry for another year, and consequently are not in a position to enter on their hospital studies until five years after matriculation. As this is more time than men of moderate means can spare, our system cannot be considered satisfactory until the medical student is enabled to devote the four years of University residence entirely to scientific education. The occupation of the first year in studies which, however excellent in themselves, do not fit him for the hard work before him is a grievance of which he may justly claim to be relieved. As, however, the exemption of natural science students from Moderations has already in principle been accepted by Congregation, there can be little doubt that, in the course of a year or two, the desired change will be carried into effect.

The great success of the School of Medicine at Cambridge, although no doubt chiefly due to the ability and energy of the men who are at the head of it, affords evidence that the conditions under which medical education is there conducted, are in themselves well adapted to the requirements of students. It is therefore of interest to compare our own proposed arrangements with those of Cambridge. Assuming that, with the aid of the new Faculty of Medicine, we succeed in giving effect to the views above indicated, there will still be fundamental differences between the two schools.

The first of these is that whereas here every aspirant to the degree in Medicine must have first graduated in Arts, the Cambridge student becomes from the moment that he has passed the "Previous Examination" free to devote himself exclusively to medicine. This of course means that, whereas in Oxford at least four years must intervene between Matriculation and the first examination in medicine, in Cambridge the corresponding point is reached in three years, or even in two,—a loss of time which is chiefly due to the circumstance already referred to, that the year which intervenes between Responsions and the First Public Examination, is by the Cambridge medical student devoted to physics, chemistry, and biology. It is not, however, in this way only that our Oxford system tends to lengthen the course of medical study. Our "Preliminaries," which are adapted to the requirements of an Honour School, are at Cambridge represented by a pass examination in the same subjects, specially intended for medical students, and therefore presumably easier. But it is a point of much more importance that our medical candidate is required, before he presents himself for any medical examination, to have obtained a class in one of the branches of biology. On general grounds there seems reason for thinking that this is disadvantageous, for it does a man harm to compel him to pass an honour examination for, if one may so express one's self, pass purposes. Of the two subjects open to him, the medical student naturally chooses physiology, not only because it is more closely related to medicine, but because he may hope, by obtaining a first or second class, to exempt himself from further examination. In that subject the schedules are framed for the purpose of affording first or second class candidates the opportunity of showing their knowledge. Consequently, in attempting to prepare for it, the medical student who studies physiology specially for the sake of its applications to medicine, acquires a knowledge of wider range than he requires, but of imperfect quality. The evil is a serious one, but happily not difficult to remedy—either by establishing a pass examination in physiology, or, more simply, by marking off in the schedules those subjects which are of less importance to the medical student than the rest.

Enough has been said to show that, however insignificant the position of medicine in Oxford may seem to be at present, it is not likely to remain so. One of the difficulties in the way of medical study here—the non-recognition by the licensing bodies of Oxford teaching—was removed a year ago. The other is the occupation of time, of which the student requires every moment for his preliminary training in physics, chemistry, and elementary biology, with studies which, excellent in themselves, are not conducive to his purpose. From the moment that we are relieved from this drawback, we shall have everything in our favour, and success or failure will depend on our own exertion. In the meantime, it is not too soon to proceed with the organisation of our system of studies, so that, when the opportunity is offered for efficient action, we may be prepared to take advantage of it.

J. B. S.

[1] For more detailed information as to the course of study in Oxford see an article by the writer in the *Oxford Magazine* for January 27, 1886.

## *CHARLES WILLIAM PEACH*

At the ripe age of eighty-six this genial and enthusiastic naturalist has at last passed away. Never was there a more notable example of the irrepressible instinct of a true lover of nature. Born in Northamptonshire, he eventually joined the Coast-Guard service, and was stationed at various parts of the coast where smuggling went on apace and where his shrewdness and tact were often more than a match for the daring spirits who defied the revenue laws. But in the intervals of his duties he found time for close observation of the living things he met with along the shores and of the plants, insects, birds, and fishes he saw inland. Working in the pre-Darwinian days, when the adding of new species to the known list was one of the chief aims of natural history students, his zeal was early enlisted on behalf of the species-makers. Some twenty species and several genera of sponges were first made known by him as inhabitants of our seas. He considerably augmented our list of native hydrozoa and polyzoa. Among the naked-eyed medusæ, echinoderms, mollusks, and fishes he also materially increased our knowledge. One of his distinguishing characteristics was his readiness to tell everything he knew to any naturalist engaged in the investigation of the departments of zoology in which he himself had worked. He was a keen observer rather than a trained naturalist. He published little himself, but he contributed rich materials to those who knew how to make the best use of them. He was consequently a valued correspondent of many of the leading naturalists of his day, who gladly acknowledged their indebtedness to his generous aid. Nor were his observations confined to the living things of the existing creation; he searched the rocks around him for traces of former plants or animals, and found them in places where no one had ever seen or suspected them before. His keen eye detected the first relics of fossil fishes in the Devonian rocks of Devonshire, and when, after his transference to the north of Scotland in 1849, he had an opportunity of looking at the limestones of Durness, he soon brought to light a series of fossils which, in the hands of Murchison and Salter, proved of the utmost value in fixing the geological age of the rocks of the North-West Highlands. After his retirement from the public service he went to reside in Edinburgh, and devoted himself with all his old enthusiasm to the exploration of the fossil flora of the Carboniferous rocks of that neighbourhood. Nothing seemed ever to escape his notice, and hence even from the quarries and sections where many a practised eye had preceded his own he was able to glean materials which no one but himself had noticed. In recognition of his important services to the cause of natural history, the Royal Society of Edinburgh in 1875 awarded to him the Neill Gold Medal. His health has for some time past been failing, and he has now gone to his rest with the affectionate regrets of all to whom the progress of natural science in this country is dear. His son, Mr. B. N. Peach, of the Geological Survey, with all his father's enthusiasm and more than his father's range of acquirement, will, we hope, for many a long year, preserve among the naturalists of this country a family name that is familiar as a household word.

## *PROFESSOR EDWARD MORREN*

CHARLES JAMES EDWARD MORREN, whose death on the 28th ult. we announced in our last issue, was the son of Charles Morren, a Professor in the University of Ghent, and was born in that city in 1833. Shortly afterwards the father removed to Liége as Professor of Botany. The son, Edward, as he was usually called, was educated for the law, but evincing a strong tendency towards the natural sciences and chemistry, took his degree in the Science Faculty with much distinction. Owing to the ill-health of the father, Edward Morren was early called on to undertake the professorial duties, but the continuation of his licence to teach was made conditional on his undergoing a "special" examination for the Doctorate. This was the occasion of the publication of his dissertation on green and coloured leaves, by which he first became known to the botanists of Europe. After the death of Charles Morren, in 1858, the son was appointed in the father's stead, and from that day to this, the aim of the son seemed to be to walk in the steps of his father, and to complete and extend his work. Both devoted themselves not only to botany but to chemistry, and in particular to horticulture and agronomy. Both were imbued, as so many of the Belgian *savants* are, with an ardent patriotism which led them to devote their science to the practical good of the nation, and to hold up to honour and respect the work of their celebrated predecessors. Hence, from father or son, or both, we have memoirs of Dodoens, of de l'Obel, of de l'Escluse, of Fuchs, and other worthies of Flemish nationality.

Both were impressed with the necessity of extending and adapting to the necessities of the times the system and the means of botanical education. The Botanical Institute of Liége, which Edward Morren lived to found and to see completed, was but the modified outcome and extension of the plans and schemes originally proposed by the father. The result is that Liége is now equipped with a compact and well-ordered laboratory for botanical tuition and research, such as some of our own Universities might envy. In order to perfect this institution Morren availed himself of his frequent travels to study the method of instruction followed in the Universities of Germany, and the organisation of the scientific establishments of Holland, Paris, London, and other centres. With his professorial work, his ceaseless duties in connection with official horticulture and the publication of the *Belgique Horticole*, Edward Morren necessarily found little time for the preparation of any separate work, but his memoirs and academic dissertations are numerous. The most important of them, as may be gleaned from what has been said, referred to questions of chemistry and vegetable physiology. A paper published in this country in the Report of the London Botanical Congress, 1866, comprises a most elaborate investigation into the action of sulphurous acid and other vapours on plants.

His academical discourses and popular lectures were remarkable both for their method and their matter. With the fluency and elegance of style of a practised orator, Morren combined the fulness of knowledge and accuracy of exposition of a man of science. Botanists, however, were looking forward with expectancy to a monograph of the Bromeliaceæ from his pen. It was known that the Professor had been accumulating for many years material for this purpose. His collection of living examples is, we believe, the largest and best selected in existence, and the materials in his herbarium and very extensive library (the most complete of its kind in Belgium) are in their way equally remarkable. Beyond detached fragments, however, Morren published little on this curious family.

Death has overtaken him, as it did his father, when little or not at all beyond the prime of life, and it has caused a void which only those who knew the warm-hearted, genial, liberal-minded Professor can fully appreciate.

## *THE WEATHER*

OVER the greater part of the British Islands last month was one of the coldest Februarys on record, the mean temperature at Greenwich being only 33°·8, or 6°·8 below the average of the month. Throughout Great Britain generally, from the Grampians to the Channel, temperature was from about 5°·0 to 7°·0 below the means of the stations. But in the northern and western divisions of these islands temperature was only from about 2°·0 to 3°·5 below the monthly averages. This difference was mainly occasioned by the distribution of temperature during the second week of the month, owing to the higher temperature in the north and west accompanying the storms which prevailed in the far north during the time. Thus during the week ending February 13, the mean temperature of Parsonstown was 43°·5, whilst at Oxford it was so low as 33°·8, or nearly 10° lower.

From the middle of February, however, to the memorable snowstorm in the beginning of March, the weather-maps of Europe presented several remarkably persistent noteworthy features. The commencement of the period was marked at the Ben Nevis Observatory by forty-eight hours of singularly dry clear weather, such as occurs in connection with anticyclones and the settled weather attendant on them. Eastern and Northern Europe was now even more pronouncedly than it had been in the earlier part of the month the theatre of a widely-extended anticyclone, which slowly shifted its position from day to day, and sent out from its central regions winds in all directions, differing much in their climatic qualities. On the other hand, Western and Southern Europe was marked by an atmospheric pressure persistently and greatly lower, with an absence of anything approaching a cyclone—usually a characteristic feature of the weather at this season—if we except a cyclone, not very decidedly marked, that appeared in the Bay of Biscay on February 25, and thence passed slowly eastwards across Italy and Greece, towards Asia Minor, which was reached on March 2.

The inevitable consequence of this distribution of atmospheric pressure was a prevalence of calms and of easterly and northerly winds over the Continent, with low temperatures. For five days, ending February 19, the anticyclone had its centre near Moscow, during which time the barometer, at 32° and sea-level, rose to 30·965 inches. Meanwhile temperature steadily fell, and $-7°·6$ was recorded at Moscow on the morning of the 19th. The anticyclone thence advanced northward to the White Sea and westward to the Gulf of Bothnia, a pressure of 30·961 inches being recorded at Haparanda on the 22nd, and 30·922 inches at Uleaborg, in Lapland, on the 24th; and thereafter southward to Stockholm with a pressure of 30·603 inches on the 28th, to Riga with a pressure of 30·742 inches on March 1, and to Charkow with a pressure of 30·398 inches on the 2nd. The central regions of the anticyclone were throughout, as happens at this season, marked by unusually low temperatures, the lowest being $-16°·8$ on the 23rd and $-18°·8$ on the 24th at Archangel, and $-15°·5$ on the following morning at Haparanda, these temperatures being about 30°·0 below the average for this time of the year at these places.

The weather-maps show that an important change had already set in on the morning of the last of February, the curvings of the isobaric lines pointing to a cyclone to the north-east of the White Sea, and to another cyclone advancing to the south-west of the British Islands. The anticyclone was thus now surrounded by three cyclones, located respectively near the White Sea, to the south-west of the British Islands, and in the Mediterranean. On the morning of March 1 the most northern of the cyclones had travelled somewhat to westwards, and the other two to eastward; and these respective movements were continued on the following day. In the meantime the anticyclone had greatly shrunk in breadth, and by the morning of the 2nd, when the snowstorm raged most fiercely over an unusually extensive breadth of country, it lay as a narrow tongue of high pressure westwards over Scandinavia, and, meteorologically considered, perilously close on to the cyclone whose centre was then about the Humber. It necessarily followed, from the contiguity of the high-pressure area to the cyclone on its north side, that the storm passed across the British Islands with uncommon slowness, thus prolonging its continuance in Great Britain; and that the steepest gradients were formed in the north-east quarter of the cyclone,—a rather unusual feature of the storms of North-Western Europe,—thus exposing North Britain to one of the worst easterly gales of recent years.

Some snow fell in a few places on Sunday, but on Monday it fell in almost all parts of England, the fall being particularly severe in North Wales and the northern counties. The Furness and Wigtownshire railway lines were blocked and traffic suspended, a circumstance that has not occurred since these railways were opened, which as regards the Furness line is twenty-seven years ago. In the more southern counties the storm was not quite so severe, and as the day advanced the snow changed to sleet and at last to heavy rain. On the Tuesday the storm spread northwards over all Scotland, and raged with a fury altogether unexampled. Owing to the fineness of the snow-particles and the force of the wind, snowdrifts in many places accumulated in a degree quite unparalleled, and all transit was seriously paralysed. The most serious railway block occurred on the East-Coast line, and it was computed that thirty trains of various sorts were snowed up between Newcastle and Berwick. Letters carried by the London Monday mail were not delivered in Edinburgh till Friday morning. The sensation produced by the rapidly-driven snow-particles on the face resembled the sharp pricking of a shower of needles; and it was remarked that the effect of the snow-drift on the eyes gave the feeling which would be produced by the spray of dilute nitric acid. As respects the singular character of the snowfall, it may be suggested that it was in some way connected with the remarkable meteorological conditions described above as having overspread Eastern and Northern Europe during the fortnight preceding the storm, and the proximity of North Britain to the anticyclone when the storm raged in all its fierceness.

It is remarkable that, while the snowfall was large in many western as well as in eastern districts, it was comparatively light over the higher midland parts of Scotland, and that on Ben Nevis and surrounding mountains little snow fell. It is to be noted, however, that at the Ben Nevis Observatory the wind blew, not as is usual on such occasions, from a different direction, but from precisely the same direction as at lower levels, with a force, however, very greatly diminished, the mean wind-force for the day being estimated by Mr. Omond at only 4 of the Beaufort scale. For the week preceding the storm the mean pressure and temperature of the air at the Observatory were respectively 25·482 inches and 14°·9; and at sea-level at Fort William, 30·154 inches and 33°·2. This mean pressure at the Observatory is 0·046 inch in excess of what previous observations show to be the mean when the sea-level pressure and temperature of the air is as above. On the morning of the storm the excess was double that of the previous week. It is these departures from the average in their relations to the cyclones and anticyclones of this part of Europe that give the Ben Nevis observations their great significance.

## NOTES

It will be remembered that the Paris Academy of Sciences on Monday week, after hearing Prof. Pasteur's account of the cases he has treated, appointed a Committee to consider the question of the establishment at Paris of a vast international hospital. On Monday last M. Vulpian communicated to the Academy the following proposals, unanimously agreed upon by the Committee:—(1) An establishment for the treatment of rabies shall be founded at Paris under the name of l'Institut Pasteur. (2) This Institution shall be open both to French subjects and to foreigners bitten by dogs or other rabid animals. (3) A public subscription is opened in France and abroad for the foundation of this establishment. (4) The employment of the funds subscribed shall be made under the direction of a Committee, consisting of Admiral Jurien de la Gravière, President of the Academy of Sciences; M. Bertrand, M. Vulpian, M. Marey, M. Paul Bert, M. Bichat, M. Charcot, M. Hervé Mangon, M. de Freycinet, M. Camille Doucet, M. Wallon, Vicomte Delaborde, M. Jules Simon; M. Magnin, Governor of the Bank of France; M. Christophile, Governor of the Crédit Foncier; M. Alphonse de Rothschild; M. Beclard, Doyen of the Faculty of Medicine, and Perpetual Secretary of the Academy of Medicine; M. Brouardel, Professor to the Faculty of Medicine, and President of the Consultative Hygienic Committee of France; M. Gaucher, Professor to the Faculty of Medicine of Paris. (5) The subscriptions shall be received at the Bank of France and its branches, at the Crédit Foncier and its branches, and at the Public Treasury Offices. The names of all subscribers shall be inserted in the *Journal Officiel*.

THE Stockport people have a pleasantly hearty way of testifying their appreciation of the services of scientific men. A series of popular science lectures has been given this winter under the auspices of the local Society of Naturalists and the directors of the Mechanics' Institution. The fourth of the series was on "The Life of Pasteur," and on the conclusion of the lecture it was proposed from the body of the hall that an address of thanks should be sent to the illustrious Frenchman. In accordance with the resolution an illuminated memorial has been prepared for transmission to M. Pasteur.

M. CHEVREUL has so far recovered that he has been able to walk out in spite of the low temperature prevailing in Paris.

SIR JOHN LUBBOCK has been appointed Rede Lecturer to the University of Cambridge for the ensuing year.

M. CORNU, member of the Paris Academy of Sciences and Professor in the Polytechnic School, has been appointed a member of the Bureau des Longitudes.

THE Council of the St. Petersburg University has awarded Prof. P. A. Tlienkoff's premium of 500 roubles to Prof. P. T. Brounoff of the same University for his work on "The Laws of the Movement of Cyclones and Anticyclones, especially in Russia." For the next year's competition the same premium is offered for the best work on the Compositæ of European Russia.

THE late Dr. Davidson had a world-wide reputation as one of the most eminent of British palæontologists, and freely gave his life-long services for the advancement of science. He also rendered, for a series of years, great and valuable services to Brighton; chief amongst which may be mentioned the arrangement of the Brighton Free Museum in its present *locale*, the presentation to it of a fine series of volcanic products, and of his collection of rocks and fossils from the Paris Basin—one that is unique of its kind and of very great value because it was obtained whilst the fortifications of Paris were being constructed, an exceptional circumstance not likely to recur; in addition to these the late Dr. Davidson acted as Chairman of the Museum Committee for several years, and filled that office at the date of his death. Whilst holding this position his attention to and care over the best interests of that institution were unremitting, and he was always ready, at his own expense, to purchase specimens for the public benefit, whenever he thought it desirable to fill up gaps in any series in the museum, on behalf of which his scientific influence was always freely exercised. It is felt that such disinterested and valuable services should be permanently commemorated in the museum in which Dr. Davidson laboured so assiduously and which he loved so well. With this end in view, the Mayor of Brighton, Mr. E. J. Reeves, on behalf of the members of the Pavilion Committee of the Town Council, and of the Museum, Fine Arts, and Library Committees, invites his fellow-townsmen and Dr. Davidson's personal and scientific friends to contribute towards the raising of a fund to be devoted to the placing in the museum of a memorial to the late Dr. Davidson.

THE Royal Meteorological Society's Exhibition of Barometers will be held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Tuesday and Wednesday next. The Exhibition will be a most interesting one, as a large number of various forms of barometer have been brought together, many of them being of great value, and some being the only specimens known to exist. At the meeting of the Society on Wednesday evening, the President, Mr. W. Ellis, will give an historical account of the barometer. Any persons, not Fellows, wishing to visit the Exhibition or to attend the meeting, can obtain tickets on application to the Assistant Secretary, Mr. W. Marriott, 30, Great George Street, S.W.

OF the three colleges—Columbia, Harvard, and University of Pennsylvania—that received the benefit of the Tyndall Fund, Columbia has been the first to act, we learn from *Science*. Her trustees have recently drawn up a series of regulations in regard to the John Tyndall Fellowship. The Fellow, who is to be appointed on the recommendation of the President and Professors in the Scientific Department, must pursue a course of study and research in experimental physics for the term of one year, and he may be reappointed. The first incumbent of the Fellowship is Michael Pupin, who graduated at Columbia in 1883 with honours, and has since his graduation been studying mathematics and physics at Cambridge, England.

M. GUERAULT, the well-known electrician and secretary of the *Lumière Électrique*, has died, at the age of forty, from consumption. He was well known in England, where he stayed during some years to learn engineering.

A VERY interesting entomological conference took place recently at Odessa; it was organised by the Zemstvos of nearly all the southern provinces of Russia. It appears from the reports read by the respective representatives, that nearly all the southern districts of Russia suffer more or less from different obnoxious insects and other animals, but principally from the Hessian fly and marmots. The latter are especially destructive to the corn-fields, and the Zemstvos found themselves compelled to encourage the extermination of the plague by offering a reward of from one to three kopeks (one to three farthings) for each animal killed. During the year 1885 alone the Zemstvo of Kherson thus paid over 100,000 roubles, this sum corresponding to 6,600,000 animals killed, while in 1883 over 2,000,000 of these animals had been killed.

MR. WESTWOOD OLIVER, with the assistance of Messrs. E. W. Maunder, F.R.A.S., W. F. Denning, F.R.A.S., T. E. Espin, F.R.A.S., A. Cowper Ranyard, F.R.A.S., T. Gwyn Elger, F.R.A.S., J. E. Gore, F.R.A.S., J. Rand Capron, F.R.A.S., Howard Grubb, F.R.S., F.R.A.S., W. S. Franks, F.R.A.S., T. W. Backhouse, F.R.A.S., and other well-known observers, has in preparation a practical manual of "Astronomical Work for Amateurs," the aim of which is to help the possessors of limited instrumental means to turn their attention to astronomical researches of real scientific utility, special attention being directed to the comparatively new fields of spectroscopy and celestial photography. The book will be published by Messrs. Longmans and Co. Mr. Oliver invites suggestions from practical workers, which may be sent to him at Lochwinnoch, Scotland.

WE have received Parts I. and II. of "British Petrography, a Description of the Ordinary Rocks of the British Isles," by J. J. Harris Teall, F.G.S. The publishers are Watson Brothers and Douglas, Birmingham.

ON March 15, 22, and 29, Prof. Bonney lectures at the London Institution on the "Making of Mountains."

THE Arago Laboratory created by M. de Lacaze-Duthiers in France is attracting much attention on the Continent. This laboratory serves as a counterpart to that formed at Roscoff, and is performing an excellent work. It is constructed in such a manner as to be capable of resisting excessive heat, which always militates considerably against the operations carried on at this and all similar laboratories.

THE artificial reproduction of the sole is being energetically carried on in France, where a laboratory was established so far back as 1881 by Dr. Joussett of Belleyme especially for this purpose. Since that time the ova of the sole have been regularly incubated with success, notwithstanding the numerous difficulties attending the process.

THE French Consulting Committee of Hygiene recently advised the prohibition of the use of vaseline for butter in food preparations. The effects of vaseline on the system, however, seemed to require fuller examination, and Dr. Dubois has made some experiments in regard to it. Two dogs were fed exclusively on soup in which the usual fat was entirely replaced with vaseline; one of them absorbed 25 grammes of vaseline a day for ten days, the other 15 grammes (this would correspond in the case of an average man to 100 grammes and 60 grammes respectively). With this diet the animals even slightly increased in weight. Their general state was good: there was no loss of appetite, nor vomiting, nor diarrhœa. In general it may be said that the carburets of hydrogen forming vaseline, though they favour neither oxidation nor saponification like fats, are readily tolerated in the alimentary canal, at least in the case of dogs. Further experiments will show if a prolonged use of the substance is equally innocuous.

IT is estimated by the Marquis de Nadaillac (*La Nature*), that Europeans can endure temperatures as widely apart as 130° C. at least. Thus, on January 25, 1882, a temperature of − 65° C. was recorded on board the *Varna* and *Dijmphna*, when blocked by ice in the Sea of Kara, east of the Straits of Waigatz. On the other hand, M. Duveyrier, in the country of the Touaregs, in Central Africa, has seen the thermometer rise to 67°·7 C.

A RECENT number of *Globus* contains an article by Prof. Nehring, on an interesting prehistoric discovery made in the neighbourhood of Madgeburg. At the village of Westeregeln, between that city and Halberstadt, in the course of some work the labourers came on the remains of an ancient grave, containing parts of the skeleton of an unburned human body, near which were about 112 bored dogs' teeth, two decorated shells of a river shell-fish now only found in Southern Europe, the *Unio sinuatus*: two pieces of an easily-burnt resin, the remains of one or more clay vessels, and a small highly-oxidised bronze ring, which appears to have been used as a finger ornament. The teeth, from their formation, must have been collected from twenty dogs at least, and they were all bored through the root portion, and were evidently meant to be hung on a string. With reference to the shell, it is noticeable that here and there in the Rhine provinces similar shells are found with Roman remains. Dr. Nehring is inclined to look for an explanation of this circumstance rather to an importation from Southern Europe than to the theory that the *Unio sinuatus* has died out since the Roman period in the Rhine provinces. Ornaments of the teeth of Carnivora for the neck, waist, arms, &c., have been found in prehistoric mounds or graves elsewhere in Germany; and even now they are in use for a similar purpose amongst certain primitive peoples. The Igorrotos of Luzon use them for necklets and earrings; so also do the inhabitants of the islands in Torres Straits.

AT the annual meeting of the London Sanitary Protection Association the Report stated that the number of members is now over 1000, and the total number of inspections made during the year 1264; a large number having been made in the suburbs of London and several in the country, including that of Eton College and other large public institutions. Unfortunately the general character of the houses inspected was as insanitary as ever, only 5 per cent. being found in perfect order, and 9·5 per cent. in fairly good order; whilst in 60 per cent. foul air was escaping directly into the house, and in 24 per cent. sewage was partly retained underground by leakage or choking of pipes.

DR. SCHLIEMANN, who has been busy at Berlin for the last few days arranging in the new Ethnological Museum the fruits of his recent excavations, intends to return to Athens shortly. The doctor promises that Berlin shall be the ultimate inheritor of all his archæological treasures.

A TELEGRAM from Catania announces Mount Etna to be in a state of eruption. Cinders and stones are being continually thrown up, and it is supposed that lava is coming out of the crater, but as it is covered by a dense mist no proper observations can be taken. Slight shocks of earthquake have been felt at the foot of the mountain.

A VIOLENT shock of earthquake was felt at 7.30 on Saturday morning at Cosenza. Several houses were thrown down. One person was killed.

THE additions to the Zoological Society's Gardens during the past week include a Patas Monkey (*Cercopithecus patas* ♀) from West Africa, presented by Master Eric Blind; a Toque Monkey (*Macacus pileatus*) from Ceylon, presented by Mr. C. Brown; a Blue and Yellow Macaw (*Ara ararauna*) from South America, presented by Lieut. W. H. Duffin, King's Own Regiment; a Serval (*Felis serval* ♂), a White-tailed Ichneumon (*Herpestes albicauda*) from West Africa, presented by Mr. F. J. Jackson; a Canada Goose (*Bernicla canadensis*) from Canada, presented by Mr. J. E. Kelsall; a Rough-billed Pelican (*Pelecanus trachyrhynchus*) from Mexico, a Hutchins's Goose (*Bernicla hutchinsi*), from Arctic America, purchased.

## OUR ASTRONOMICAL COLUMN

LUNAR INEQUALITIES DUE TO THE ACTION OF JUPITER.—Some years ago Prof. Newcomb, discussing certain discordances between the observed and tabular places of the moon, was led to the conclusion that there existed a hitherto undetected inequality with a coefficient of 1″·5 in the longitude, and having a period of about 17 years as regards its effects on the eccentricity and longitude of the perigee. Shortly afterwards Mr. Neison announced that he had found in the action of Jupiter the explanation of this inequality. Using Delaunay's notation, his expression for the inequalities in longitude is—

$$\delta V = -1''{\cdot}163 \sin(2h + 2g + l - 2h'' - 2g'' - 2l'') + 2''{\cdot}200 \sin(2h + 2g - 2h'' - 2g'' - 2l'').$$

Now, the coefficient of the second of these inequalities is, theoretically, a quantity one order higher than that of the first; the first having the simple power of the eccentricity as factor, while the second has the square. Hence we should naturally expect to find the latter coefficient the smaller. On the grounds, therefore, that there is reason to think that Mr. Neison's value of this coefficient is possibly too large, Mr. G. W. Hill has investigated the lunar inequalities arising from the action of Jupiter, and has computed afresh the values of the coefficients of the resulting perturbations in longitude. His final result is—

$$\begin{aligned}\delta V = &- 0''{\cdot}903 \sin(2h + 2g + l - 2h'' - 2g'' - 2l'')\\ &+ 0'{\cdot}209 \sin(2h + 2g - 2h'' - 2g'' - 2l'')\\ &- 0''{\cdot}118 \sin(l - 2h' - 2g' - 2l' + 2h'' + 2g'' + 2l'').\end{aligned}$$

It will be seen that the coefficient of the second term is only about one-tenth of that found by Mr. Neison. It is not possible at present to determine the cause of this discordance, as Mr. Neison has not published the details of his investigation. It is to be hoped that he will now do so, in order to afford the means of deciding this interesting matter.

SPECTROSCOPIC DETERMINATION OF THE MOTION OF THE SOLAR SYSTEM IN SPACE.—The *Astronomische Nachrichten*, No. 2714, contains a brief note by Herr Hans Homann, giving the result of a discussion he has recently made of the spectroscopic observations of the motions of stars in the line of sight made at the Royal Observatory, Greenwich. He finds the position of the apex of the solar motion as derived from these to be R.A. 320°·1, Decl. 41°·2 N., and the speed of translation to be 39·3 ± 4·3 kilometres per second. He has likewise discussed the similar observations made by Dr. Huggins, and at the Temple Observatory, Rugby, by Mr. Seabroke, although these two latter series embraced too few stars, and these insufficiently observed to furnish adequate grounds for any satisfactory conclusions. The results derived from these three series, though differing very considerably from each other, yet show a certain rough correspondence which was perhaps all that could be expected, and Herr Homann considers that the velocity of translation may be taken as not greatly differing from 30 kilometres a second. The results from Dr. Huggins's and Mr. Seabroke's measures are as follows:—

| Observer | Velocity of Translation km. per sec. | Apex of Solar Motion |
|---|---|---|
| Huggins | 48·5 ± 23·1 | R.A. 309°·5, Decl. 69°·7 N. |
| Seabroke | 24·5 ± 15·8 | R.A. 278°·8, Decl. 13 6 N. |

These results all differ very considerably from those obtained by Struve, Airy, Galloway, and others, from a consideration of the proper motions of stars as observed with the telescope, *i.e.* in a direction at right angles to the line of sight, the most probable mean value of the co-ordinates of the apex from all these discussions being about R.A. 260°, Decl. 35° N., whilst Struve found the velocity of translation to be about 7 kilometres per second. This speed was, however, based upon the assumption that the average annual parallax of stars of the first magnitude is about 0″·25, and it should be borne in mind that Airy obtained (*Mem. R.A.S.*, vol. xxviii. p. 161) from the discussion of 113 stars with large proper motions a speed of translation nearly six times as large as that of Struve. Plummer also (*Mem. R.A.S.*, vol. xlvii. p. 341), from a rediscussion of Galloway's data, found for the co-ordinates of the apex R.A. 276° 8′, and Decl. 26° 31′ N., a result which differs considerably from the earlier ones above referred to, and in the direction of greater accordance with those obtained by the spectroscopic method. It may, however, be doubted whether the spectroscopic results are yet ripe for satisfactory discussion; the preliminary investigation undertaken by Plummer some time ago gave distinctly disappointing results, and, so recently as last May, Maunder (*Observatory*, vol. viii. p. 165) stated that but "some fifty stars in all had been observed a sufficient number of times for us to be able to deduce their speed to the nearest ten miles per second." He considered, however, that the results, so far as they went, "indicated a motion towards α Aquarii rather than towards any point in Hercules." This would agree well with Herr Homann's calculations in R.A., but not in Decl.

## *ASTRONOMICAL PHENOMENA FOR THE WEEK* 1886 *MARCH* 14-20

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

*At Greenwich on March* 14

Sun rises, 6h. 17m.; souths, 12h. 9m. 19·2s.; sets, 18h. 1m.; decl. on meridian, 2° 26′ S.: Sidereal Time at Sunset, 5h. 30m.

Moon (one day after First Quarter) rises, 11h. 20m.; souths, 19h. 14m.; sets, 3h. 6m.*; decl. on meridian, 18° 14′ N.

| Planet | Rises h. m. | Souths h. m. | Sets h. m. | Decl. on meridian ° ′ |
|---|---|---|---|---|
| Mercury | 6 39 | 13 6 | 19 33 | 4 33 N. |
| Venus | 4 40 | 10 3 | 15 26 | 7 57 S. |
| Mars | 16 35 | 23 32 | 6 29* | 10 27 N. |
| Jupiter | 18 37* | 0 44 | 6 51 | 0 37 N. |
| Saturn | 10 26 | 18 38 | 2 50* | 22 47 N. |

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

*Variable-Stars*

| Star | R.A. h. m. | Decl. ° ′ | | h. m. | |
|---|---|---|---|---|---|
| U Cephei | 0 52·2 | 81 16 N. | Mar. 18, | 19 55 | *m* |
| Algol | 3 0·8 | 40 31 N. | ,, 18, | 4 18 | *m* |
| ζ Geminorum | 6 57·4 | 20 44 N. | ,, 15, | 0 0 | *M* |
| | | | ,, 20, | 4 50 | *m* |
| S Cancri | 8 37·4 | 19 27 N. | ,, 17, | 1 10 | *m* |
| T Ursæ Majoris | 12 31·2 | 60 7 N. | ,, 14, | | *M* |
| δ Libræ | 14 54·9 | 8 4 S. | ,, 18, | 21 44 | *m* |
| U Coronæ | 15 13·6 | 32 4 N. | ,, 16, | 4 46 | *m* |
| W Scorpii | 16 5·1 | 19 50 S. | ,, 16, | | *M* |
| U Ophiuchi | 17 10·8 | 1 20 N. | ,, 14, | 23 15 | *m* |
| | | and at intervals of | 20 | 8 | |
| X Sagittarii | 17 40·4 | 27 47 S. | Mar. 17, | 0 0 | *m* |
| | | | ,, 19, | 21 40 | *M* |
| W Sagittarii | 17 57·8 | 29 35 S. | ,, 18, | 21 30 | *m* |
| β Lyræ | 18 45·9 | 33 14 N. | ,, 18, | 21 40 | $m_2$ |
| η Aquilæ | 19 46·7 | 0 7 N. | ,, 17, | 19 10 | *M* |
| T Cephei | 21 8·0 | 68 2 N. | ,, 14, | | *M* |
| δ Cephei | 22 24·9 | 57 50 N. | ,, 19, | 2 20 | *M* |

*M* signifies maximum; *m* minimum; $m_2$ secondary minimum.

*Occultations of Stars by the Moon* (visible at Greenwich)

| March | Star | Mag. | Disap. h. m. | Reap. h. m. | Corresponding angles from vertex to right for inverted image ° ° |
|---|---|---|---|---|---|
| 14 | B.A.C. 1930 | 6½ | 0 40 | 1 28 | 152 279 |
| 15 | 1 Cancri | 6 | 22 15 | 23 12 | 70 327 |
| 18 | 37 Sextantis | 6 | 17 25 | 18 17 | 11 240 |
| 20 | B.A.C. 4043 | 6½ | 1 38 | 2 34 | 119 239 |

| March | h. | |
|---|---|---|
| 15 | 6 | Mercury at least distance from the Sun. |
| 20 | ... | Sun in equator. |
| 20 | 8 | Jupiter in conjunction with and 0° 13′ north of the Moon. |

## *GEOGRAPHICAL EDUCATION AND NATURAL SCIENCE* [1]

ONE of my claims to address you on the subject of geographical education is that I have been a traveller. In my opinion nothing can better bring home to the mind the value of good geographical instruction, or make more keenly felt the disadvantages of the lack of it, than a scientific journey round the world. It is naturally the scientific side of geography which interests me most; and it is on the importance and prospects of physical geography as a subject of education that I have now to speak.

To the naturalist a knowledge of physical geography is becoming yearly more and more essential. The geographical distribution of plants and animals is one of the most important and fascinating of all the branches of his subject, presenting an immense field for research, full of problems of the utmost interest. Such problems can only be approached, with hope of success in elucidating them, with a clear comprehension of the principles of physical geography, and a power of entering into the utmost details whenever required. The distribution of organisms, and often their very forms and existence, are the result of the relative positions of the various climatic and other physical barriers on the earth's surface. On the land surfaces, where these barriers present most sharply-defined and serious obstacles to migration, the complexity of the distribution of the terrestrial fauna and flora is most remarkable. On the shores, where the barriers are less complete, the isolation and geographical restriction of the littoral fauna and flora is less developed. Whilst in the ocean, with no absolute land-barriers, the pelagic fauna shows little more than a distribution of animal and vegetable forms, according to climatic zones. In the depths of the ocean, which the effects of climate do not reach, the distribution of the animal inhabitants almost approaches universality.

It is, however, scarcely necessary to insist on the especial importance of the study of physical geography as one of the bases required for the scientific pursuit of zoology; and I am sure no one will be more ready than my friend Sir Joseph Hooker, to whom our knowledge of the geographical distribution of plants, and its meaning, is so largely due, to testify to its immense importance in the case of botany. It is obvious that it is equally indispensable in the cases of geology, astronomy, and meteorology.

Far more important is the question, Ought not physical geography to form part of every liberal education, as being a subject specially adapted for purposes of general learning, and as the only true basis on which can be founded a knowledge of what is termed political geography? Political geography may be regarded to some extent as the geographical distribution of mankind; and its various features of importance—its boundaries, its lines of migration and commerce, its cities and battle-fields—have their positions determined by the physical conditions and conformation of the earth's surface, as much as in the case of the distribution of the lower organisms.

In Germany and Austria, and many other parts of Europe, the necessity of physical geography as a subject of general education and of higher University study seems to be thoroughly accepted. There can be little doubt that it is an excellent subject of general education. I have become more and more convinced of this from my own experience as an examiner in the subject, and especially when examining for the Public School medals of this Society.

[1] Abstract of Lecture by Prof. H. N. Moseley, F.R.S., at the Royal Geographical Society's Exhibition of Geographical Appliances, Sir Joseph Hooker, K.C.S.I., V.P.R.S., in the chair.

Dr. Archibald Geikie, than whom no one can speak with higher authority, has expressed a most decided opinion on the peculiar value of physical geography in education. He writes, in a letter published in Mr. Keltie's excellent Report, that he knows of no other subject "that lends itself so effectively to the teacher who wishes to inspire his pupils with some appreciation of the nature and value of scientific education and reasoning." He has long been of opinion that, in this sense of the term, geography (that is physical geography), should form an essential part of education.

It seems generally conceded that the teaching of geography in this country is at present in a very unsatisfactory condition, and far behind that existing over a great part of the Continent of Europe. It is most remarkable, and much to be regretted, that in England, of all countries where advanced education prevails, with her world-wide possessions and interests, such a condition should exist. There can be no doubt about the matter. The fact that it is not found by English publishers to pay to issue first-rate maps and works on physical geography, equivalent to those published in Germany, is striking evidence of its correctness. The present movement is founded on a conviction that it is so. The reasons for this condition of things are probably not far to seek. When we find that geography, whether in lower or higher schools, or at military training colleges, is best taught and provided for in such countries as Germany and Austria, where the subject is represented by special professors and systematically taught at the Universities, whilst at no British University is there any professor of geography at all, it is surely not unreasonable to conclude that the lack of professors and higher teaching of the subject at our Universities is the main cause of the inferior position of the subject here.

The present energetic effort of the Society to promote geographical teaching cannot but yield considerable results in improving the position of the subject in this country, but it is most important that a further attempt to introduce the subject in any form, as a University one, should be made.

Possibly, although at the present moment it may not be feasible to secure the representation of geography as a whole, because of the apparent vagueness of its bounds and the attacks on all sides to which it is in consequence liable, there may be a chance of success if the attempt be made to press the claims of physical geography. It is, however, scarcely possible that the establishment of physical geography at the Universities can ever be effected without the cordial co-operation of the leading geologists of this country. I know that several of these, including Prof. Bonney, to whom I am indebted for much advice in the present matter, believe that the time has come when special chairs of physical geography should be established, regarding the question as one of, as it were, splitting the subject of geology into two parts. Prof. Archibald Geikie expresses himself as of a similar opinion in his letter published in Mr. Keltie's Report. That there is a necessity for lectures on the higher branches of physical geography is shown by the fact that courses of lectures nearly relating to this subject are now being given by Prof. Hughes at Cambridge and Prof. Boyd Dawkins at the Owens College, and I venture to suggest to the Council of the Society that it would be well to make an attempt to secure the co-operation of the Geological Society in a joint endeavour to induce the Universities to establish professorships of physical geography. There are many reasons why success may attend an effort to establish the representation of physical geography rather than the wider subject. It is obvious that any professor who could hold such a chair must be a geologist, the two subjects of physical geography and geology being most closely allied and overlapping. I am not going to attempt to define physical geography as a subject. The term geography is, no doubt, a somewhat unfortunate one, and a great deal of serious opposition has been raised to the advancement of the subject on such grounds as that it is a "graphy" and not a "logy." But the Germans have not only practically separated geology and geography as subjects of University study with the highest success, but find no difficulty in the use of the term physical geography to cover such knowledge as is represented in Peschel's excellent work, "Allgemeine Erdkunde."

Regarding physical geography as a part of geology to be separated from it:—The reason why such a separation should be effected is that there is thus formed and brought together for special treatment a subject which is far more necessary and suitable for general educational purposes than the whole of geology itself, which will attract far more students and act as a lever for promoting the study of other branches of science as special subjects, as well as of geology itself.

The principal argument that is always brought against the establishment of professorships of physical geography at the Universities, is that the subject is already covered by the professors of geology; but Prof. Geikie evidently does not take that view, and points out in his letter already referred to, "Geology is every day increasing in its scope, which is already too vast for the physical powers of even the most indefatigable teacher." It is already impossible for one teacher to cover all which may be supposed to be included under the name geology. When both physical geography and geology are represented by a single professor in a University they must needs be inadequately covered, or one branch must receive but meagre treatment in proportion to the other; or the period covered by a course of lectures is too long for any one student to attend the whole. Convinced that it is a matter of the utmost importance for the progress of geography here to show that the two subjects, geology and physical geography, can be taught with perfect harmony and advantage by different professors at the same time at a University, I asked Mr. Keltie to write to some of the German professors of geography, and request them to express their views on the matter, and to ask for copies of the syllabuses of courses of lectures on geography and geology delivered at the same time within their Universities. He has handed me the following most interesting letter from Prof. Kirchhoff, Professor of Geography at Halle:—

"Unfortunately, I cannot send you syllabuses of the University lectures in geology like those of my own lectures on general geography, and on the countries of Europe, which I placed in your hands, since such do not exist.

"However, the question now being raised in England is already practically settled in Germany. All the Universities in Northern Germany, and now those in Austria also, possess geographical as well as geological professors, and it is not found in any instance that the two interfere with each other, or are superfluous to each other, but, on the contrary, they have proved to afford mutual support.

"It is, no doubt, correct that geology, in just the same way as geography, is concerned with the earth and all its various parts. But the point of view on either side is different. For example, whilst I am delivering in Halle during four successive semestres the course on geography, with the nature of which you are acquainted, Prof. von Fritsch and two colleagues are lecturing to almost entirely different audiences on mineralogy, crystallography, geology, and palæontology. In summer, Prof. von Fritsch arranges excursions for geological purposes, and many of the students attending my lectures take part in these, because a problem of great geographical importance is able to be solved during these excursions, namely, the explanation of the form of the land surface as resulting from its composition, and by means of the history of its development.

"The two sciences do, indeed, touch one another in what is termed superficial geology, but from this zone of contact they stretch wide apart from one another. Geology discusses not only the developmental history of the earth in the Quaternary period, a matter which concerns the geographer quite as much as the geologist, but it discusses also that of the most remote periods of the earth's antiquity, investigates the petrographic structure and the organic life of every formation, subjects which hardly concern the geographer at all.

"On the other hand, geography has to deal not only with the land surface and the waters, but also with climate, the flora and fauna, and human inhabitants, both of the earth as a whole and of each separate country, confining its view to the present only, that is to say to the Quaternary period. It might as well be said that the existence of history as a subject at Universities rendered geography unnecessary, because it also has to do with the entire earth's surface.

"In reality, geography embraces all facts relating to the earth, borrowing them often from other sources. The geology of the British Islands, for example, together with their history since the time of Cæsar, does not by any means represent the geography of the islands."

Prof. Wagner, of Göttingen, Professor of Geography in that University, a most eminent authority on geographical education, has sent a note, in which he gives a syllabus of his own lectures and those of Prof. von Koenen, Professor of Geology.

From this it is evident, as Prof. Wagner concludes, "that there is no connection whatever (*gar kein Konnex*) between my lectures and those of the geologist."

Can any one doubt that the establishment of such a system of teaching geography and geology, side-by-side, as set forth in these two communications, would not be of the utmost benefit to our country and its education generally, if established in our Universities also? It will be impossible to obtain adequately trained teachers of physical geography until such courses of instruction are open; and until adequately trained teachers are produced for higher schools and training colleges, no real progress in the teaching of physical geography can be made throughout the country.

There can scarcely be a doubt that the establishment, at our Universities, of such a condition as that at the German ones, would be in every way to the advantage and advancement of geology, and to the increase of the numbers of its students; it would also advance the cause of all other branches of natural science, and all interested in the teaching of these subjects ought to support a movement in favour of its adoption warmly. No doubt the adoption of the system is merely a question of time,—England cannot lag behind in the study of geography for ever.

## UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—Mr. J. E. Marr, M.A., Fellow of St. John's College, has been appointed University Lecturer in Geology.

It is estimated that the ethnological collections now displayed in the Antiquarian Museum are worth at least 2000*l*., and with a little additional accommodation objects valued at 1000*l*. more can be displayed. These series are of inestimable value to the student of anthropology, and from the labours of Baron von Hügel in their arrangement the University is reaping a rich harvest. The baron contemplates illustrating them by a full series of maps and drawings.

The honorary degree of M.A. is to be conferred on Mr. C. Todd, Government Astronomer, Postmaster-General, and Director of Telegraphs in South Australia.

The Open Entrance Scholarships for Natural Science to be competed for in the ensuing months include those of Peterhouse, Chemistry and Physics, October; Clare, Natural Science, March 24; Downing, Natural Science, June 1; Non-Collegiate Students, Physical Science, July.

Mathematical Scholarships will be given at each College mentioned above (except Downing), and at Trinity Hall, March 17; Corpus Christi, March 23; Queens', April 27; St. Catherine's, May 11; Magdalene, March 17. Further information will be given by the Tutors of each College.

AT the City and Guilds of London Institute, Central Institution, Exhibition Road, S.W., Prof. Ayrton, F.R.S., will give a course of six lectures on some of the industrial applications of electricity, from 5 p.m. to 6 p.m. Friday afternoons, March 12, 19, 26, April 2, 9, and 16. The lecture on March 12 will be on Electric Lighting; March 19, Electricity as a Motive Power; March 26, Electric Storage of Energy; April 2, Electric Transmission of Power; April 9, Electric Meters; April 16, Electric Locomotion.

AT Clifton College a Scholarship of the value of 30*l*. per annum, tenable for three years at the Central Institution of the City and Guilds of London Institute for the Advancement of Technical Education, is offered by the Committee of the Institute, and will be awarded, on the nomination of the head-master, in July next. The candidate so nominated will be required to pass the Entrance Examination of the Institution, to be held in the following October. It is the intention of the Committee of the Institute to offer this Scholarship annually for six years, beginning with 1886. The object of the Central Institution is to provide advanced instruction in those kinds of knowledge which bear upon the different branches of industry, whether manufactures or arts.

It is intended that a subdivision of the Military and Engineering Department of Clifton College shall have its studies specially, though not exclusively, directed with a view to prepare for entrance to the Central Institution and similar Engineering and Technical Colleges.

## SOCIETIES AND ACADEMIES

### LONDON

**Royal Society**, February 4.—"On the Polarisation of Light by Reflection from the Surface of a Crystal of Iceland Spar." By Sir John Conroy, Bart., M.A. of Keble College, Oxford. Communicated by Prof. G. G. Stokes, P.R.S.

In the year 1819 Sir David Brewster communicated to the Royal Society (*Phil. Trans.*, 1819, p. 145) an account of some experiments he had made on the polarisation of light by reflection from the surface of double-refracting substances, and showed that Malus's statement with regard to Iceland spar was incorrect.

Malus said that Iceland spar behaves towards the light it reflects like a common transparent body, and that its polarising angle is about 56° 30′, and that, whatever be the angle comprehended between the plane of incidence and the principal section of the crystal, the ray reflected by the first surface is always polarised in the same manner ("Théorie de la Double Refraction," pp. 240, 241).

Some years later Seebeck made a number of very accurate observations on the same subject, and in 1835 and 1837 Neumann published an account of further experiments that he had made on the reflection of light by Iceland spar.

He begins his second paper by a brief summary of the results obtained by Brewster and Seebeck. "Brewster found that the angle of complete polarisation for calc-spar depends on the position of the reflecting surface relatively to the axis, and upon the position of its principal section to the plane of reflection; he also found that when the reflecting surface is covered with a liquid the plane of polarisation of the completely polarised ray does not coincide with the plane of reflection, but makes a smaller or greater angle with this; when a cleavage-face of calc-spar is covered with oil of cassia this deviation may amount to 90°. The knowledge of these phenomena has only been further advanced in recent times. Dr. Seebeck has so followed out, by means of most accurate determinations, the influence of optically uniaxial crystals upon complete polarisation that the angle of incidence at which this occurs can be determined as accurately beforehand as it can by Brewster's law in the case of uncrystallised bodies. Seebeck also discovered that the deviation of the plane of polarisation from the plane of reflection, which Brewster had observed, also occurs when the ray of light falls directly from air on to the surface of the crystal."

Seebeck's observations having been mainly directed to the determination of the angle of polarisation, Neumann's object was to determine the azimuth of the plane of polarisation of the reflected light.

Seebeck and Neumann only repeated a portion of Brewster's experiments, and no one except Sir David Brewster appears to have made any determinations of the angles and azimuths of polarisation when the spar was in contact with media other than air.

Prof. Stokes very kindly called my attention to these experiments of Sir David Brewster, and pointed out that, as they had never been published in detail, and had not been repeated by any one else, it was desirable that further observations should be made on this subject. The experiments, the results of which I have the honour of submitting to the Royal Society, were undertaken at Prof. Stokes's suggestion, and in carrying them out I had the benefit of his advice.

The apparatus used was essentially the same as that employed by Seebeck; the divided circle of the goniometer was, however, horizontal, and not vertical, as in Seebeck's instrument, and the arrangement for keeping the reflected ray constantly in the axis of the observing-tube, whilst the angle of incidence was varied, differed from that employed by him.

The measurements were made by altering the angle of incidence and the azimuth of the observing Nicol until the light reflected by the Iceland spar was reduced to a minimum, the position of the crystal remaining fixed.

In order to obtain anything like accurate results with observations of this kind it is necessary to make a large number of determinations and take their mean: it was obvious that there were two ways in which any given number of observations might be grouped, either by making a good many separate determinations for a few positions of the crystal, or by making a few observations at a number of different azimuths; the latter alternative being the one adopted, two readings were made at seventy-two different azimuths of the crystal.

Two complete series of observations were made with cleavagefaces of Iceland spar in air, water, and tetrachloride of carbon, the water and tetrachloride of carbon being contained by a nearly cylindrical thin glass vessel (a chemical beaker), which stood on the horizontal stage of the goniometer, the tetrachloride being prevented from evaporating by a layer of water floating on its surface.

The position of the crystal in which the principal section was in the plane of incidence and the obtuse summit nearest the observer was considered the zero position; when the principal section was in the plane of incidence, and the obtuse summit towards the side from which the light was incident upon it, was therefore azimuth 180°. The crystal was rotated clockwise, and the same direction of rotation was considered the positive direction for the Nicol.

It had been intended to make similar measurements with artificial surfaces cut perpendicular and parallel to the axis of the crystal, and three pieces of Iceland spar cut respectively parallel to a natural face, and perpendicular and parallel to the axis, and all polished with "whiting" were obtained.

Seebeck states (*Pogg. Ann.*, vol. xxi. 290) that Iceland spar polished with rouge or putty powder differs in its optical properties from the natural substance, but that an artificial surface polished with chalk behaves very nearly, if not exactly, like a natural one.

Seebeck's measurements were all made with the crystal in air, and as the changes in the azimuth of the plane of polarisation, and in the value of the polarising angle, for different azimuths of the crystal, when such is the case, are small, it seemed desirable, before making any measurements with the artificial surfaces cut perpendicular and parallel to the axis, to make some determinations with an artificial surface parallel to a natural face of the crystal when the crystal was immersed in water; this was accordingly done.

These results differed considerably from those obtained previously with a natural face in water, and it therefore did not appear worth while to make any further experiments with artificial surfaces, as it seemed certain that the results would be untrustworthy.

The difference between the results obtained with this artificial surface and with a natural surface of the crystal is too great to be explained by supposing that the artificial surface was not cut absolutely parallel to the direction of the cleavage, and must therefore be attributed to some change produced by the polishing, possibly due to the pressure employed (conf. Seebeck, *Pogg. Ann.* vol. xx., 1830, 27).

Prof. Stokes pointed out to me that the experimental results which had been obtained were well suited for reduction by means of the harmonic analysis, and not only explained the method but himself reduced the first set of observations made with a cleavage-face in water. All the observations were accordingly reduced by this method.

Owing to the fact that the principal section of the crystal is a plane of symmetry, the periodic series for the development of the azimuths of the planes of polarisation can contain sines only, and that for the polarising angles cosines only, including the constant term; therefore the coefficients of the cosines in the former case, and of the sines in the latter, were not calculated, except with the observations made with the artificial surface; it seemed possible that the process of polishing might occasion some want of symmetry, and that therefore it was desirable to calculate the values of the coefficients in both sines and cosines.

Omitting the terms which we know from theoretical reasons ought not to appear, and which at any rate are extremely small, we obtain as the final result the following approximate expressions—

*Azimuths of the Plane of Polarisation of Light Polarised by Reflection*

| | |
|---|---|
| Cleavage surface in air | $-\ 2^\circ\ 10' \sin\theta + 1^\circ\ 49' \sin 2\theta + 0^\circ\ 2' \sin 3\theta + 0^\circ\ 1' \sin 4\theta.$ |
| Ditto, in water ... ... | $-\ 9^\circ\ 27' \sin\theta + 5^\circ\ 29' \sin 2\theta + 0^\circ\ 47' \sin 3\theta - 0^\circ\ 10' \sin 4\theta.$ |
| Ditto, in tetrachloride of carbon ... ... ... | $-23^\circ\ 47' \sin\theta + 10^\circ\ 25' \sin 2\theta + 4^\circ\ 17' \sin 3\theta - 0^\circ\ 24' \sin 4\theta.$ |
| Artificial surface in water | $-\ 3^\circ\ 52' \sin\theta + 5^\circ\ 11' \sin 2\theta + 0^\circ\ 33' \sin 3\theta - 0^\circ\ 21' \sin 4\theta.$ |

*Polarising Angles*

| | |
|---|---|
| Cleavage surface in air | $58^\circ\ 17' - 1^\circ\ 15' \cos 2\theta + 0^\circ\ 2' \cos 4\theta.$ |
| Ditto, in water ... ... | $52^\circ\ 2' - 3^\circ\ 14' \cos 2\theta + 0^\circ\ 13' \cos 4\theta.$ |
| Ditto, in tetrachloride of carbon ... ... ... | $53^\circ\ 9' - 8^\circ\ 54' \cos 2\theta + 1^\circ\ 12' \cos 4\theta.$ |
| Artificial surface in water | $48^\circ\ 53' - 2^\circ\ 9' \cos 2\theta + 0^\circ\ 1' \cos 4\theta.$ |

From these expressions the values of the ordinates of the curves representing the phenomena were calculated, and the curves plotted from the values so obtained.

These curves correspond very closely with the smooth curves drawn from the points given by the observations, the values of the ordinates for those portions of the curve corresponding to azimuths 0°–40° and 320°–360°, being rather greater than the values given by the smooth eye-drawn curve. The curves for the artificial surface in water show clearly, when compared with the corresponding curves for the natural surface, how greatly these two surfaces differed in their optical behaviour.

Brewster, in his paper in the *Philosophical Transactions* for 1819, says:—"In any given surface when $A$ and $A''$ are the maximum and minimum polarising angles, viz. in the azimuths of 0° and 90°, the polarising angle $A'$ at any intermediate azimuth $\alpha$ may be found by the formula $A' = A + \sin^2\alpha(A'' - A)$."

This expression is the same as that given by the harmonic reduction of the observations set forth in this paper, if we assume that the smaller terms are due to errors of observation, as in that case the expression for the polarising angle in air ($B$) becomes $58^\circ\ 17' - 1^\circ\ 15' \cos 2\theta$.

Brewster's formula also appears to hold good for the case of Iceland spar in water, as the harmonic series for the value of the polarising angle ($D$) may be taken as $52^\circ\ 02' - 3^\circ\ 14' \cos 2\theta$. But with the spar in tetrachloride of carbon the agreement no longer holds, as the coefficient of $\cos 4\theta$ becomes too large to be neglected, being 1° 12′. The determinations made in this strongly refracting liquid were less satisfactory than the others, but there is hardly sufficient ground for assuming that the value of the coefficient of $\cos 4\theta$ is merely due to errors of observation.

The experiments, of which an account had been given, confirm the accuracy of Brewster's observations made with a surface of Iceland spar in contact with media other than air, and show moreover that, as Seebeck pointed out, the change in the value of the azimuth of the plane of polarisation of the reflected light also occurs, though to a far less extent, when the crystal is in air, and further, as the refractive index of the medium increases, the change in both these values is greatly augmented.

The harmonic analysis affords a means of expressing approximately at least both these changes as functions of the azimuth of the principal section of the crystal, and further shows that, when the crystal is in air or water, Brewster's formula for the angle of polarisation expresses the facts of the case.

**Linnean Society**, February 18.—Dr. St. George Mivart, F.R.S., in the chair.—Prof. H. Macaulay Posnett, N.Z., was elected a Fellow of the Society.—Mr. W. Joshua exhibited over 130 species of Lichens from Jamaica, collected by Mr. J. Hart in the Blue Mountains near Gordon Town, and afterwards determined by Dr. J. Müller (Arg.) of Geneva; many of these were of great interest.—Mr. T. Christy exhibited some flowers preserved by a new chemical process; he also called attention to a hitherto unknown Cinchona bark from South Africa; and besides showed a living plant of *Erythoxylon coca* in fruit.—Mr. H. Goss made remarks on specimens of the Wild Parsnip (*Pastinaca sativa*) gathered by him on the Thames side, Moulsey, Surrey.—Mr. A. D. Michael read a paper on Acari of the genus *Glyciphagus*, discovered in moles'-nests. In *G. platygaster* the male, although slightly differing from the female, as is usual in the genus, still can easily be recognised as of the same species; but in *G. dispar*, while the female closely resembles that of *G. platygaster*, the male, on the contrary, is totally unlike in size, form, markings of body, and arrangement of the legs, &c. *G. dispar* also affords evidence of the retro-anal position of the bursa copulatrix, and its being the posterior median projection characteristic of the females of the genus. Mr. Michael speculates wherefore the above divergence of the male form of *G. dispar*, seeing that its habitat and other conditions are the same as its female, and the closely-allied species. — Mr. John Ball gave a communication on the botany of Western South America. He introduced the subject with reflections on the climatal relations of the western seaboard, which have such a remarkable influence on the development of vegetable life. He then describes his collection of plants from Buena Ventura in Columbia, from Payta in Northern Peru, from Caldera in Northern Chili, and Lota in Chili, from the neighbourhood of the Channels of Western Patagonia, and Straits of Magellan, throughout interspersing reflections and brief summaries of the peculiarities of the floras in each of the districts in question. He infers that the vast region including the warm and moist parts of South and Central America should be regarded as a single botanical province, in which the same generic types are represented by species of which a large proportion are endemic, and confined to comparatively small areas. Along with these we find, in various parts of the same region, a few forms so distinct as to be ranked as separate genera, mostly represented by one, or very few, species, and nearly allied to types of wide distribution. He assumes that, in a broad sense, the most natural divisions of the vegetation of the earth are wide areas of low country, over which, with more or less modification, a limited number of types have extended, with islands of high land, which are the original homes of special types that form the characteristic features of different regions.

**Zoological Society**, March 2.—Dr. St. George Mivart, F.R.S., Vice-President, in the chair.—Mr. J. G. Millais, F.Z.S., exhibited an adult specimen of the Ivory Gull, shot by himself near Thurso, in December 1885; also a young example of the same species, obtained in Scotland in 1879.—Mr. T. D. A. Cockerell exhibited a living Slug of the genus *Parmacella*, obtained at Tangier, and probably referable to *P. valenciennesi*.—A communication was read from Prof. R. Collett, C.M.Z.S., containing an account of a new Pediculate fish from the sea off Madeira, belonging to the family Ceratiidæ, which the author proposed to call *Linophryne lucifer*.—Mr. P. L. Sclater read a note on the external characters of the head of *Rhinoceros simus* as compared with those of *R. bicornis*.—Mr. F. E. Beddard read a note on the air-sacs of the Cassowary.—A second paper by Mr. Beddard treated of the syrinx and some other points in the anatomy of certain forms of Caprimulgidæ.

**Entomological Society**, March 3.—Mr. Robert McLachlan, F.R.S., President, in the chair.—Mr. J. M. C. Johnston was elected a Fellow, and Cavaliere Piero Bargagli, of Florence, was elected a Foreign Member.—Mr. Pascoe exhibited a curious larva, probably of a *Papilio*, from Pará; and also a pupa-case of *Anosia plexippus* (*Danais archippus*), from the same locality.—Mr. W. J. Williams exhibited, on behalf of Mr. C. Bartlett, a gigantic hairy and spiny larva, perhaps allied to *Gastropacha*, from Madagascar.—Mr. C. O. Waterhouse exhibited *Rutela rufipennis*, *Doryphora haroldi*, and some other (undescribed) species of *Coleoptera*, from Colombia.—Mr. Billups exhibited a specimen of *Cholus forbesei*, found alive in a horticultural sale-room in London.—Mr. Eland-Shaw referred to the exhibition at the last meeting of *Tettix australis* from New South Wales, and called attention to the fact that the aquatic habits of certain species of the genus *Tettix* in India had been previously recorded.—Dr. Fritz Müller communicated a paper on fig insects from Itajahy, South America; and Prof. Meldola exhibited, on behalf of Dr. Fritz Müller, a number of specimens of the insects described in the paper.—Mr. E. B. Poulton read further notes upon lepidopterous larvæ and pupæ, including an account of the loss of weight in the freshly formed pupa.

DUBLIN

**University Experimental Science Association**, January 27.—Prof. Cathcart in the chair.—Prof. Fitzgerald showed his new galvanometer. This instrument was constructed, and exhibited in the Inventions Exhibition last year, by the Cambridge Scientific Instrument Company. Its peculiarities are (1) the arrangement by which the coils can be measured in their place, which is an advantage when practical classes are working, and should measure their instruments; (2) the arrangement by which the circle is read with a microscope by reflection-mirrors attached to the magnet, when the instrument is used either as a sine or tangent galvanometer; (3) an arrangement by which a spot of light reads the tangents of deflection. The first advantage is secured by having the two pairs of short and long coils wound in grooves closed in, outside, by a glass plate through which they can be seen, and the external and internal diameter of each layer of wire measured; the transverse diameter, by seeing through small holes left in the ring that covers the coils outside. The reading is effected by viewing a scale engraved on the inside of a horizontal ring surrounding the needle by reflection in two right-angled prisms attached to the needle which reflect opposite sides of the scale. The corresponding lines in the two maps, which differ by exactly 180°, is the line at right angles to the line of intersection of the reflecting planes of the prism. The exact position of that line can be read by means of a micrometer in the eye-piece of the microscope. The horizontal graduated ring is attached through the vertical axis on which the coils, &c., turn to the base of the instrument, and so the same circle does for reading when the instrument is used as a sine galvanometer. By means of a small mirror attached to the needle at 45° to the line of suspension, a spot of light can be reflected through the glass side of the instrument to a scale, and then a uniform scale represents the tangents of the deflections.—Mr. J. Joly, B.E., gave an account of a method of finding the specific gravity of small heavy bodies. The substance, whose specific gravity is required, which may only be a few milligrammes in weight, is melted into a small dish of paraffin of known specific gravity. The paraffin and substance is then floated in a specific gravity solution, and from the formula

$$S = \frac{W}{\frac{w_1}{s_1} - \frac{w_2}{s_2}}$$

the required specific gravity of the body can be obtained. In the above equation $W$ is the weight of the solid, $w_2$ that of the paraffin, $w_1$ the sum of these weights; $s_2$ is the specific gravity of paraffin, $s_1$ the specific gravity of paraffin and substance together. This method is extremely useful in dealing with porous bodies, owing to the capability of paraffin, when in a molten state, of entering the pores and expelling air. Mr. Joly gave details of a number of experiments which show excellent results.—The next paper was read by Mr. Gerald Stoney, on the dynamics of bicycling. He described experiments made by him, in conjunction with his father, Dr. G. Johnstone Stoney, F.R.S., by which the energy required to propel a bicycle was obtained. They found that it required, when the velocity was 9 miles per hour, about 5500 foot-pounds per minute, and that it often rose higher than 10,000 foot-pounds per minute, which was the highest the apparatus used was capable of recording. Their results were higher than those of other experimenters on the power a man can exert. This shows that the bicycle or tricycle is probably the most economical way of using human muscles. The experiments were made by attaching an indicator-diagram-apparatus to the lever of the safety-bicycle, known as the "Extraordinary," and also by observing the reduction in speed due to friction, when the bicycle was running free. The experiments also showed that the resistance varied almost as the velocity, and that the pressure on the pedal was not constant, but was at a maximum at the centre of the stroke.

PARIS

**Academy of Sciences**, March 1.—M. Jurien de la Gravière, President, in the chair.—Results of the application of the new method for preventing rabies after the bite of a mad dog, by M. Louis Pasteur. Since October 26, 1885, when his process was first announced to the Academy, 350 patients of all ages and both sexes have been treated with perfect success in every case except one. The eminent biologist considers his prophylactic method established, and expresses a hope that a hospital may now be founded for the regular treatment of patients by this process of inoculation. This suggestion met with general approval, and a Commission was appointed to give it effect, including the names of MM. Vulpian, Marey, P. Bert, Jurien de la Gravière, Bertrand, and De Freycinet.—Direct formulas for calculating the momenta of flexion in continuous girders of constant or variable section, by M. Maurice Lévy.—Note on the comparative results of direct astronomical observation with those obtained by MM. Henry's photographic process, by M. Wolf. Discrepancies are pointed out between the photographs of the Pleiades and the author's observations of that constellation in 1874. He adds: "The chart of the heavens now obtained by photography is different from that drawn from direct observation, and it also differs from that which will be obtained twenty years hence by the photography of the future, whose processes will certainly be different from ours. The human eye, on the contrary, is an organ which is always the same; consequently its observations are always capable of being compared together. . . . Celestial photography must work hand in hand with the observer's eye, which it can never replace."—Reply to M. Lalanne's note of February 22, on the mechanical effects of tornadoes, by M. Faye. M. Lalanne's facts are not questioned, but they are shown to be perfectly in accordance with M. Faye's well-known theory.—Remarks on the various theories of tornadoes, by M. Lecoq de Boisbaudran. While admitting the descending movement as the general law, the author suggests that a secondary movement in the opposite direction may perhaps occasionally be produced, which would serve to explain many phenomena difficult to account for on any one theory.—On the equivalent of the terbenes; explanatory note, by M. Lecoq de Boisbaudran.—On the employment of the azimuthal co-ordinates in geodetic surveys, by M. Hatt.—Communication on the approaching centenary of Arago, by M. Mouchez. It was announced, on behalf of the Committee, that the intended banquet in the Hôtel de Ville has been abandoned, and that it has been decided to erect a more lasting monument to the memory of the illustrious astronomer, to take the form of a colossal statue to be raised by national subscription on the Boulevard bearing his name.—Remarks on the *Year-book* of the Imperial Observatory of Rio de Janeiro, presented to the Academy on behalf of the Emperor of Brazil, by M. Faye.—Position of telescopic stars in the constellation of the Pleiades, by M. G. Rayet. A complete list is given of 143 stars observed with the 14-inch equatorial of the Bordeaux Observatory during the winters of the years 1884–85 and 1885–86.—Observations on Fabry's comet made at the Observatory of Algiers with the 0·50 m. telescope, by M. Ch. Trépied.—Orbit and ephemeris of the same comet, by M. Lebeuf. From the observations taken at Algiers, Hamburg, Nice, and Paris, the elements of the new orbit have been determined as under:—

$$T = 1886 \text{ April } 5{\cdot}9580 \text{ Paris Mean Time}$$

$$\left.\begin{aligned} \omega &= 126^\circ\ 36'\ 6''{\cdot}7 \\ \Omega &= 36\ 22\ 32{\cdot}0 \\ i &= 82\ 36\ 34{\cdot}9 \end{aligned}\right\} \text{Equinox } 1886{\cdot}0.$$

$$\log q = 9{\cdot}807626$$

—On the angle of the line of depression below the horizon at sea, by M. E. Perrin. The observations of depression here published were taken in 1884–85 on board the *Galissonnière* in the Chinese seas by means of a Lorieux reflection circle furnished with Daussy's additional small mirror. The mean value of apparent depression was determined at 5′ 31″·5. The corresponding geometric depression being 5′ 46″·8 for an altitude of 9 metres, about 1/23 was fixed for the coefficient of geodetic refraction at sea.—Calculation of mechanical regulators; the proper course to follow in practice in order to establish a regulating apparatus with indirect action, by M. H. Léauté.—Note on the articulated hyperboloid and the application of its properties to the demonstration of De Sparre's theorem, by M. A. Mannheim.—On Deprez d'Arsonval's aperiodic galvanometer employed as a ballistic galvanometer, by M. Ledeboer.—On the spectrum of erbine, by Prof. W. Crookes. The phosphorescent spectrum of this earth, of which a comparatively pure specimen has recently been obtained by the author, showed four green bands coinciding with none of those of the spectra of yttrium and samarium.—On the crystallisation of the paratartrate of soda and ammonia, by M. J. Joubert.—On the relations existing between the variations of terrestrial magnetism and the protuberances and other phenomena observed on the sun, by M. H. Wild. As far as the question has hitherto been studied the author considers it well-nigh established that the great movements of the solar atmosphere are revealed on the globe by corresponding disturbances of the magnetic needle.—Actinometric observations made at Montpellier during the year 1885, by M. A. Crova.—On the hygroscopic properties of tobacco, by M. Th. Schlœsing, jun.—On the isomeric states of the sesquichloride of chromium, green sesquichloride, by M. A. Recoura.—On some immediate principles of the peel of the bitter orange, by M. Tanret.—On the respiratory centres of the spinal marrow, by M. E. Wertheimer. Numerous experiments made on dogs show that in the spine there exist nervous centres, some determining inspiration, others expiration.—On the character of an anomalous rock in the Aspe Valley, Lower Pyrenees, by MM. E. Jacquot and A. Michel Lévy. This rock, by Charpentier called *compact feldspar*, is interstratified at the base of the Carboniferous formations, its age coinciding with the end of the granulite and beginning of the microgranulite eruptions. Although soft and oily to the touch, like the steatites, its dust scratches glass. Chief constituents: silica, 76·33 per cent.; alumina, 14·30; potassa, 3·33; lime, 0·90.—On the stratigraphic relations existing between the miliolite limestones and the *Micraster tercensis* formation in the department of the Haute-Garonne and the canton of Sainte-Croix (Ariège), by M. J. Roussel. The new facts determined by the author show that in the Pyrenees the relations of the Chalk and Tertiary formations are sometimes of an extremely complicated character. But in his remarks on this paper M. Hébert was unable to accept the view that the *Micraster tercensis* of the Pyrenees, essentially a Cretaceous rock, was contemporary with the Tertiary formations containing *Cerithium ladevezi*, *Ostrea uncifera*, and similar fossils.

STOCKHOLM

**Royal Academy of Sciences**, February 10.—On Binuclearia, a new genus of Confervacea, by Prof. V. B. Wittrock.—On the biology of some Arctic plants, by Prof. E. Warming.—Contributions to the anatomy of the cotyledons of the monocotyledonous plants, by Miss M. Lewin.—On the amount of the rainfall on bare and wooded ridges in the North of Halland, by Dr. H. Hamberg.—Insects collected in the Cameroon Mountain, by G. Waldau and H. Knutson: I. Coleoptera, Cetoniidæ, described by Prof. Chr. Aurivilius.

## BOOKS AND PAMPHLETS RECEIVED

"Across the Jordan," by G. Schumacher (Bentley).—"Marvels of Animal Life," by C. F. Holder (Low).—"Japanese Homes," by E. S. Morse (Low)—"Highlands of Cantabria," by Ross and Cooper (Low).—"The Rain-Band," by J. R. Capron (Stanford).—"Lessons in Elementary Chemistry," new edition, by Sir H. E. Roscoe (Macmillan).—"Rotifera," part 2, by Hudson and Gosse (Longmans).—"Bees and Bee-keeping," part 7, by F. R Cheshire (Gill).—"The Western Pacific and New Guinea," by H. H. Romilly (Murray).—"British Petrography," part 2, by J. J. H. Teall (Watson, Birmingham).—"Indian Meteorological Memoirs," vol. ii. part 5 (Calcutta).—"Report on the Administration of the Meteorological Department of the Government of India in 1884-85."—"The Monthly Weather Report," Oct. and Nov., 1885.—"Proceedings of the Linnean Society of New South Wales," vol. x. part 3 (Cunninghame, Sydney).—PAMPHLETS:—"La Sensibilité et la Motilité des Végétaux," by E. Morren (Hayez, Bruxelles).—"Une Expérience sur l'Ascension de la Sexe chez les Plantes," by L. Errera.—"Fremdländische Zierfische, mit Abbildungen," by B. Dürigen (P. Matte, Berlin).—"The Fixed Idea of Astronomical Theory," by A. Tischner (Fock, Leipzig).—"Report on the Action of the Sheffield Water on the Lead Communication Pipes," by S. White.

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