THURSDAY, JANUARY 12, 1882

CLERK MAXWELL'S "ELECTRICITY AND MAGNETISM"

A Treatise on Electricity and Magnetism. By James Clerk Maxwell. Second Edition. (Oxford: At the Clarendon Press, 1881.)

An Elementary Treatise on Electricity. By James Clerk Maxwell. Edited by William Garnett, M.A. (Oxford: At the Clarendon Press, 1881.)

THESE volumes have a melancholy interest for the student of electrical science, inasmuch as they are the unfinished work of one of its great masters. The printing of the second edition of the larger work had reached the second half of the first volume when it was interrupted by the premature death of the author. Up to this point considerable modifications have been introduced into the work; but the rest is merely a reprint under the superintendence of Mr. W. D. Niven, of Trinity College, Cambridge.

We shall allude by and by to these alterations in the earlier part of the new edition; but it may not be without interest to the readers of NATURE briefly to review the progress of electrical science during the eight years that have elapsed since the publication of the first edition, and to trace the influence of the work therein.

Clerk Maxwell appeared avowedly as the mathematical expositor of Faraday; in this he was the pupil and follower of Sir William Thomson. The electrical papers of the last, reprinted the year before Maxwell's work appeared, reach some of them as far back as 1842; and in the earliest of them he occupies himself with the translation of Faraday's ideas into the ordinary language of Mathematical Physics. He shows that his translation leads to a theory in no wise contradictory of the received theory of action at a distance, so far at least as this theory merely exhibits the facts of observation; but to a theory in some respects more comprehensive, inasmuch as certain experimental facts find a much more natural explanation in it than in the older and more prevalent one. It may be asserted, without fear of contradiction, that Sir William Thomson was the first who thoroughly understood and clearly expounded Faraday. Abroad his methods and conceptions were decried as vague and, although suggestive and worthy of notice, as the machinery that had been used by a man of genius, yet devoid of accurate foundation.[1] At home the loose way in which Faraday's methods and terms had been used, or more properly speaking, abused by many, gave but too specious a confirmation of the justice of this criticism.

What Thomson began Maxwell continued, and in a sense completed. In his work we have the first systematic exposition of Faraday's theory of a dielectric medium applied to all the main phenomena of electricity.

He shows, for the first time, that all the ordinary phenomena of electrical action and reaction can be explained by the thoroughly legitimate physical hypothesis of *stresses* in the medium of having a static or a kinetic origin, as the case may be.

Had this innovation been a mere matter of the transformation of surface into volume integrals (as may still seem perhaps to some who are mathematicians by birth, and physicists by application only), it would not be necessary to dwell on it in these pages. But it was far more. To the old theory of action at a distance, in so far as it assigned the mathematical laws of certain of the observed facts, there could be no sort of objection. Its success in this respect is scarcely rivalled by that of the corresponding artifice in celestial dynamics.

But the theory of action at a distance was not, and never could be, a dynamical explanation of electrical phenomena. It may be true that we know little of what occurs in the medium between electrified conductors; but we are not likely to extend our knowledge by adopting a theory which begins by directing us to divert our attention altogether from the very field in which a dynamical explanation *must* be sought. This strong argument against it is supported by the still stronger, that, notwithstanding the admirable accuracy with which the old theory explains a large body of facts, there are still other facts, continually increasing in number, of which it gives no explanation whatever, if indeed they do not flatly contradict it.

The medium theory was therefore a first and necessary step towards a dynamical theory of electricity. It begins by divesting the facts of all hypothetical raiment, and expressing them in language appropriate to themselves, suggesting nothing but what Nature has indicated, indicating nothing that Nature has denied, supposing as little as may be where nothing has been revealed. Above all banishing from the catalogue of physical conceptions the imponderable electrical fluids that have worked such mischief in indolent minds, and poisoned electrical literature so long.

Moreover the old theory, although it was a weapon of wonderful power in the hands of expert mathematicians, was difficult of translation into even technical verbal expression. It was inflexible and unwieldy when applied in general explanation, and in the universally occurring cases where approximate estimation is all that is necessary or desirable. In one sense therefore Maxwell's treatise was a popularisation of the theory of electricity. By this we do not mean that Faraday's method of lines of force brings the subject to the level of a mind untrained in the handling of accurate ideas, but simply that it often renders the profound and laboriously acquired skill of the mathematical expert unnecessary. In the highest sense, viz. that of an accurate thinker, Faraday himself was, as has been often said, a great mathematician; although he was so little of a mathematical expert, that he once expresses his obligation to one who had calculated the tangents of some galvanometer deflections for him. And yet complaints have often been made of the obscurity of Clerk Maxwell's work. Certainly it is not easy reading; but the difficulties are always to be found where Nature has herself set them; they arise simply because the author refuses to put a bridge over the gap presented by experience. Such complaints come alike from the mathematician in search of an ideal logical completeness, and ever impatient of *facts* that do not fit with his preconception; and from

[1] If the reader wishes to see how much Maxwell's work has done to popularise Faraday's ideas abroad let him consult the electrical papers that have appeared of recent years in *Wiedemann's Annalen*, or read the emphatic testimony of Helmholtz, given at the commencement of the Faraday lecture. See Jour. Chem. Soc., June, 1881.

those whose powers of generalisation do not carry them far enough to put them above regarding the small details of experiment as the ultimate end of science. For both classes there are lessons in Maxwell's pages.

Another most important feature of the work under review is that it is in the strictest sense a Treatise on Electrical *Measurement.* It looks at electrical actions almost exclusively as measurable; it does not profess to be a complete experimental treatise of all electrical phenomena qualitatively or quantitatively observed. In this respect it is a continuation of the labours of its author in conjunction with the rest of the distinguished band of electricians who formed the Committee of the British Association on Electrical Measurements. Maxwell's work is in many parts, particularly in the second volume, a development of the methods employed by this Committee. Many of the electrical measurements there described had, when the volumes were first published, been actually carried out only by the author himself or by very few beside; but now the great majority of them have become the commonplaces of a physical laboratory.

The effect on practice of the work thus consummated in Maxwell's Treatise has been, both directly and indirectly, enormous. The extension of technical applications has been immensely facilitated by the introduction of definite units. Instead of the old vague, unscientific, and still more, unbusinesslike statements of quantity and intensity, we have the precise ideas of electromotive force, resistance, current, and so on, measured in their respective units, the volt, the ohm, the ampère, &c.; and now electrical commodities can be bought and sold by rule and measure, as heretofore cloth, coals, or horse-power. And yet we have noticed a tendency now and then in technical journals, on the part of men of practice, evidently ignorant of the history of the science they apply, to depreciate unduly the services of their theoretical brethren. One would have thought that in electrical science, beyond all others, where the mutual obligation is so great and so equally balanced, the folly of either the man of theory or the man of practice attempting to minimise the services of his fellow worker would have been evident.

The seal has been set to the work of the B.A. Committee by the Congress of Electricians which met last autumn in Paris, by the adoption of the B.A. units as the basis of an international system. To this result Maxwell's treatise has powerfully contributed, but it would be little in the spirit of its author to boast of this as a national, much less as his personal triumph; it is more fitting to remind the readers of NATURE that in the work thus consummated the English electricians were the followers of Gauss and Weber, and, more remotely, the disciples of Coulomb, Poisson and Ampère, so that they have simply acted up to the motto of all true scientific men, λαμπάδια ἔχοντες διαδώσουσιν ἀλλήλοις; they have but passed the torch from hand to hand.

The electromagnetic theory of light formed a fitting crown to the first edition of Maxwell's Electricity. It was left by its author in the form of a general sketch, carried just so far as was necessary for comparison with experiment. Concerning the progress of this theory during the last eight years much might be said, and it is greatly to be regretted that we have not before us what Maxwell himself would undoubtedly have said, had he lived to superintend the publication of the second volume of his work. It has formed the basis, as every good physical theory should, for a large number of further researches, both theoretical and experimental. We need only mention the work of Helmholtz, Boltzmann, Rayleigh, Silow, Hopkinson, Fitzgerald, Glazebrook, J. J. Thomson, and many others. The theory has not proved, and its author certainly never expected it to prove, a framework ready made with appropriated pigeon-holes, into which would naturally fall every electrical fact to be discovered in all time coming; but it has proved itself, so far, the best theory with anything like a physical basis that has been proposed to explain the facts with which it deals. The more it has been worked out, the more it has been found to explain in a natural way the known phenomena of electricity and light; and it does not appear to have been shown as yet that there is any observed fact that may not ultimately be reconciled with it, either by farther development of the theory, or by deeper probing of the experimental results. This is really all that could be expected when we reflect that, much as we know about electricity, there is an infinity yet unknown.

We have now to allude briefly to the changes that have been made in the second edition.

In the introductory chapter we are glad to see that few changes have been made; we need, therefore, only recommend our readers to peruse it again, as perhaps the most admirable thing of the kind that has been written in any language; we direct their attention more particularly to the distinctions drawn between electricity, force, and energy, of which some of our scientific men seem strangely oblivious, and to the admirable remarks on the two fluid theory. We must at the same time warn the student as to a radical change that has been introduced into the terminology of the subject. He is aware that at every point of the electric field there is conceived a directed quantity, which in the former edition of this work was called resultant electric force, or the electromotive force *at the point*, according as it was regarded from the ponderomotive or electromotive point of view, and he is also aware that the electromotive force *at a point* was a very different thing from the electromotive force *between two points*, the latter being in point of fact of different dimensions. It was always difficult, even for those who clearly understood the distinction, to avoid occasionally using the one term where the other was appropriate. Most probably from a feeling of this difficulty, our author has substituted for the two first of these terms *resultant electric intensity and electromotive intensity* respectively, wisely leaving the old established terms *electromotive force between two points* with its original meaning, although in point of fact it involves an abuse of the word *Force*. We could have wished the danger of confusion still more effectively barred by dropping the word "electromotive" in the first case altogether; but a more serious objection to this change is, that the author evidently intended, from his foot-note on p. 72—

"The electric and magnetic intensity correspond in electricity and magnetism to the intensity of gravity, commonly called g in the theory of heavy bodies,"

to have made a corresponding change of terminology in the case of magnetism; whereas on turning to the second volume we find intensity of magnetisation used in its old sense, and (resultant) magnetic force used in the sense in which magnetic intensity occurs in the above note, and, what is worse, in place of electric intensity or electromotive intensity we find on, p. 239, "electromotive force," and on p. 244, "electromotive force at a point" used in its place. This result of the interruption of the issue of the new edition is unfortunate, for it renders the confusion of terms greater than ever, and affords a kind of cover to those who excuse or justify the inaccuracy of their own ideas by appealing to the terminological inconsistencies of standard works. Yet we can scarcely blame the editor, for it is a very delicate matter to interfere with another's work, even in points like this.

The want of definiteness in electrical terminology makes itself felt in the definition of the electric strength of a dielectric; thus we find, on p. 51—

"The value of the electromotive force which can exist in a dielectric without a discharge taking place, is called the electric strength of the dielectric."

And again on p. 54:—

"The intensity of the electromotive force when this takes place is a measure of what we may call the electric strength of the dielectric."

Assuming for a moment that any accurate definition of electric strength can be given, which at present, experimentally speaking, is open to doubt; or, which comes to the same thing, taking, as Maxwell probably means us to do, an ideal case, the second of these definitions is right, if we understand by intensity of the electromotive force resultant electric intensity, or electromotive intensity, as previously defined; while the first, strictly read, *i.e.* taking electromotive force to mean the electromotive force between two points, is clearly wrong; for, if it were right, a spark would always pass between two conductors in the same medium when the difference of potential between them is the same, no matter what their form or surroundings, which is well known to be untrue.

In this connection we may mention that the account formerly given of Thomson's classical experiments on the electric strength of air no longer finds a place in Art. 57. No doubt the author had intended to describe them later on along with what has been done of late in the same direction; but no mention of them occurs, except a cursory one in Art. 59; yet we are still referred in Art. 369 to Art. 57 for the account which is no longer there. It is a pity that a footnote was not inserted referring the reader to the reprint of Thomson's papers.

In Chapter II. we may note, as new, an interesting account of Cavendish's experiment, on which, mainly, we may now rest the evidence for the elementary law of electrostatic action; a simplification of the treatment of the variation of the potential at charged surfaces; farther direct application of the theory of lines of force in proving electrical theorems of considerable interest and generality; and a clearer explanation than was formerly given of the distinction between the real electrification according to the medium theory and the apparent electrification which may be used to represent it, if we abstract the specific inductive capacity of the dielectric.

Chapter III. has been in great part rewritten, and several very interesting and practically useful calculations of the approximate values of coefficients of induction and potential have been added. In Chapter IV., which contains the general theory of electrical equilibrium, the changes have been greater still; the result has been, on the whole, we think, considerable simplification; it would appear, however, from the way in which the chapter ends, that the author had contemplated some farther additions. Chapter IX. has also been greatly modified; in particular the problem of two spheres has been worked out in great detail, and series given to a high degree of approximation for the coefficients of induction and potential.

The rest of the work is practically a reprint from the former edition, and calls for little remark. We may, however, call the attention of our readers to Art. 261, the ideas and notation of which will, we believe, be found discordant with the best modern chemical views.[1] The editor has very properly appended a note to Art. 357, calling attention to Mr. Lodge's exposure of the fundamental defect of Mance's method (in the *Philosophical Magazine* for 1877, not 1857, as the reference is printed). Mr. Lodge's remarks are, so far as we know, the first published allusion to the matter, but the defect in question was well known to Prof. Clerk Maxwell, for it was discussed with him by the present writer some time before the above date. It used to be not uncommon to set over-confident students in the Cavendish Laboratory the problem of measuring the internal resistance of a battery, and then to explain to them the reason of the hopelessly indefinite character of the results obtained under certain circumstances by Mance's method. Notwithstanding Mr. Lodge's remarks, one sees, even in the most recent text-books, this method confidently cited as apparently irreproachable.[2] So tenacious is scientific error! Another correction we may mention—the interlacing circuits in Art. 421 *are* now so arranged that the integral $\iint \Pi\, ds\, d\sigma$ vanishes.

Before taking leave of the work, we have to express the gratitude which the scientific public owes to its editor, Mr. Niven. It has been our duty to indicate some points in which there might have been improvement, and we have said little as to what has been actually done by Mr. Niven. It is but justice to him, therefore, to add that we went over the new edition, and compared it with our copy of the first edition, and we found that in almost every case the errors we had noted were corrected, while explanations had been inserted at many of the places where we had found them necessary. The labour involved in doing all this will be best understood by those who are perfectly familiar with the whole of Maxwell's great work, and all such will know how to appreciate the conscientious labour which Mr. Niven has so unostentatiously bestowed on the editing of this edition.

The regret one feels that Prof. Maxwell did not live to complete his work is much increased when we read the elementary treatise. In the earlier parts it is characterised by that originality, freshness, and exemplary clearness familiar to the readers of his Heat. Chapters I. and II. reproduce with more ample experimental detail the admirable introduction to the larger work. Chapter III. gives the mathematical theory of electrical work and energy in a form accessible to students of moderate mathematical acquirements, although such need not expect to

[1] This applies also to the corresponding passage in the elementary work.
[2] It is unfortunate that Mr. Garnett should have transferred the description of Mance's method to the Elementary Treatise, without correcting or alluding to the error it contains.

find it easy reading. Chapter IV., on the exploration of the electric field is perhaps the most interesting in the book; its contents will be familiar to those who heard the lectures of its author, but much of it is new to the scientific public. Chapter V. contains the theory of Faraday's lines of electric induction, and here for the first time the reader begins to feel that the matter must have been left in a state more or less unfit for publication. The text reads more like a series of disjointed notes than a coherent treatise, and the admirable simplicity and symmetry of treatment which characterises the earlier part of the work is lost. Still, what we have is of great interest, and will be invaluable to a good teacher in giving him hints how to arrange an elementary exposition of Faraday's theory. Chapters VI., VII., and VIII. are more complete, and will be very useful in giving to beginners in electricity some idea of the applications of the mathematical theory; Chapter VIII., on capacity, is one likely to be particularly useful, as it deals with one of the fundamental ideas in electrical measurement. We recommend it all the more, as we have seen the term capacity both ill defined and loosely applied in recent treatises. Chapters IX. and X., fragmentary as they are, are full of interest to those who have studied the larger work; for they throw much light upon many points concerning which the author had formerly but briefly indicated his opinion. We may mention more particularly his remarks on the vexed question of contact electromotive force; also, as new, and specially interesting, the experiments on the insulating power of air and other gases, Art. 138, *et seq*.

The rest of the book is a series of extracts from the larger work, concerning the utility of which, in their present form and arrangement, there will be difference of opinion.

In the interest of the author's reputation it might have been better to have published simply what he had left in MSS. in a more confessedly fragmentary form. If, however, the additions that have been made will secure the use of the treatise by elementary teachers, we shall rejoice; for some of the manuals which they use are not remarkable either for scientific method or for the extent and accuracy of their information; in fact the study of many of them, far from introducing the learner to the science of electricity, is simply a waste of his time.

G. CHRYSTAL

OUR BOOK SHELF

The Zoological Record for 1880; being vol. xvii. of the Record of Zoological Literature. Edited by E. C. Rye, F.Z.S. (London: John Van Voorst, 1881.)

WE heartily congratulate the editor on his praiseworthy success in publishing this important Record of Zoological Literature for 1880 before the termination of 1881. It is the first time, as the editor himself reminds us, that this event has taken place since 1870, and now we trust it will be once more the usual plan. So far as we have been able, by looking here and there throughout the volume, to ascertain, this expedition has not been at the expense of accuracy; and as to incompleteness, any omissions are very easily supplied in the next volume. The Recorders are nearly the same as for 1879, Mr. G. A. Boulenger taking the place of the late Mr. O'Shaughnessy, and reporting on the reptiles, batrachians, and fishes. Mr. W. A. Forbes gives us an admirable report on the mammals. This and Mr. Howard Saunders' report on the birds leave little to be desired in either the arrangement of the matter or in the terseness and yet clearness of the notices. The latter Recorder adopts P. L. Sclater's systematic arrangement as laid down in the important paper by Dr. Sclater "On the Present State of the Systema Avium." The mollusca and molluscoida are reported on by Prof. Ed. von Martens, who also gives the record of the crustacea. These we venture to regard as the model reports of the volume. After a pretty full list of the publications relating to the group recorded, we find a list of the special journals and manuals relating to the class. Then under the heading of Anatomy and Physiology, we find most interesting summaries of the additions made to a knowledge of the general morphology, muscular system and movement, shell formation, digestion, excretion and secretion, nervous system, organs of sense, of generation, embryology, abnormities, and even on the action of poisons. After this some details of the geographical distribution, and of the recently-described forms. The amount of labour spent over this most useful grouping of details on the part of the recorder is great, but the reader reaps from it an immense benefit. The literature of the arachnida is recorded by Rev. O. P. Cambridge, with the assistance of Mr. F. M. Campbell. To Mr. Kirby falls the larger share of the Record of the insecta; indeed all the orders save the neuroptera and orthoptera, which fall to Mr. McLachlan's share are reviewed by him. The vermes and echinoderms are recorded by Prof. Bell; the hydrozoa and cœlenterata by Mr. A. G. Bourne; the anthozoa by Mr. S. J. Hickson; while the literature of sponges and protozoa is recorded by Mr. Stuart O. Ridley. From a summary appended by the editor we find that this volume contains a record of no less than 1008 new genera and sub-genera, described as follows:—

Mammalia	34	Myriopoda	2
Aves	16	Insecta	438
Reptilia	21	Vermes	28
Pisces	31	Echinodermata	24
Mollusca and Molluscoida	79	Coelenterata	70
Crustacea	80	Spongiida...	51
Arachnida	78	Protozoa	56

a goodly number, going even beyond the average of most years.

Land und Leute in der brasilianischen Provinz Bahia. Streifzuge von Julius Naeher. (Leipsig: Gustav Weigel, 1882.)

THE author essays, in a small volume of not quite 300 pages, to write a Guide to the Province of Bahia. Starting from Hamburg he steamed, *vià* Lisbon, the Canary and Cape de Verd Islands, to Brazil, and he asserts that he found the steamers on this route excellent. The details of all the other routes from Europe are, however, also given. As the work is the result of the author's own observation, it only describes a small part of the Brazils. It affords a graphic insight into the tropical vegetation of the country, and gives many details as to the sugar plantations. The social life he did not find to differ much from that described in the older books of travel; only the Indians and the wild beasts were less numerous and troublesome. In Bahia about one-fifth of the population belonged to the white race, while about one-half were pure negroes, and the rest were half-castes. While the author does not profess to give a scientific description of the products of this province, he still has evidently paid a good deal of attention to the fruits and other produce of the colony, and in many cases gives statistics as to the present value of these. The gradual abolition of slavery is beginning to hamper the cultivation of sugar, and the great question of the day will no doubt soon be, How is the agriculture of the country, to which so much of the wealth of the country is at present due, to be kept up when slave labour comes to an end?

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 ensure the appearance even of communications containing interesting and novel facts.*]

A Glimpse through the Corridors of Time

IN a letter which appeared in last week's NATURE (p. 217), Dr. Dupré refers to a "too much forgotten paper by Immanuel Kant," and speaks of Kant's contributions to natural science as being, at present, "almost universally overlooked."

Whatever may be the case elsewhere, I do not think that, in England, we are open to this reproach, inasmuch as in the year 1869, when I had the honour to be President of the Geological Society, a very considerable portion of my anniversary address "On Geological Reform" was devoted to an attempt to do justice to Kant's work, and to indicate the high place which it occupies in the history of scientific geology. The address is reprinted in my "Lay Sermons," and therefore I have reason to know that a considerable proportion of the reading, or at any rate book-buying, public has no excuse for "overlooking Kant's work."

I may remark, in passing, that, so far as my knowledge extends, the extreme "Uniformitarianism" which Prof. Ball attacks, has long been as much "a creed outworn" as "Plutonism" or "Neptunism." Indeed, I said as much in 1869.

T. H. HUXLEY

Normal School of Science and Royal School of Mines, South Kensington, January 8

Outburst of Sun-Spots, July 25, 1881

MY letter of August 5, 1881, which appeared in NATURE, vol. xxiv. p. 508, stated that a considerable group of sun-spots burst into appearance between 4 and 5 p.m. (about) on July 25, 1881; or more exactly that the new group was absent at 3h. 58m. (*i.e.* in negative No. 1175), but was present at 4h. 47m. (*i.e.* in negative 1176), local apparent time; further, that no additional negatives could be taken here until July 30, when the spots had disappeared.

This communication has elicited obliging notices by other observers, including Prof. Piazzi Smyth and Prof. Perry, F.R.S., in NATURE, besides others posted to me direct. The observers were not able to observe the sun when the outburst occurred, nor for some twenty-two hours afterwards; none of the observers saw the new group.

One of the observers remarks: "I fancy your sudden group of spots is after all a curious system of blemishes in the negative!" Certainly the appearance of the negative (No. 1176) did not (to me) admit of conjectures suggesting the *unreality* of the spots. However, in presence of the remark now offered, I made inquiries of the photographer, Mr. L. H. Clarke, as to the circumstances connected with his detection of the outburst. I inclose his narrative (see below). It establishes the fact that he *first* saw the new spot-group *on the ground-glass slide* used for focussing the photoheliograph, and indeed that it was *this view* of the unexpected event which urged him to persevere (notwithstanding the clouded state of the sky) in securing a negative, *i.e.* No. 1176, on which the new spot-group he had seen on the ground glass slide stands photographed. His narrative further establishes narrower limits of time in which the outburst occurred, *i.e.* between 3h. 58m. to about 4h. 35m., instead of to 4h. 47m. p.m.

I communicate the foregoing facts, as they are essential circumstances of the event, and should be placed on record.

J. B. N. HENNESSEY

Dehra Doon, N. W. Provinces, India, December 16, 1881

On July 25, 1881, the sun was quite invisible owing to clouds, until towards 4 p.m., when a temporary break occurred, and I took negative No. 1175 at 3h. 58m. p.m. After this the sun again became invisible, while the rising clouds were so dense as to present little hope of getting another negative; so, as evening was approaching I was thinking of closing work for the day, when, while I was still watching at the instrument, an unexpected opening occurred in the rising clouds below the sun, and, soon after, the sun's image appeared on the ground glass used for focussing. To my surprise I now saw, at about 4h. 35m. p.m., a large group of spots about the sun's centre, which were quite absent in the previous negative, No. 1175; little expecting anything of the kind, or indeed to see the sun at all that evening, I was not ready to expose a plate, but now seeing what had happened, I determined to persevere, though the clouds were very unpromising of another break. So I at once took points on my blue setting glass, as is usual to set the instrument by (so as to avoid needless hiding of spots behind the wires), and having done this, I prepared a plate as quickly as possible, and set the exposing slide all ready, though the sun now was invisible; fortunately another opening occurred at 4h. 47m. p.m., when I took negative number 1176, in which appears the group of new spots about the sun's centre, which new group I saw without doubt at about 4h. 35m. on the ground glass for focussing. I then continued to watch for another negative until 5h. 30m. p.m., when the sky having become quite dark, I gave up work for the day.

L. H. CLARKE

December 2

Polymorphism of the Flower-heads of Centaurea Jacea

IN *Centaurea Jacea*, the flower-heads of the same stem, as far as I have seen, are always of the same form, but different stems of the same locality often present astonishing difference in their flower-heads.

In the most common and apparently original form the flower-heads consist of florets which are all of the same tubular shape and all contain both fully developed anthers and stigma, the divergence of the outer florets giving to the whole head a diameter of 20–30 mm. (see H. Müller, "Bie Befruchtung der Blumen," p. 382–384). From this original form variation has gone on in two opposite directions, the final effects of this variation being on the one side most conspicuous male flower-heads of 50–55 mm. diameter, and on the other side less conspicuous female flower-heads of 30–35 mm. diameter. In both these extreme forms the outer row of florets possesses greatly enlarged radiating corollas which are sexually functionless, but useful in making the flower-mass more conspicuous. In the male flower-heads anthers and pistils of the disk-flowers are well-developed, but the style-branches never open so as to expose their stigmatic surfaces, and in their basal portion are grown together. In the female flower-heads, on the contrary, only the pistil of the disk-flowers is fully developed, the anthers being pollenless, shrivelled, and brownish-coloured.

These two extreme forms are linked with the original one by a continuous series of gradations. When in the original form variation begins in the one direction, the outer row of florets gradually becomes longer and more radiating, and in the same degree their sexual organs diminish in size and become functionless, the anthers first aborting, and then the pistil. Finally, the barren ray-florets continuing to increase, the pistils of the disk-florets, too, become functionless, and the conspicuous male flower-head is accomplished.

In the contrary variation some of the outer florets of the original form begin to diminish in size, while their anthers become brownish and pollenless, and this change step by step proceeds inwards and seizes a greater and greater number of disk-florets, until the whole flower-head is female, and redneed to a diameter of 15–18 mm. This state being reached, the corollas of the marginal flowers recommence to increase and become radiating, while in the same time their anthers disappear without leaving any trace, and their style-branches remain closing together.

These are, shortly sketched, the main varieties of *Centaurea Jacea*, near Lippstadt. Further details are about to be published in one of the next numbers of *Kosmos*.

Lippstadt HERMANN MÜLLER

The Weather

THIS morning I noticed the first blossoms of the Coltsfoot (*Tussilago farfara*), ordinarily considered an indication of the near approach of spring. For many years a generous rivalry has existed between myself and a friend (both travellers on the

North Kent Railway, on the banks of which the plant is excessively abundant) as to which could record the first blossoms. I think I have seen them as early as the first week in February; on the other hand, it is possible that the first week in April is on record as the earliest appearance; almost without exception it truly indicated that any long period of severe weather was over for that winter. Will this prove the case with the so-called "winter" of 1881–1882?

Lewisham, January 6 R. McLACHLAN

INDIAN FOSSILS.—Dr. Gordon, of the Manse, Birnie, Elgin, writes that there is a pretty large collection of Siwalik fossils in the Falconer Museum at Forres, N.B.

THE TRANSIT OF VENUS IN 1882

THE French Ministry of Public Instruction has issued the *Procès-verbaux* of the International Conference on the approaching Transit of Venus, held at Paris from the 5th to the 13th of October last. Representatives of fourteen nationalities were present at the Conference, but regret was expressed that the United States had no delegate present Russia; also was unrepresented, but it has been understood that the Government of that country do not propose to organise expeditions beyond the limits of the Empire, or perhaps to undertake observations elsewhere than at the fixed observatories. M. Jules Ferry, then Minister of Public Instruction and the Fine Arts, was present at the opening meeting of the Conference on October 5, and stated its objects; he was named honorary president by acclamation, and on his proposition the meeting proceeded to the election of the acting-officers, which resulted in the choice of M. Dumas, perpetual secretary of the Academy of Sciences of Paris, as president; Prof. Fœrster, director of the Observatory of Berlin, and Prof. Weiss, director of the Observatory of Vienna, as vice-presidents; with M. Hirsch, of the Observatory of Neuchâtel, delegate from the Swiss Republic, and M. Tisserand, the proposed chief of a French expedition to Martinique, as secretaries. M. Dumas pointed out that the expeditions in 1874 were organised by the various nations without any general previous understanding, each acting independently, adding that the necessity of co-operation in the arrangements of different countries for the observation of the approaching phenomenon is now generally admitted. He directed special attention to the desirability of coming to some definite conclusion as to the employment or otherwise of photography on the occasion. In the discussion which followed Prof. Fœrster announced that the German Commission had resolved not to employ photography in 1882, and Mr. E. J. Stone, the Radcliffe Observer, directing astronomer of the British Commission, which he represented at the Conference, mentioned that it was not seriously intended to introduce photography in the expeditions of 1882, remarking that the French results from this method were not encouraging, and the American results had not been published in time to allow of due discussion before the British Commission was called upon to advise the Government on the best methods of observing the transit.

M. d'Abbadie, however, reminded the Conference that good results had been obtained by Mr. Todd from the American photographs. M. Hirsch said the scientific public had been surprised to find that after the lapse of seven years since the transit of 1874 there was yet but a partial publication of results, and these in small number: each nation had given its solar parallax, but could not a different method of procedure be adopted for the transit of 1882? It might be worth while to form a *bureau des calculs*, charged to collect, reduce, and discuss the whole of the observations in 1882, and the same bureau might also discuss the entire series of observations at the late transit, and publish the final value of the sun's parallax from the two transits. Prof. Fœrster took a similar view; Prof. Oudemanns preferred that each commission should in the first place draw up and publish its separate report; the definitive parallax would follow. At the second sitting of the Conference on October 6 M. Dumas notified the stations selected by the French Commission and the observers whom it was proposed to place in charge of the respective expeditions. At three of the stations, viz., Santa-Cruz, Rio Negro, and Port-Desire or Chubut, in Patagonia, M. Mansilla from the Argentine Republic, said the French expeditions might count upon the co-operation of his Government, and the same was stated as regards Santiago, another of the French stations, by Dr. Moesta on the part of the Government of Chile. M. Liais, delegate from Brazil, mentioned that M. Cruls would observe at Rio Janeiro, where the sun would be nearly in the zenith soon after the second contact; he had also organised a station in a locality situated at an altitude of 1800 metres to guard against unfavourable weather at Rio, and a third station would be at Pernambuco, where the chances of a clear sky are very great; further, M. Liais contemplated two additional stations, one of them in the Straits of Magellan; the telescopes employed in Brazil would be of 9 inches and 6 inches aperture. Prof. Fœrster stated that the German Government had not finally decided upon the precise localities to which the four authorised expeditions would be sent, but it had been proposed to place two of them in the southern part of the United States, one in the south of the Argentine Republic, and the fourth at the Falkland Islands. With regard to observations in the Straits of Magellan, M. Bouquet de la Grye, of the French Navy, said the Chilian Government had for a long time past instituted meteorological observations which, while they indicated that the probabilities of a fine sky were great at Santiago, were not promising for the Straits: "Il n'y a peut-être pas une probabilité de $\frac{1}{10}$ de voir une partie seulement du phénomène." Prof. Fœrster stated that according to his information, there existed near the Magellan Straits very limited localities where the conditions would be favourable in December. With regard to the Antilles, to which expeditions were intended to be sent, the conditions, according to M. Bouquet de la Grye, were complicated: at Martinique they would be pretty good, as also for Florida; at Cuba moderately so; on the coasts of the Gulf of Mexico the chances of fine weather are small, though improving in the interior. M. Pechüle of Copenhagen said the Danish Government proposed, with the assent of the Chambers, to equip an expedition either to St. Croix or St. Thomas. Dr. Bakhuysen said, although the Netherlands Government had not made a final decision, it was proposed to send an expedition to Curaçao or St. Martin in the Antilles, and a heliometer would form part of the equipment. M. Viegas, delegate from Portugal, remarking that the weather is usually magnificent in December n that country, mentioned that the observatories of Lisbon and Coimbra possessed large equatorial instruments, and suggested, if it were considered of utility, an expedition might be placed in the Portuguese colonies, at Benguela, for example. Mr. Stone reported upon the selection of stations by the English Commission, the principal centres being the Cape, Australia, New Zealand, and the Antilles. On the part of the Spanish Government, M. Pujazon, director of the Naval Observatory at San Fernando, said it was intended to organise two stations, one at Porto Rico, the other in the southern part of Cuba, where the chances of favourable weather will be considerable: equatorials of 6-inches (English) aperture to be supplied. At the same sitting of the Conference, M. Dumas proposed the nomination of two committees, the one to be charged with the distribution of the observing-stations, the other with the methods and instruments of observation; it had been previously pointed out by M. d'Abbadie that he was named to conduct an expedition to Cuba on the part of the French Government, where it was now stated one would also be sent by Spain: the duplication of stations is not desirable in such a case. The President's propositions were adopted, but in addition the Committee on Methods was also charged with the consideration of the calculations and publication of the observations in 1882, and were further deputed to consider the formation of a temporary international bureau, to be intrusted with the reduction of the whole of the observations. At the fifth and last sitting of the Conference on October 13, the report from the Committee on Methods of Observation, &c., was presented. The proposals of the British Commission respecting the phenomena to be noted at the contacts of the limbs of the sun and Venus, brought forward by Mr. Stone, were made the foundation for a series of instructions to observers, some explanations being appended thereto. After much divergence of opinion with regard to the advantage of an international *bureau des calculs*, the following proposition introduced by M. Dumas, and supported by Mr. Stone on the part of the English commission, was adopted by a large majority.

"The Conference expresses the wish that the French Government may be willing to communicate in diplomatic form with the other governments represented in this conference, or those who are interested in the transits of Venus, in order to lay before them the proposition of convoking, after the return of the expeditions of 1882, an international conference on the transits of Venus, with a view to establish an understanding on the means to be adopted to arrive at the best and most expeditious use of the observations of the transits of 1874 and 1882, and in particular to inquire whether towards this end a temporary international bureau should not be formed."

The report closes with a list of the projected stations for the expeditions, so far as at present arranged. The British stations selected are:—Bermuda, Jamaica, Barbados, Cape Colony (3), Madagascar, New Zealand, Falkland Islands (?), with the Australian observatories.

ON THE PHYSICAL CAUSE OF THE OCEAN BASINS

GEOLOGISTS have reason to thank Prof. Ball for directing their attention to the remarkable investigations of Mr. G. H. Darwin upon "The Precession of a Viscous Spheroid, and the Remote History of the Earth," (*Phil. Trans.* Roy. Soc., Part ii., 1879). Prof. Hull has already been led to point out one result which appeared to him to flow from them, in showing how the ancient tides may have produced the planes of marine denudation, though Mr. Darwin has since expressed doubts as to the legitimacy of this conclusion. I wish to offer another speculation arising from Mr. Darwin's work, which I think may account for the hitherto unexplained distribution of land and water upon the surface of the globe.

Herschel remarked long ago, in his "Physical Geography," that the prevalence of land and water over two opposite hemispheres "proves that the force by which the continents are sustained is one of *tumefaction*, inasmuch as it indicates a situation of the centre of gravity of the total mass of the earth somewhat eccentric relatively to that of the general figure of the external surface —the eccentricity lying in the direction of our antipodes: and is therefore a proof of the comparative *lightness* of the materials of the terrestrial hemisphere." In my "Physics of the Earth's Crust," just published, I have shown reasons for thinking that the distribution of the materials of the earth, which gives rise to this condition, is of the following kind. I accept on the whole the theory that the earth is a hot globe, of which the superficial crust is rendered solid by having become cool, and that the central part is solid, either from great pressure, or from whatever other cause may be assigned; an intervening layer beneath the cooled crust still remaining liquid. The layers of which the whole is composed are arranged in order of their density. Now I have given reasons for believing that Herschel's "comparative lightness of the materials of the terrestrial hemisphere" arises from the fact that the cooled crust beneath the continents is intrinsically less dense than that beneath the great oceans. I think that the crust beneath the continents consists of the cooled acid, or granitic, and therefore lighter magma, which ought naturally to have formed originally the entire superficial portion of the globe. But I conclude that the bottoms of the great oceans consist nevertheless of a crust formed out of the cooled basic layer. Beneath the cooled crust the laws of hydrostatic equilibrium would require that, if the substratum is truly liquid, it should be of the same density under both these areas. I also conclude that the upper surface of the basic crust which forms the floor of the oceans is really depressed below the mean surface of figure.

To these conclusions I arrived without being able to suggest any satisfactory explanation of the facts. I saw that they agreed with, and were supported by, the view of those geologists who assert that the great oceanic and continental areas have never changed places; but neither could I any better see the reason for this.

Let us now inquire whether Mr. Darwin's researches throw any light upon the subject. I shall refer chiefly to the summary and discussion of results appended to his paper, for it is small blame to a sexagenarian, not a professed mathematician, to admit that to follow the *calculations* is beyond the scope of his powers. As I understand Mr. Darwin, he thinks it probable that the moon and the earth were once a single mass, and that at the time when this mass was rotating at the rate of about one revolution in five hours the whole separated into two portions, the smaller of which went to form the moon; and that the moon then began to recede from the earth, until now, after the lapse of fifty-four millions of years *or more*, it is at its present distance. The ellipticity of the mass when rotating at the above-named speed would be about 1-12th. [This would make the mass very much less compressed than an ordinary orange.] He does not think it probable that this amount of ellipticity would cause the spheroid to break up simply from the centrifugal effect of the rotation; but he suggests, judging from the calculated period of a gravitational oscillation of a fluid spheroid, of uniform density equal to the mean of the earth, viz. 1 hour 34 minutes, that the period of the free oscillation of a spheroid "consisting of a denser nucleus and a rarer surface," but of the same mean density as the earth, might coincide with the period of the bodily solar tide at that time. "It seems to be quite possible that two complete gravitational oscillations of the earth in its primitive state might occupy four or five hours." "Accordingly the solar tides would be of enormous height." He then adds: "Does it not then seem possible that, if the rotation were fast enough to bring the spheroid into anything near the unstable condition, then the large solar tides might rupture the body into two or more parts? In this case one would conjecture that it would not be a ring that would detach itself."

I now proceed to build my speculation upon his. It is obvious that, according to the above theory, the act of fissiparturition by which the moon was born must have been sudden. One of the two solar tidal protuberances broke away from the earth to inchoate a separate existence. A great but shallow hole must consequently have been formed, whose centre would have been on or near the equator. Prof. Ball says: "Not for long would that fragment retain an irregular form; the mutual attraction of the particles would draw the mass together. By the same gentle ministrations the wound on the earth would soon be healed. In the lapse of time the earth would become as whole as ever, and at last it would not retain even a scar to testify to the mighty catastrophe."

I form a less hopeful prognostication. I think the ocean basins are the scar, which still testify to the place of separation.

The density of the moon is 0·56654 times that of the earth. Putting the mean density of the earth at 5·5, this makes the density of the moon 3·1. The density of granite is about 2·68, and that of basalt 2·96. Consequently the density of the moon is a little greater than that of the basic layer of the earth's surface, which I think we may expect to occur at the sea-board at a depth of about 25 miles. The entire mass of the moon is 0·011364 of the mass of the earth.

Accordingly, it would require a layer of about 31 miles thick, of the density of granite, to be taken off the surface of the primitive mass to make a body of the mass of the moon; and if the mean density of the matter removed was the same as that of the moon, a somewhat thinner layer would suffice. But if we reduce the area of the skin removed to the area of the oceans, it would require to be $\frac{197}{146} \times 31$, or about 41 miles deep. Hence a uniform layer rather less than 41 miles thick taken off the oceanic areas would be sufficient to make the moon.

Of course the layer removed would not, in fact, have been of uniform thickness. But the above estimate gives an idea of the size of the cavity which would be produced. What then would happen? This would depend upon whether the surface had already become at all solid. I conceive this would be the case at a very early stage, judging from the manner in which a solid layer forms on the liquid lava of Kilauea. The hole would therefore fill up by the rise of the liquid from below, rather than by the lateral approach of the edges of the wound. When the raw surface again solidified we should have a crust of greater density over the area in question, because formed from a lower and denser layer, which would have risen not quite to the level of the lighter crust. There would, however, have necessarily been a certain amount of flow in the upper fluid layers towards the cavity, and this would have carried the cooled granitic crust which, floating on it, still remained upon the earth along with it. What was left of the granitic crust would therefore be broken up into fragmentary areas, now represented by the continents. This would make the Atlantic a great rent, and explain the rude parallelism which exists between the contours of America and the Old World.

The sudden rupture of so considerable a fragment from the rotating spheroid, would alter its mass, form, and moment of momentum. It appears then that its axis of rotation would be altered, which might account for the fact, that the approximate pole of the oceanic area is not in the equator.

The volcanic surface of the moon, if volcanic it be, would lend considerable support to the view which I maintain, that the water substance emitted by volcanoes is an integral constituent of the fluid substratum. For when the moon broke away from the earth it would carry with it the aqueous constituent of the magma. Owing to the much smaller force of gravity in the moon, the pressure under which this would there be placed would be much less than in the earth. Consequently it would more easily escape, and the signs of volcanic action would be more pronounced. But the difficulties surrounding terrestrial vulcanism are so great, that one is hardly tempted to add the lunar to them. O. FISHER

CLASSIFICATION OF THE DINOSAURIA[1]

IN the May number of the *American Journal of Science* (p. 423) I presented an outline of a classification of the Jurassic Dinosaurian reptiles of this country which I had personally examined. The series then investigated is deposited in the Museum of Yale College, and consists of several hundred individuals, many of them well preserved, and representing numerous genera and species. To ascertain how far the classification proposed would apply to the material gathered from wider fields, I have since examined various Dinosaurian remains from other formations of this country, and likewise during the past summer have visited most of the museums of Europe that contain important specimens of this group. Although the investigation is not yet completed, I have thought the results already attained of sufficient interest to present to the Academy at this time.

In previous classifications, which were based upon very limited material compared with what is now available, the Dinosaurs were very generally regarded as an order. Various characters were assigned to the group by von Meyer, who applied to it the term *Pachypoda;* by Owen, who subsequently gave the name *Dinosauria,* now in general use; and also by Huxley, who more recently proposed the name *Ornithoscelida,* and who first appreciated the great importance of the group, and the close relation it bears to birds. The researches of Leidy and Cope in this country, and Hulke, Seeley, and others in Europe, have likewise added much to our knowledge of the subject.

An examination of any considerable portion of the Dinosaurian remains now known will make it evident to any one familiar with reptiles, recent or extinct, that this group should be regarded not as an order but as a sub-class, and this rank is given it in the present communication. The great number of subordinate divisions in the group, and the remarkable diversity among those already discovered indicate that many new forms will yet be found. Even among those now known, there is a much greater difference in size and in osseous structure than in any other sub-class of vertebrates, with the single exception of the placental Mammals. Compared with the Marsupials, living and extinct, the *Dinosauria* show an equal diversity of structure, and variations in size from by far the largest land animals known—fifty or sixty feet long, down to some of the smallest, a few inches only in length.

According to present evidence the Dinosaurs were confined entirely to the Mesozoic age. They were abundant in the Triassic, culminated in the Jurassic, and continued in diminishing numbers to the end of the Cretaceous period, when they became extinct. The great variety of forms that flourished in the Triassic render it more than probable that some members of the group existed in the Permian period, and their remains may be brought to light at any time.

The Triassic Dinosaurs, although so very numerous, are known to-day mainly from footprints and fragmentary osseous remains. Not more than half-a-dozen skeletons, at all complete, have been secured from deposits of this period; hence, many of the remains described cannot at present be referred to their appropriate divisions in the group.

From the Jurassic period, however, during which Dinosaurian reptiles reached their zenith in size and numbers, representatives of no less than four well-marked orders are now so well known that different families and genera can be very accurately determined, and almost the entire osseous structure of typical examples, at least, be made out with certainty. The main difficulty at present with the Jurassic Dinosaurs is in ascertaining the affinities of the diminutive forms which appear to approach birds so closely. These forms were not rare, but their remains hitherto found are mostly fragmentary, and can with difficulty be distinguished from those of birds, which occur in the same beds. Future discoveries will, without doubt, throw much light upon this point.

Comparatively little is yet known of Cretaceous Dino-

[1] By Prof. O. C. Marsh. Read before the National Academy of Sciences, at the Philadelphia meeting, November 14, 1881. Communicated by the Author.

saurs, although many have been described from incomplete specimens. All of these appear to have been of large size, but much inferior in this respect to the gigantic forms of the previous period. The remains best preserved show that, before extinction, some members of the group became quite highly specialised.

Regarding the Dinosaurs as a sub-class of the REPTILIA, the forms best known at present may be classified as follows:—

SUB-CLASS DINOSAURIA

Premaxillary bones separate; upper and lower temporal arches; rami of lower jaw united in front by cartilage only; no teeth on palate. Neural arches of vertebræ united to centra by suture; cervical vertebræ numerous; sacral vertebræ co-ossified. Cervical ribs united to vertebræ by suture or ankylosis; thoracic ribs double-headed. Pelvic bones separate from each other, and from sacrum; ilium prolonged in front of acetabulum; acetabulum formed in part by pubis; ischia meet distally on median line. Fore and hind limbs present, the latter ambulatory and larger than those in front; head of femur at right angles to condyles; tibia with procnemial crest; fibula complete. First row of tarsals composed of astragalus and calcaneum only, which together form the upper portion of ankle joint.

(1.) Order SAUROPODA (Lizard foot).—Herbivorous.

Feet plantigrade, ungulate; five digits in manus and pes; second row of carpals and tarsals unossified. Pubes projecting in front, and united distally by cartilage; no post-pubis. Precaudal vertebræ hollow. Fore and hind limbs nearly equal; limb bones solid. Sternal bones parial. Premaxillaries with teeth.

(1) Family *Atlantosauridæ*. Anterior vertebræ opisthocœlian. Ischia directed downward, with extremities meeting on median line.

Genera *Atlantosaurus*, *Apatosaurus*, *Brontosaurus*, *Diplodocus*, ? *Camarasaurus* (*Amphicœlias*), ? *Dystrophæus*.

(2) Family *Morosauridæ*. Anterior vertebræ opisthocœlian. Ischia directed backward, with sides meeting on median line.

Genus *Morosaurus*.

European forms of this order: *Bothriospondylus*, *Cetiosaurus*, *Chondrosteosaurus*, *Eucamerotus*, *Ornithopsis*, *Pelorosaurus*.

(2.) Order STEGOSAURIA (Plated lizard).—Herbivorous.

Feet plantigrade, ungulate; five digits in manus and pes; second row of carpals unossified. Pubes projecting free in front; post-pubis present. Fore limbs very small; locomotion mainly on hind limbs. Vertebræ and limb bones solid. Osseous dermal armor.

(1) Family *Stegosauridæ*. Vertebræ biconcave. Neural canal in sacrum expanded into large chamber; ischia directed backward, with sides meeting on median line. Astragalus co-ossified with tibia; metapodials very short.

Genera *Stegosaurus* (*Hypsirhophus*), *Diracodon*, and in Europe *Omosaurus*, Owen.

(2) Family *Scelidosauridæ*. Astragalus not co-ossified with tibia; metatarsals elongated; four functional digits in pes. Known forms all European.

Genera *Scelidosaurus*, *Acanthopholis*, *Cratæomus*, *Hylæosaurus*, *Polacanthus*.

(3.) Order ORNITHOPODA (Bird foot).—Herbivorous.

Feet digitigrade, five functional digits in manus and three in pes. Pubes projecting free in front; post-pubis present. Vertebræ solid. Fore limbs small; limb bones hollow. Premaxillaries edentulous in front.

(1) Family *Camptonotidæ*. Clavicles wanting; post-pubis complete.

Genera *Camptonotus*, *Laosaurus*, *Nanosaurus*, and in Europe *Hypsilophodon*.

(2) Family *Iguanodontidæ*. Clavicles present; post-pubis incomplete. Premaxillaries edentulous. Known forms all European.

Genera *Iguanodon*, *Vectisaurus*.

(3) Family *Hadrosauridæ*. Teeth in several rows, forming with use a tesselated grinding surface. Anterior vertebræ opisthocœlian.

Genera *Hadrosaurus*, ? *Agathaumas*, *Cionodon*.

(4.) Order THEROPODA (Beast foot).—Carnivorous.

Feet digitigrade: digits with prehensile claws. Pubes projecting downward, and co-ossified distally. Vertebræ more or less cavernous. Fore limbs very small; limb bones hollow. Premaxillaries with teeth.

(1) Family *Megalosauridæ*. Vertebræ biconcave. Pubes slender, and united distally. Astragalus with ascending process. Five digits in manus and four in pes.

Genera *Megalosaurus* (*Poikilopleuron*), from Europe. *Allosaurus*, *Cœlosaurus*, *Creosaurus*, *Dryptosaurus* (*Lælaps*).

(2) Family *Zanclodontidæ*. Vertebræ biconcave. Pubes broad elongate plates, with anterior margins united. Astragalus without ascending process; five digits in manus and pes. Known forms European.

Genera *Zanclodon*, ? *Teratosaurus*.

(3) Family *Amphisauridæ*. Vertebræ biconcave. Pubes rod-like; five digits in manus and three in pes.

Genera *Amphisaurus* (*Megadactylus*), ? *Bathygnathus*, ? *Clepsysaurus*; and in Europe, *Palæosaurus*, *Thecodontosaurus*.

(4) Family *Labrosauridæ*. Anterior vertebræ strongly opisthocœlian, and cavernous. Metatarsals much elongated. Pubes slender, with anterior margins united.

Genus *Labrosaurus*.

Sub-Order CŒLURIA (hollow tail).

(5) Family *Cœluridæ*. Bones of skeleton pneumatic or hollow. Anterior cervical vertebræ opisthocœlian, remainder bi-concave. Metatarsals very long and slender.

Genus *Cœlurus*.

Sub-Order COMPSOGNATHA.

(6) Family *Compsognathidæ*. Anterior vertebræ opisthocœlian. Three functional digits in manus and pes. Ischia with long symphysis on median line. Only known specimen European.

Genus *Compsognathus*.

DINOSAURIA?

(5.) Order HALLOPODA (leaping foot).—Carnivorous?

Feet digitigrade, unguiculate; three digits in pes; metatarsals greatly elongated; calcaneum much produced backward. Fore limbs very small. Vertebræ and limb bones hollow. Vertebræ biconcave.

Family *Hallopodidæ*.

Genus *Hallopus*.

The five orders defined above, which I had previously established for the reception of the American Jurassic Dinosaurs, appear to be all natural groups, well marked in general from each other. The European Dinosaurs from deposits of corresponding age fall readily into the same divisions, and, in some cases, admirably supplement the series indicated by the American forms. The more important remains from other formations in this country and in Europe, so far as their characters have been made out, may likewise be referred with tolerable certainty to the same orders.

The three orders of Herbivorous Dinosaurs, although widely different in their typical forms, show, as might be expected, indications of approximation in some of their aberrant genera. The *Sauropoda*, for example, with *Atlantosaurus* and *Brontosaurus*, of gigantic size, for their most characteristic members, have in *Morosaurus* a branch leading toward the *Stegosauria*. The latter order, likewise, although its type genus is in many respects the most strongly marked division of the Dinosaurs, has its

Scelidosaurus, a form with some features pointing strongly towards the *Ornithopoda*.

The Carnivorous *Dinosauria* now best known may all be placed at present in a single order, and this is widely separated from those that include the herbivorous forms. The two sub-orders defined include very aberrant forms, which show many points of resemblance to Mesozoic birds. Among the more fragmentary remains belonging in this order, but not included in the present classification, this resemblance appears to be carried much farther.

The order *Hallopoda*, which I have here referred to the *Dinosauria*, with doubt, differs from all the known members of that group in having the hind feet specially adapted for leaping, the metatarsals being half as long as the tibia, and the calcaneum produced far backward. This difference in the tarsus, however, is not greater than may be found in a single order of Mammals, and is no more than might be expected in a sub-class of Reptiles.

Among the families included in the present classification, I have retained three named by Huxley (*Scelidosauridæ*, *Iguanodontidæ*, and *Megalosauridæ*),[1] although their limits as here defined are somewhat different from those first given. The sub-order *Compsognatha*, also, was established by that author in the same memoir, which contains all the more important facts then known in regard to the *Dinosauria*. With the exception of the *Hadrosauridæ*, named by Cope, the other families above described were established by the writer.

The *Amphisauridæ* and the *Zanclodontidæ*, the most generalised families of the *Dinosauria*, are only known from the Trias. The genus *Dystrophæus*, referred provisionally to the *Sauropoda*, is likewise from deposits of that age. The typical genera, however, of all the orders and sub-orders are Jurassic forms, and on these especially the present classification is based. The *Hadrosauridæ* are the only family confined to the Cretaceous. Above this formation, there appears to be at present no satisfactory evidence of the existence of any *Dinosauria*.

[1] Quarterly Journal Geological Society of London, vol. vxvi. p. 34. 1870.

THE TAY AND THE FORTH BRIDGES

THE reconstruction of the Tay Bridge (if it really go on) by Mr. W. H. Barlow and the re-designing of the Forth Bridge by Mr. John Fowler and Mr. B. Baker will undoubtedly mark a new point of departure in the practice of British engineers. With the advent of railways there arose a generation of engineers who for some inexplicable reason ignored the traditions of their predecessors and gave no thought to wind pressure. Previous to this the question was always considered of vital importance by constructors. For example, Tredgold, writing some sixty years ago about roofs over building slips, directed special attention to the fact that such structures were "much exposed to be racked and strained by high winds," and recommended certain proportions, based upon the assumption of the actual weight of the roof being 16 lbs. per square foot, and the pressure of the wind 40 lbs. per foot. He thus clearly warned engineers that in some instances the pressure of the wind and not the load governs the strength of the structure. Nevertheless so completely have British engineers ignored this condition that it may safely be said at least three-fourths of the railway bridges in Great Britain and Ireland have no lateral bracing or provision of any kind to enable them to resist wind pressure. Even metallic arched bridges, which from their form must, in the absence of cross bracing, be necessarily in a state of more or less unstable equilibrium, form no exception to the rule. At Richmond, for instance, and at Kingston also, there are cast-iron arches about 100 feet in span, the lateral stability of which is dependent solely upon the 8 inches or 10 inches wide flanges of the arched ribs. There is no lateral bracing nor are there any iron cross-girders to bind the arched ribs together, and the lateral stiffness of a 10-inch flange over a span of 100 feet is more easily imagined than calculated. Within a few hundred yards of the Richmond Bridge is an anemometer which, according to the official returns, has not infrequently recorded a pressure of 27 lbs. per square foot, but it is hardly necessary to say that no wind pressure even approximating to that amount could ever have taken effect on the bridge.

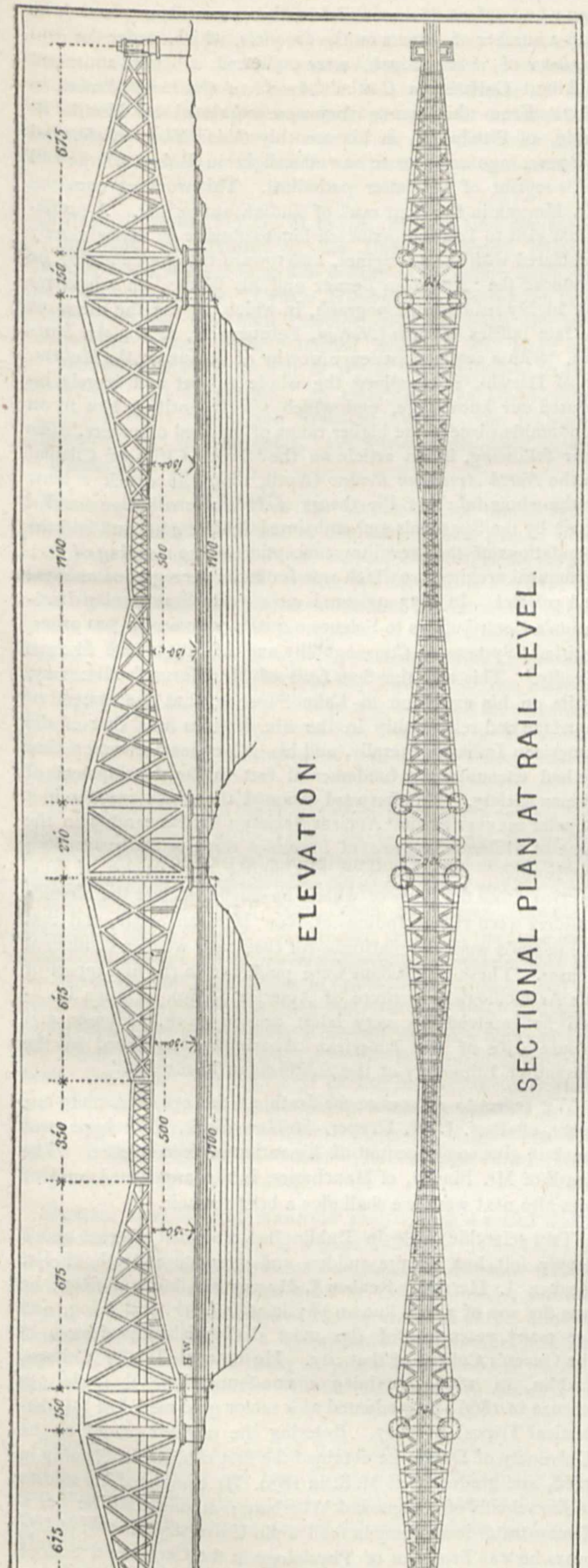
Since the fall of the Tay Bridge the principles and practice of Telford's day have been reverted to by British engineers, and the question of wind pressure has been most influential in determining the design and proportions of the new Tay and the proposed Forth Bridges.

In the original Tay Bridge the type of pier foundation finally developed was, it may be remembered, a single cylinder of 31 feet diameter. This was satisfactory enough as regards vertical pressure, but in the new design it was lateral and not vertical pressure which governed the form of the pier's foundation, and the latter will consist not of a single 31 feet cylinder but of two 23 feet cylinders spread 32 feet apart centre to centre, and affording correspondingly increased lateral stability. Similarly, as regards the metallic piers resting on these foundations: originally these consisted of a group of cast iron columns, and as regards vertical pressure nothing could be better, for, as we have recently ascertained by tests, a hollow cast-iron column of ordinary proportions will carry more load than either a wrought iron or a steel tube of equal weight. When the bending action of the wind upon a bridge pier is taken into consideration, however, the steady vertical pressure due to the load becomes of comparatively little moment, and Mr. Barlow has very properly adopted wrought iron for the piers of the new Tay Bridge, and the Board of Trade have with no less propriety intimated, in their recent "Memorandum of Requirements," that piers made up of a group of small cast-iron columns will no longer be passed by the inspecting officers.

The superstructure of the new Tay Bridge, no less than the piers, affords evidence of the provision which it is now thought necessary to make against the consequences of high wind pressures. Thus Mr. Barlow has provided three lines of defence against a train being hurled into the Tay, firstly, a guard balk of considerable height outside each rail; secondly, a ballasted floor of sufficient strength to hold up a derailed locomotive at any point; and thirdly, a strong iron parapet. Most of these provisions will in all probability be insisted upon by the Board of Trade in future railway bridges.

Turning now to the gigantic Forth Bridge, the influence of wind pressure in determining the design is beyond all precedent. The assumed lateral pressure of the wind upon the 1700 feet span girder is in fact no less than 50 per cent. greater than the maximum rolling load, so that were it not for the influence of gravity on the mass of the bridge, the required strength would be greater laterally than vertically to the extent of one-half. The weight of steel in the 1700 feet girder is, however, so considerable, that the stresses both for rolling load and wind pressure are relatively less than in smaller bridges.

The original design for the Forth Bridge by Sir Thomas Bouch was, it will be remembered, on the suspension principle. Except as regards the enormous drop in the suspension chains and the consequent unprecedented height of the piers, there was little to distinguish the proposed structure from an ordinary suspension bridge with stiffening girder, and without the inclined stays characteristic of American suspension bridges. During the past forty years the suspension principle has been universally rejected by engineers of all countries as unsuitable to the conditions of high-speed railway traffic, and the only reason for introducing it in the case of the Forth Bridge was the assumption that no other plan was commercially



The Forth Bridge.

attainable. It caused no little surprise therefore, and not a few expressions of incredulity, when Messrs. Fowler and Baker announced that the span of 1700 feet was no reason at all for departing from the old and well-tested principle of girder construction, but that on the contrary a girder bridge of proper design would be not only infinitely stiffer, but also considerably cheaper, than a structure on the suspension principle. This conclusion was however confirmed by Mr. W. H. Barlow and Mr. T. E. Harrison after careful independent investigation, and a design was finally agreed upon by all the engineers and accepted by the four railway companies finding the funds for its construction.

The Forth Bridge as intended to be constructed may be briefly described as a continuous steel girder bridge, of varying depth and with fixed points of contrary flexure. In several essential points it is analogous to the well-known continuous tubular girder bridge across the Menai Straits, as in both cases the continuous girder traverses two main spans and two end spans of about half the width of the former. In the Forth Bridge, however, the main spans are 1700 feet, and the end spans 675 feet, whilst in the Britannia Bridge the respective dimensions are but 460 feet and 230 feet. It is hardly necessary to say, therefore, that though the principle is the same the proportions and general details of the two structures present no points of resemblance.

The continuous girders of the Forth Bridge are 340 feet, or one-fifth of the span, in depth at the piers, and only 45 feet deep at the centre of the 1700 feet span. In ordinary continuous girders the points of contrary flexure are situated about one-fifth of the span from the piers, but in the Forth Bridge they are fixed at the most economical distance, which was found on investigation to be about two-fifths. Every continuous girder may, for purpose of calculation, be regarded as made up of a central girder supported by two cantilevers. In ordinary cases the central girder will have an effective span about equal to the total span $\div \sqrt{3}$. If the Forth Bridge were an ordinary continuous girder the effective span of the central girder would thus be about 982 feet, but as a matter of fact it will be made 350 feet, and it is to this deviation from the ordinary proportions that the economy of the design is largely due. By keeping the central girder of moderate span, or in other words by removing the points of contrary flexure farther from the piers, the central portion of the girder is lightened, the bending moment correspondingly reduced, and the mass of metal is concentrated near the piers, where it acts with the least leverage. Thus the lower member of the great girder, where it springs from the masonry piers at a height of 20 feet above high water, is a steel tube 12 feet diameter and about 2 inches thick, whilst at the centre of the span, where the bottom member has been cambered upwards to a height of 150 feet for navigation purposes, it is of ordinary trough section, about 3 feet in width.

On plan as well as in elevation the Forth Bridge is a continuous girder of varying depth. To resist high wind pressures the tubular lower members are spaced 120 feet apart, centre to centre at the piers, whereas the trough lower members of the central girder are spaced but 27 feet apart. By varying the effective depth of the girder both on plan and elevation a great saving is effected in the bracing, as the shearing stresses are to a large extent taken up by the main members of the girder. In a bowstring girder the bracing is similarly lightened by the arching of the top member. There are other sources of economy in the proposed design which we have not space to refer to at present.

It is intended, we understand, to give such proportions to the several members of the structure that under the combined stresses resulting from the maximum rolling load and a wind pressure of 56 lbs. per square foot, the stress per square inch will in no case exceed one-fourth of the ultimate strength of the steel used in construction. The steel used in compression will be considerably stronger and harder than the more ductile material used for the parts in tension.

Facility of erection has necessarily been one of the most important governing elements in the design of the Forth Bridge. With 200 feet depth of water, scaffolding is of course out of the question, and the only practicable way of erecting the great girders is by commencing the work at each pier and adding successive bays of struts, ties, and bracing on either side, until the whole structure is complete. This mode of erection, technically known as "erection by overhang," is that which has been successfully adopted in the two largest railway bridges yet constructed, namely, the St. Louis Bridge across the Mississippi, of 525 feet span, and the Douro Bridge, of the same span, near Lisbon. The great advantage and security of erection by overhang in a case like the Forth Bridge is that every piece of the work is finished and securely braced against storms before another length is commenced. When the bridge is partially finished it will at the distance present much the appearance of three huge birds perched with outspread wings on as many rocks. The distance between the tips of the wings will be represented by that between the ends of the cantilevers, or say, 1500 feet. It might be thought that this would be a critical stage of the erection, but it is not so, as the work is so designed that if a hurricane of 56 lbs. per square foot were to strike one wing whilst the other remained becalmed the cantilevers would not spin round like a weathercock, but remain perfectly stable and secure.

We understand that the Board of Trade have signified their approval of the design and of the provisions made by the engineers as regards strains and wind pressures.

NOTES

THE last work of the late Mr. H. C. Watson on the distribution of British plants was his "Topographical Botany," published in 1873-4, in which he traced the dispersion of each species through the 112 vice-counties of Britain which he adopted. Of this book only 100 copies were printed for private circulation, and these were all given away by the author immediately. Since its issue a large amount of new material has been accumulated, principally through the exertions of the members of the Botanical Record Club, and at the time of his death, last autumn, Mr. Watson was engaged in the preparation of a new edition. This he did not live to complete as regards its prefatory and explanatory portions, but he had kept an interleaved copy in which he regularly entered up every record of any plant in a new district that was brought to his notice. At his own special request this was deposited with his herbarium at Kew, and from this it is now proposed to prepare a second edition of the book, which Mr. Quaritch has undertaken to publish, and Mr. J. G. Baker, of the Royal Gardens, and the Rev. W. W. Newbould to make ready for the press.

THE New York *Nation* announces the death of Mr. Lewis H. Morgan at Rochester, N.Y., on December 17, after a brief illness following a long period of delicate health. "With reference to our pre-Columbian antiquities," the *Nation* states, "he might for some time past have been called the Nestor of Indian ethnologists. A native of Western New York, he early became interested in the neighbouring remnant of the once mighty Six Nations, and gained a thorough insight into the political and military constitution of the Confederacy, its manners and customs, and above all its curious system of tribal intermarriages. Together with some kindred inquiring spirits he instituted, at the age of twenty-five, an Order, or 'New Confederacy' of the Iroquois—a sort of antiquarian society, having as a subsidiary aim the promotion of a kindlier feeling toward the red man; and before its 'councils' in the years 1844, 5, and 6, he read a number of papers on the Iroquois, which, under the *nom de plume* of 'Skenandoah,' were published, as letters addressed to Albert Gallatin, in Cotton's *American Quarterly Review*, in 1847. From this source they were reprinted by Neville B. Craig, of Pittsburgh, in his monthly *Olden Time* (1848), and five years ago once more saw the light in Robert Clarke and Co.'s reprint of the latter periodical. This work at once put Mr. Morgan in the front rank of Indian authorities. A professional visit to Lake Superior led him to observe an animal closely associated with the aborigines, and toward the close of 1867 he produced the 'American Beaver and his Works,' an exhaustive but highly readable monograph, in which, to use the words of the late Jeffries Wyman (*Nation*, February 27, 1868), Mr. Morgan, 'with a zeal and patience worthy of Réaumur, the Hubers, or of Darwin, re-examined the whole subject and largely increased our knowledge,' and which 'justly entitled him to an honourable place in the higher ranks of original observers.' The year following, in an article on the 'Seven Cities of Cibola,' in the *North American Review* (April, 1869), he struck a blow at the whole fabric of the theory of Indian civilisation handed down by the Spaniards and embalmed by Prescott, and laid the foundations of the prevailing conception of the meaning of that communal architecture which was for centuries regarded as royal and palatial. In 1873 appeared among the Smithsonian Institution's Contributions to Science a quarto volume of 700 pages, entitled 'Systems of Consanguinity and Affinity of the Human Family.' This was the first fruit of Mr. Morgan's discovery, while on his excursion to Lake Superior, that the system of marriage and relationship in the Six Nations was that of the American Indians generally, and his subsequent reflection that he had encountered a fundamental fact in the development of human society. He afterward resumed the whole question in a popular manner in his 'Ancient Society; or, Researches in the Line of Human Progress of Savagery through Barbarism into Civilisation.' His last work was among the pueblos of New Mexico, from the study of which he concluded that the Mound-builders were village Indians of New Mexican origin, and that the mounds were the platforms for their long wooden communal houses. These conclusions were published in the first report of the Archæological Institute of America (1880). On his death-bed he received his very latest printed work, 'Houses and House Life of the American Aborigines,' published by the Bureau of Ethnology of the Smithsonian Institution."

WE regret to announce the death, at the age of seventy-one years, of Prof. J. W. Draper, of New York. We hope next week to give some account of his varied work in science. The death of Mr. Binney, of Manchester, is also announced, and of him also next week we shall give a brief memoir.

THE scientific circle in Dublin has sustained a great and a deeply felt loss by the sudden and premature death of Dr. Reuben J. Harvey. Reuben J. Harvey was born in 1845; he was the son of a well known physician in Cork, still living, and for many years one of the most distinguished professors in the Queen's College of that city. He entered Trinity College, Dublin, in 1863, obtaining a non-foundation scholarship in science in 1866; he graduated as a senior gold medallist (Mathematical Tripos) in 1867. Entering the medical school of the University of Dublin he obtained the first medical scholarship in 1868, and graduated as M.B. in 1870. He completed his studies in the schools of Vienna and Würzburg; at one time he was a demonstrator in anatomy in the Dublin University School; of late years he was Professor of Physiology in the Carmichael School of Medicine, and one of the physicians of the Cork Street Fever Hospital. In the pursuit of his professional duties he was taken ill of typhus fever on the 24th of December last, of which he died on the 28th of the same month. His sterling qualities endeared him to a host of friends; his mathematical abilities were of no common order, he was a painstaking and enthusiastic worker in the school of modern physiology, but it is to be feared that the results of much of his labour will now never be known, for he was slow to publish, preferring to wait for a completer confirmation of his numerous observations. He was an extremely popular lecturer, and his sudden death has saddened many a heart.

MR. A. G. MORE, F.L.S., M.R.I.A., &c., has been appointed to the post of Curator of the Dublin Natural History Museum. Mr. More is known to botany as the author (with the late Dr. Moore) of the "Cybele Hibernica." During the past fifteen years he has been principal assistant under the late curator, Dr. Carte. The published works of Mr. More are a "Natural History of the Isle of Wight," a supplement to Dr. Bromfield's "Flora Vectensis," an essay on "The Distribution of British Birds during the Breeding Season," the "Cybele Hibernica" and its "appendix," and numerous detached papers on natural history.

WE are glad to learn from Dr. Lindemann, of the Geographische Gesellschaft in Bremen, that the brothers Krause were sent by the Society to explore the Behring Straits regions, *not* for commercial purposes. MM. Krause are naturalists, and have no other object than to make observations and collections in natural history and ethnology. Their reports on their studies on the Chukchi Peninsula, which will be published with charts and woodcuts in the next numbers of the *Journal* of the Bremen Society, will prove that their voyage was not at all unsuccessful. They are now on their way from San Francisco to Sitka, and intend to winter on a station of the North-West Trading Company.

M. A. HÜNTER, who has spent twenty years in the exploration of the flora and fauna of the Onega region, discusses (*Memoirs* of the St. Petersburg Society of Naturalists, vol. xi.) the interesting question as to the natural boundary between Finland and Northern Russia, which had already been raised by Wirtzen, Bonsdorf, Malmgren, and Nylander. M. Hünter arrives at the same conclusions as most of the above-named explorers, namely, that all the region west of Lake Onega to the Gulf of Bothnia, and as far as the White Sea to the north must be considered as a part of Finland, Lake Onega being a marked boundary between the two regions as to their geological structure, topographical features, fauna, and flora. The flora west of this lake is far richer than east of it, and does not contain plants which are common to the latter region and higher latitudes. The list of plants of the whole of the Onega region contains 578 Angiospermæ, 5 Gymnospermæ, and 36 Cryptogams.

MESSRS. TAYLOR AND FRANCIS have issued a useful Tide-Table for 1882, in the form of a large wall-card. The Table is compiled by Mr. E. Roberts, of the Nautical Almanac Office, and contains the time of high-water at London Bridge, and the depths on the silt of the Shadwell lower entrance of the London Docks, showing also in a conspicuous manner the possible overflows. The table is likely to prove useful to many people, and we trust Mr. Roberts will be encouraged to continue it yearly.

THE Geographical Society of Paris received, at its last meeting, a communication sent from Lieut. Rogozinsky, of the Russian Imperial Navy, who proposes to explore the region between the Congo, the southern borders of Adaman, and the Cameroon Mountains.

A SCIENTIFIC Commission has been appointed by the Préfet of the Seine to determine the measures which ought to be taken immediately for the protection of the public in theatres. All of them which will not comply immediately with the provisions of the law will be closed. Two of them have been already proclaimed so. A sharp discussion took place in the Municipal Council, and it was proposed by influential members that the electric incandescent light should be deemed obligatory for all the municipal theatres.

THE Council of the Meteorological Society have determined upon holding an Exhibition of Anemometers at the Institution of Civil Engineers, 25, Great George Street, on the evening of March 15 next. The Committee are anxious to obtain as large a collection as possible of various patterns of anemometers, either full size, models, photographs, or drawings. Special interest will attach to all apparatus bearing upon the history of anemometers and to their modification and improvement. The Committee will also be glad to show any new meteorological apparatus invented and first constructed since the last Exhibition.

THE French exploring party who went to Fouta Djalon in the Niger Country, has arrived in Paris with a deputation from the sovereign of that land who has entered into a treaty with them. This is an important step towards the opening of Timbuctoo to trading caravans.

WITH the January number of the London Missionary Society's *Chronicle* is issued a sketch-map of South-Eastern New Guinea, in which are included the discoveries recently made by their agents, Messrs. Chalmers, Macfarlane, and Beswick.

AT Leghorn an interesting geological discovery has been made. The brothers Orlando have found a well-preserved skeleton of an *Elephas antiquus;* it was lying at a depth of about 4 metres below sea-level. Prof. Meneghini, of Pisa, superintended the excavation, and announces that the tusks are pretty straight and have the enormous length of nearly 4 metres.

AT Nordrup, near Ringsted (Denmark), an interesting discovery has been made. At a depth of only a few feet in a deposit of pumice-stone the remains of seven human bodies were found, together with numerous bronze objects, urns, gold rings, Roman glasses, mosaics, glass beads, &c. A similar discovery was made at the same spot some years ago.

THE "Year-Book of Photography" for 1882, edited by Mr. Baden Pritchard, contains a great deal of information that must be useful to those interested in photography. It contains a fine portrait of the late Mungo Ponton. Piper and Carter are the publishers.

WE have received the first part of a new monthly German journal of science—*Humboldt*—devoted to natural science generally. The contents are very varied and the style on the whole popular, with many illustrations. There is a long list given of eminent contributors. Enke, of Stuttgart, is the publisher.

ON May 15 next an exhibition of minerals and objects illustrating ceramics and the manufacture of glass will be opened at Madrid.

A SPECIAL despatch has been received at St. Petersburg from M. Sullowsky, dated Irkutsk, December 26 (O.S.) 1881, which says: "At 10 o'clock on the morning of August 9 I parted with the *William Rodgers*, which shaped her course for Herald Island. The clipper *Strelok* then returned to the Chinese ports. Up to that time the *Strelok* and *William Rodgers* had kept company. They were joined in Providence Bay by an American schooner, having on board the captain of a whaler which had stranded. This captain narrated that he had seen a boat with dead men on board which had been driven upon Herald Island. The boat also contained, besides other articles, some silver spoons with the name *Jeannette* engraved on them. In consequence of this narrative the captain of the *William Rodgers* resolved to proceed to Herald Island with the view of wintering there, and, with the aid of dogs purchased in Kamschatka, sending out his crew in small parties to the various sides of the island and its vicinity to search for the lost explorers."

THE Geographical Society have issued (through Stanford) the first part, of seven sheets, of the large-scale map of East Central Africa, by Mr. E. G. Ravenstein, which we have referred to as in preparation. The map is on the scale of fifteen miles to an inch, extends from 10° N. to 20° S. and lat. of 25°. It thus includes an extensive area of great interest, and is on a scale to show all the leading features in detail so far as they have been discovered. Mr. Ravenstein has collected in his map a vast amount of information which could be obtained only by consulting many books, so that it will form a library in itself. The routes of all explorers are shown, and abundant notes are laid down as to the nature of the country, ethnology, doubtful points, &c. It gives evidence of conscientious, painstaking, and wide research. To all interested in marking the progress of African exploration it will prove of great utility.

ADMIRAL MOUCHEZ will give his usual annual *soirée* at the Paris Observatory in March. He has distributed to the leading Parisian engineering firms the conditions for the construction of the cupola for the great equatorial to be built in the newly-annexed grounds. The diameter of the revolving cupola is to be 20 metres. The form must be hemispherical. The time required for rapid revolution is 10 minutes. It is to revolve in the same direction as the heavens, and the mechanism will cause the revolution of a seat for two astronomers. The dimensions of the moving platform are 1 metre by 2. The competitors are to employ either a falling weight or a gas engine as motor. In this case the motor must be placed at a distance outside.

THE French Government is busy preparing a large number of new bills which will be laid before both Houses when the Parliamentary recess is over. One of these relates to the the use of the surplus gained by the Electrical Exhibition and the other to the telegraphic network.

PROF. J. G. MCKENDRICK, the new Fullerian Professor of Physiology at the Royal Institute, will give the first of a course of eleven lectures on the Mechanism of the Senses on Tuesday next (January 17); Mr. H. N. Moseley will give the first of a course of four lectures on Corals on Thursday (January 19); and Prof. E. Pauer will give the first of a course of four lectures on Ludwig van Beethoven (with musical illustrations) on Saturday (January 21). The first Friday evening discourse will be given by Dr. Huggins, on Comets, on January 20.

To the *British Trade Journal* for January, Dr. James Geikie contributes an interesting article on "The Gulf Stream and the Panama Canal," in which he concludes that the opening of the Canal "will have as much effect on the Gulf-Stream and the climate of Northern Europe as the emptying of a teapot-ful of boiling water into the Arctic Ocean would have in raising the annual temperature of Greenland."

AMONG the sixpenny popular editions issued by Messrs. Longman and Co. is an abridgement of the Rev. J. G. Wood's "Homes Without Hands," under the title of "Strange Dwellings; being a Description of the Habitation of Animals."

THE Austrian Minister for Public Instruction has ordered a colossal statue of the late Austrian Arctic explorer and discoverer of Franz-Josef Land, Carl Weyprecht. The statue will be executed by the celebrated sculptor, Victor Tilgner, of Vienna, in Laas marble.

M. PAUL BERT has filled an important lacuna in the organisation of the French system of public instruction in Algiers. He has authorised the École Supérieure of Letters in Algiers to grant honours in Arabic literature, after candidates have passed through a special examination.

RUSSIAN papers state that on December 22, 1881, at 11.20 p.m., a meteor, spreading an intense violet light and the fall of which was accompanied by a strong explosion, was seen at Byejetsk, in the government of Tver, and at the village Nasilovo, twenty-seven miles distant from that town.

THE detailed report of Prof. Sorokin to the Kazan University on the downs of Kara-koum in Russian Turkestan, has appeared in Russian as a separate volume, under the title of "Travel in Central Asia in 1878-79."

A SHOCK of earthquake was felt at Batoum (Caucasus) on December 28, 1881, at 6.33 p.m. It lasted for about ten seconds, and was accompanied by a loud underground noise.

EXPERIMENTS in the culture of the Chinese Soja bean (*Soja hispida*), which gave good results in Vienna when Prof. Haberlandt made use of seeds received from Northern China and Mongolia, have been repeated in Russia, and so far have been quite successful in Kieff, Saratoff, and the Crimea, but they have failed in the western provinces of Russia.

ON December 22, 1881, Tiflis was covered with a sheet of snow ten inches deep. Snow is a very rare occurrence in this town, and its appearance seems the more strange as there has been no snow in all Central Russia.

A VALUABLE discovery has been made in a quarry at Dillingen, near Saarlouis. Some workmen found in a small cavity a bronze vessel containing gold and silver ornaments of partly beautiful and partly very coarse workmanship. Amongst them is a golden disc of 8 centimetres in diameter richly covered with rubies, emeralds, and filigree work; also a silver object weighing over 150 grammes, and bearing inscriptions in Latin, Greek, and unknown characters. The proprietor of the quarry will present the objects found to the Bavarian National Museum.

NEAR Caltanisetta, Sicily, a series of caverns have been discovered, which are evidently burial-places dating from the period when the ancient Sicilians had already been ousted by the Italian tribes, but before the Greek colonisation had begun. Their arrangement is similar to the tombs at Pantelica, Acri, and Girgenti. In the neighbourhood of the caverns are numerous remains of ancient buildings and other proofs of the existence of an ancient populous colony. The spot derives its name from the hill of Gibil Gaib.

PHYLLOXERA seems to have abandoned the vineyards of Lombardy and Liguria, but has appeared in other parts of Italy, viz. Elba, Sardinia, &c. In Sicily the plague is making rapid progress; the districts of Messina and Caltanisetta being particularly unfortunate.

M. SYNGROS, the Athens banker who gave 100,000 francs for the erection of an archæological museum at Olympia, has again given a like sum to facilitate the progress of the work.

AN earthquake is reported from Honolulu on September 30, 1881. It consisted of one very violent and two lighter shocks. The first was felt at 4.53 a.m. lasting thirty seconds, and accompanied by loud subterranean rumbling; direction south-east to north-west. The crater of Kilauea was very active at the same time. Numerous houses were more or less injured. The phenomenon was also observed on all the other islands of the Hawaiian Archipelago. An earthquake is also reported from Eastern Galizia on December 29, 1881; several shocks lasting a few seconds. A sharp and unusually sustained shock of earthquake passed along the east coast of India on the morning of the 31st ult. At Calcutta it lasted about two minutes. It was especially severe in Madras, where, judging from the space devoted to it by the local papers, it would appear to have caused much alarm. No damage is, however, reported.

THE English representative of the company which manufactures the Griscom motors, mentioned in our article last week, writes us to the effect that the small form of motor there described will attain a speed of 3000 revolutions per minute when worked with a 6-cell bichromate battery, and will accomplish 1000 foot-pounds of work per minute; and that with a dynamo-current this limit is far exceeded. We are also informed that the Company has established a branch in London for the direct supply of their motors to the public.

DR. WOEIKOFF asks us to correct an error in the abstract of his paper on the freezing on a salt lake (NATURE, vol. xxv. p. 206). It is there stated that "it was never observed before in laboratories that salt water was cooled below -4° without being frozen, and here we have salt water remaining unfrozen at -13° below zero." In the paper referred to Dr. Woeikoff simply stated that temperatures below -4° C. were not before observed in saline solutions outside of laboratories, while here we have temperatures of -13° observed in a salt lake.

THE additions to the Zoological Society's Gardens during the past week include a Crab-eating Raccoon (*Procyon cancrivorus*) from South America, presented by Mr. H. B. Whitmarsh; two Pileated Jays (*Cyanocorax pileatus*) from La Plata, presented by Mr. C. S. Barnes; two Lesser Black-backed Gulls (*Larus fuscus*), British, presented by Mrs. Brindley; a Speckled Terrapin (*Clemmys guttata*) from Massachusetts, three Confluent Rattlesnakes (*Crotalus confluentus*) from Dakota, U.S., presented by Mr. W. A. Forbes, F.Z.S.; a Blue-eyed Cockatoo (*Cacatua opthalmica*) from the Solomon Islands, a Short-eared Owl (*Asio brachyotus*), British, deposited; six Grey Squirrels (*Sciurus cinereus* var. *nigra*) from North America, purchased; a Gayal (*Bibos frontalis*), born in the Gardens.

THE SWISS SEISMOLOGICAL COMMISSION

THE Seismological Commission of Switzerland, after having published in French and German an excellent text-book on earthquakes, by Prof. A. Heim, and after having widely circulated its queries on earthquakes, has received a great mass of information which is mentioned in high terms both as to their accuracy and interest. Prof. A. Heim, availing himself of this material, has already published in the *Annuaire* of the Physical Observatory at Bern, an interesting monograph on the earthquakes of last year, and now M. Forel, of Morges, also publishes in the *Archives des Sciences Naturelles* of Geneva a first paper on earthquakes for the first thirteen months of the existence of the Commission from December, 1879, to January, 1881. We see, from a list of earthquakes during the years 1876 to 1880, which he publishes, that there were in Switzerland during this period of time, no less than forty-eight earthquakes, of which twenty-five were in 1880, the Commission having received accurate information on twenty-one of them, and four feeble shocks more having been reported in newspapers, but they still are rather doubtful. This increase of earthquakes in 1880 must, however, be to a certain degree the result of more accurate observation since the appointment of a special Commission for that purpose. The chief earthquakes during these thirteen months—December, 1879, to January, 1880—were the following:—On December 4 and 5, 1879, consisting of three main shocks and of seven feebler ones. The first and the third of the main shocks had each an extent of about 100 miles, and the aggregate area shaken by these earthquakes had a length of 250 miles and a width of 40 miles, its longer axis being parallel to the main chain of the Alps; the centres of the successive shocks advanced from south-west to north-east.

The earthquake of December 29 to 31, 1879, had a great extension. To use M. Forel's expression, "this beautiful earthquake" consisted of three chief shocks and of a dozen smaller ones. The first strong shock was experienced on an area limited by Lyons, Locle, Solothurn, Luzern, Sion, Chamonix, and Annecy, affording thus an ellipse 200 miles long and 100 miles wide, the great axis of which also was parallel to the main chain of the Alps. Its centre was between the Arve and Drarse Rivers, and its intensity at Geneva reached seven degrees of the decimal scale proposed by M. Forel. The shock propagated itself by several oscillations, at a speed of 300 to 400 metres per second. The following main shocks had a smaller area, but the centre did not advance along the axis of the shaken area; it remained in the neighbourhood of Geneva. It is rather remarkable that at the same moment as Savoy and Western Switzerland experienced this earthquake, another series of feeble shocks was felt at Niederaach in Thurgau, both earthquakes being separated by a zone 160 miles wide, where no shocks were observed.

The earthquake of July 3 to 5, 1880, extended throughout the whole of Switzerland, reaching also the southern parts of the Grand Duchy of Baden and Northern Piemont, and shaking an area 203 miles long and 187 miles wide. It was much complicated, two strong shocks having been felt almost in all Switzerland, whilst many other feebler shocks, about twenty in number, which preceded and followed the main ones, had a merely local extension. Prof. Heim shows that in this earthquake there was no central point from which the shocks might have been transmitted in all directions; and he thinks therefore that there was a general dislocation of strata on a very wide surface, rather than any shock departing from any determined point of the territory.

After further details, M. Forel tries to classify the earthquakes with relation to the seasons, and to the position of the moon; but we will not follow him in these researches, as he himself states that a thirteen months' period of observations is too short a time for such generalisations. But we may notice the circumstance that, whilst in some earthquakes the shock is propagated from a centre to the circumference, in others all the surface of the country seems to be pushed in one general direction; Prof. Heim discovers this character in the earthquakes of December 5, 1879, and of July 4, 1880. The importance of this remark will not escape the attention of those who are eagaged in the study of the formation of mountain ridges.

It is obvious that the Commission met with several difficulties in performing their task, and the chief are in the notation of the time of the earthquake and of its direction. Humanity seems to be, even in the fatherland of clockwork, very far from knowing the true time, and even the clocks of the towns, of the railway and telegraph stations, seem to leave very much to desire as to the accuracy of the information they give us. Some improvement, however, is shown in that direction during this last year, and it may happen that the desire of making accurate observations on earthquakes will give an impulse to some improvement in our knowledge of time. As to the direction of earthquakes, there are yet more difficulties, and M. Forel points out the interesting circumstance that nearly all information as to the direction of earthquakes is influenced by the orientation of streets, the direction of earthquakes nearly always being given either parallel or perpendicular to the observer's street. On some occasions, as in the earthquake of July 28, 1881, everything on the surface of the soil seems to be in a vibratory motion, as grains of sand on the surface of a vibrating slab, and the shocks are observed in all possible directions as well vertical as horizontal.

But it is not only from Switzerland that the Seismical Commission has received valuable information, and we find in the *Archives* two interesting papers on the earthquakes of the island of Chio, by M. Arland, and on those in Asia Minor, by M. van Lennep. As to the former, we notice that the volcanic eruptions on the island of Nisyros had ceased a month before the catastrophe, and that they have not begun again up to the present. The oscillations of April 3 seem to have had an amplitude of 15 degrees, and from April 3 to April 7, there were counted no less than 250 shocks, of which 30 to 40 were very strong. On April 11, at 7 p.m., there were the well-known great shocks which occasioned such a panic. They continued until the month of August, being followed by a standstill from August 1 to August 25. On this day and the following there were again strong shocks. From a complete list of houses destroyed, published by M. Arland, we see that there were no less than 6730, and that the number of killed and wounded was, in various villages, as much as 10 to 30, and even 36 per cent. of the population. M. Arland gives also some interesting notices as to the direction of the shocks in Calimassia, and to the disturbances they have done to the walls of the houses.

SOLAR PHYSICS[1]

III.

YOU will recollect that in my last lecture I explained to you the special absorptions that were due to certain hydrocarbons. To-day I wish to tell you some of the applications that can be made from an investigation and examination of this series. Before I proceed farther I wish to show you not only that there are absorptions which are due to the vibration of the atoms of these hydro-carbon molecules, but also that there is a general absorption which seems due to the molecular vibration of these same hydro-carbons.

I wish to point out to you the general absorptions of the first three of those spectra, the alcohol series. The first one is methyl alochol which has a light molecule, or rather, I may say that it is lighter than the molecule of the next, common alcohol which in its turn again is lighter than propyl alcohol. You will see that the general absorption creeps up from the ultra-red towards the red. In each case the lighter the molecule the more vigorous is the general absorption. I was obliged to refer to this, because through the course of my lecture you will see that this has an important bearing on some of the results which we got.

Another point is the effect of heat upon this general absorption; and here I speak with a certain amount of diffidence. My belief is that the general absorption is increased by an increase of temperature in the hydro-carbons, in water, or in any of those liquids which we examined. Thus, with water, we find that the general absorption creeps up, the hotter the water is. In fact, at this time of the year when the temperature is high, it is very hard indeed to get some of those special absorptions very far down in the ultra-red, because they are blocked out, as it were, by the general absorption which is due to the vibrating molecules—molecules of course vibrating in the manner which I have described.

The hydro-carbons which we have examined were all in the liquid state, and it seems very probable—nay, more than probable—almost certain,—that if you were to convert those liquid hydro-carbons into gas you would get their absorptions in the same localities. For instance, in the diagram (Fig. 14) I will

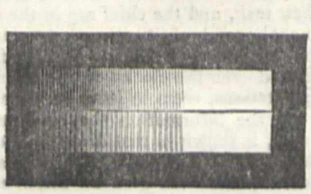


FIG. 14.—Absorption spectra of liquid and gaseous bromine.

show you the absorption spectrum of bromine. The top spectrum is the absorption spectrum of liquid bromine, and the bottom spectrum is the spectrum of gaseous bromine. You will see that the difference between the two is that, where you have the general absorption in the liquid, it is split up into lines in the case of the vapour. Thus in the case of the liquid we have the blue end of the spectrum entirely cut off by a general absorption gradually increasing from the blue to the green. When we pass to the gaseous bromine we have the same absorption but terminated with bands. I do not say that this is always the case in bodies which have the same chemical constituents.

Now, so far having cleared the ground for our next point, I must refer you back to the map of the infra-red of the solar spectrum which I alluded to last time. After we had completed our map of the ultra-red absorption spectra of these different hydro-carbons, we had the curiosity to look at the map of the solar spectrum to see whether we could discover anything at all in relation between the two. Col. Festing and myself were very surprised to find that we had mapped lines identical with lines in the ultra-red region of the solar spectrum. Thus, for instance, when we looked at the spectrum of water—we found that one line lay in the little *a* group, and had a wave length of 833. When we examined chloroform, we also found that it had a great many lines which we suspect are to be found in the solar spectrum. We then went to ethyl iodide, and here we also found coincidences; but the coincidences were more marked than in the former case. For instance, last time I told you that, in order to identify the base or radical band of any particular hydrocarbon, it was necessary to look for the base or radical, and, if need be, to confirm it by looking at other bands which were situated somewhere about the little *a* in the solar spectrum. In the case of ethyl iodide we found that the band about little *a* coincided with part of the band in the solar spectrum. We also found that all of the alcohol series had one strong line coincident with a solar line; other lines were also to be found coincident with other lines of the solar spectrum. Furthermore the relative intensities of the lines in the liquid and solar absorption spectra seemed identical.

[1] Lecture delivered on May 25, 1881, at the Lecture Theatre, South Kensington Museum, by Capt. Abney, R.E., F.R.S. Continued from p. 191.

What, then, was the conclusion which we had to draw from this? It was that the lines in the solar spectrum must have something to do, or must have some connection at all events, with the lines found in the hydro-carbon series.

Now, as I have said, where you have a liquid you need not expect the spectrum to be so minutely covered with lines as you would if you had the body in the gaseous state, and, in all probability, suppose that we could have ethyl iodide in a gaseous state which is a thing not impossible to have—(it is very easy, only it is difficult to get enough of it)—those radical bands would probably split up into fine lines, and from the preliminary experiments which we have made it seems as if that would really be the case. But as this of course is a work of only a few months old, this requires confirmation; and Col. Festing and myself propose to carry out with vapour what we have already carried out with liquids. In our own minds there is no doubt that some at least of the hydro-carbons we have examined, are to be found in the solar spectrum, and we are also inclined to think that the hydro-carbons are not combined with oxygen, because, were they combined with oxygen, we should expect to find a more complicated spectrum of these particular lines or bands in the solar spectrum.

Now suppose that we have such a compound as an ethyl compound of some description in the solar spectrum. Where can such a compound exist? It must exist in one of two places. It must exist near the sun, or it must exist in our own atmosphere. Now the nearer you go to the sun of course the hotter the region, and it is quite impossible that these hydrocarbons, whatever they may be, should exist very near to the solar disc or to the nucleus. But if they do exist in the solar atmosphere at all they must exist at some little distance from them in a cooler region. You will recollect that Mr. Lockyer pointed out very clearly that there are regions around the solar nucleus which have vastly different temperatures.

Now the question is, what compounds could exist even at a comparatively low temperature. There is a gaseous body allied to the ethyl series which would have the same radical or base bands, namely, acetylene. Acetylene can exist at a high temperature; it has been found in the cooler part of the arc of the electric light, but we also know that three of its molecules will combine to form benzine. Let us see what we should get, suppose that we have a ring of this hydro-carbon outside the sun; and then let us see what evidence would prove the presence of this hydro-carbon. I have here a small diagram showing what we

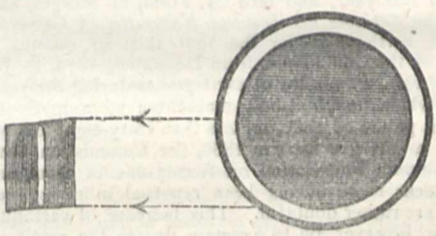


FIG. 15.—Solar disc surrounded with a hydrocarbon ring.

might expect to see. The black disc is supposed to be the solar nucleus, and this black ring which you see round it is supposed to be the ring of a hydro-carbon. The slit of the spectroscope is also shown. Now, if it exists at some little distance from the solar nucleus, you will see that when the light passes from the edge of the sun through that ring and then through the slit of the spectroscope, it will pass through a deeper layer of matter than it would supposing the light came from the centre. In some photographs I took of the solar spectrum some time ago, before we made these experiments, I found that some of the lines were more broadened at the limb of the sun than were other lines; and these lines apparently are coincident with these hydro-carbon lines. So, you see, from an examination of the hydro-carbons and an examination of the sun's limb, we may be able to form an idea as to whether such hydro-carbons are present in the atmosphere of the sun. Of course, as I said before, this work is new and will require a great deal of confirmation before it can be put forward as an absolute certainty. Mr. Lockyer suggested another way in which this broadening of the lines of the limb might arise. Supposing the corona were to be composed of hydro-carbon, and also supposing we got a streamlet or streamer of the corona lying in the same line to the earth as the limb of the sun; then we should get exactly the same result. Studying the diagrams, it is quite evident that if it were composed of hydro-carbon we might get a thickening of the hydro-carbon lines at the limb, because it would pass through a greater quantity of matter than at the centre.

Secondly, these hydro-carbons might exist in our atmosphere; but then if they did exist there, there is a very easy way of

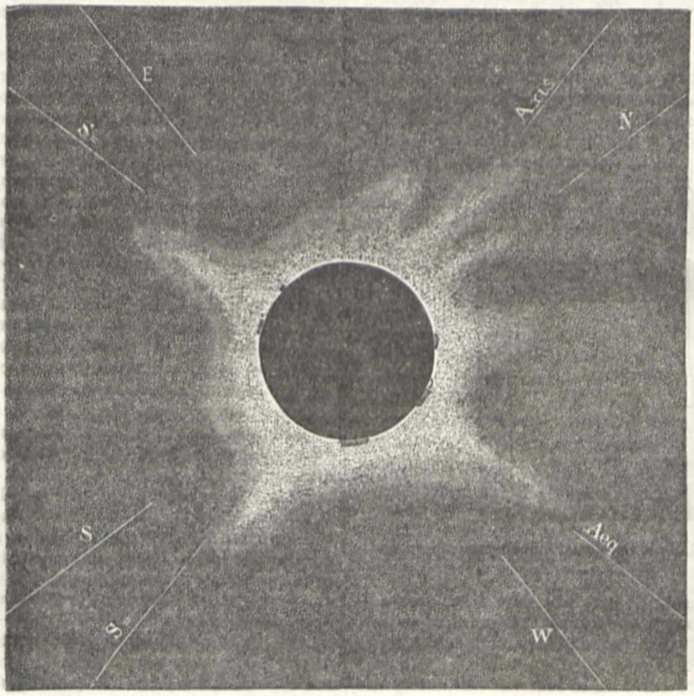


FIG. 16.—Solar corona, as seen during an eclipse.

proving whether such is the case. If we photograph the solar spectrum when the sun is very high in the heavens, and photograph it also when the sun is very low, we pass through a very much larger quantity of atmosphere in the second case than we do in the first case. Now I have never noticed that there has been any particular absorption with a low sun, except, it may be perhaps, a greater general absorption on the more refrangible side of the dark *Z* line. But this, of course, like the rest of the work, requires still further examination, and I am only, as it were, throwing out hints as to the work that will have to be done in this particular region.

But now about aqueous vapour. In 1860 Brewster and Gladstone published a map of the spectrum, showing atmospheric absorption, and this investigation was continued by an eminent French physicist, M. Janssen, in 1864. Brewster and Gladstone had shown that certain lines were due to atmospheric absorption; and Janssen went farther and tried to confirm their researches, or to correct them. He lit a bonfire near Geneva, and observed the spectrum of the bonfire thirteen miles off, and found that there were a great many lines in the spectrum when he observed it thirteen miles off, which were not present when he observed it near; and these could only be due to atmospheric absorption. But that did not settle the point whether the lines that he noticed were due to the air or to aqueous vapour. In order to ascertain which lines were due to aqueous vapour, in 1860, he filled an iron cylinder 330 feet long with steam, and then observed the absorption spectrum of steam through this great length of tube, and he noted that certain lines were visible in the spectrum which were also visible in the spectrum which he obtained by viewing the bonfire thirteen miles off. As a further confirmation, he appealed to the stars. He observed Sirius, for instance, at a high altitude, and also at a lower altitude, and found that there was a darkening of some of the Fraunhoferic lines. Whether this was due to aqueous

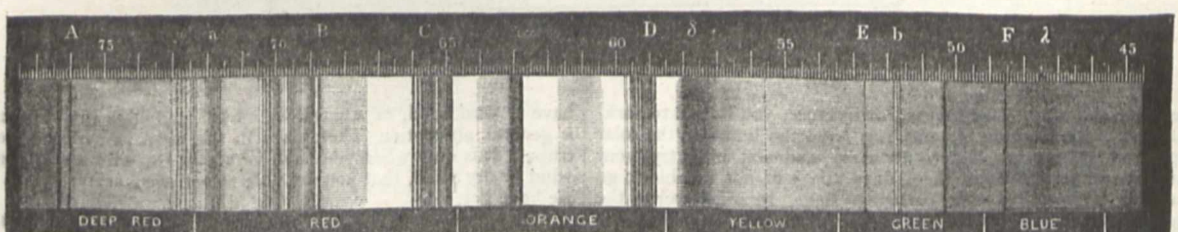


FIG. 17.—Angström's map, showing telluric lines.

vapour or to the air lines, of course could only be settled by reference to the absorption spectrum which he obtained from viewing a candle flame through the 330 feet of aqueous vapour. That distinguished man, Ångström also examined into the subject, and in his map are shown the lines which he considered to be due to the atmosphere.

Now from very careful examination, he considered that neither the A line nor the B line was due to aqueous vapour, but he thought they might be due to carbonic acid. From what we have observed, it seems highly probable that they are not due to carbonic acid because, if we had a compound of carbon and oxygen, we should not expect to see such a spectrum at all. I think that from the context we shall see a great probability as to what they may be due. Other observers observed the spectrum of the atmospheric lines, but perhaps the most recent and trustworthy observer is Prof. Piazzi Smyth. He published in the "Edinburgh Astronomical Observations," vol. xiv., a memoir regarding these black lines. Regarding B he says that they are merely dry air lines, and that the A line is a solar line. I should like to quote his own words with regard to this particular A line. He says, "The remarkable clearness, plainness, and strength of the lines composing A and its parallel bands in a high sun tend to claim the said A and its appendages a solar line, and, if so, it is a grander one by far than any other solar line in the whole of the solar spectrum. If, on the other hand, some observers will claim A as only a gas effect, Ångström has said that

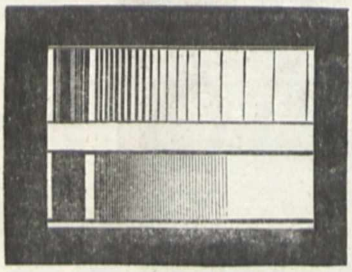


Fig. 18.—Top spectrum the A line; bottom spectrum the benzine radical band.

A grows or reinforces at sunset. I may say that I have seen it this summer (1877) in Edinburgh almost at the point of sunset, and ascertained with a more powerful prism, that, though heavily black as it was, it hardly was in reality grown at all." He says that the time was too short for him to measure absolutely the number of lines, but his impression was that the number of lines composing the A band was not diminished at all, and that the A line presents the same appearance at sunset as it did in a high sun.

Now, as I said at the beginning of my lecture, we must take into consideration the general absorption which is always more or less visible in the spectrum, and at sunset we always have greater general absorption than we have of course when the sun is at midday, because the stratum of air between us and the sun is greater in the one case than it is in the other. Smyth, I believe, supposed that the A line was due to hydro-carbon, and I should like to show you that, probably, his surmise is correct. [I subsequently found that this idea did not originate with the Professor.] An examination will show you how very remarkably like the A group of lines is, to this B group of lines. They both have a band—a common band which, with a very high magnifying power, is splits up into fine lines. From analogy, one would suppose, then, that the A and B lines are due to the same substance. If one looks at the A line with a small dispersion, one sees it very much of the same form as the benzine radical band that you saw (Fig. 18). It appears to have a dark distinct band, and then to shade off for some considerable distance. Now the remarkable thing about this is that if I transferred this A line such as we see here to about a thousand wave lengths down the spectrum, we should get the characteristic radical band of benzine. In our comparison of the solar spectrum with that of benzine we have found unmistakable proofs of the existence of benzine in the solar atmosphere. This being the case, the big A line and the benzine, or the benzine series may have some connection one with the other; and it seems to me from analogy that sooner or later the A line will be found to be due to a hydro-carbon of some kind. You recollect in the photograph I showed you last time, that as the molecule got lighter the radical band went up towards the blue. Now the benzine has essentially a comparatively speaking heavy molecule. It is composed of 6 of hydrogen and 6 of carbon, and therefore if we can examine a hydro-carbon, which has the same proportions of hydrogen to carbon, but of a different molecular grouping, I think it extremely probable that eventually we shall find that the A line belongs to that particular group of the hydrocarbons in which there are equal atoms of hydrogen and carbon. As to the B line the question is, can the B line, which has a similar look to the A line be due to a hydro-carbon. That requires great consideration. Every observer, I believe, has put B down as an atmospheric line, and therefore if such be the case we shall have to look for a hydro-carbon high up in our atmosphere.

Now, to come back, what would be the effect of aqueous vapour? First of all, we should have a general absorption in the ultra red part of the spectrum, and then also a special absorption. The annexed diagram (Fig. 19) is from a photograph of the solar spectrum taken through various thicknesses of water, and you will see what kind of effect the various thicknesses have upon absorption

This bottom figure is the spectrum through three feet of water. The next part of the spectrum is photographed through one foot of water; and the next part of the spectrum is photographed through six inches of water. You will see what a remarkably little absorption there is of the visible spectrum due to a certain amount of water. Now, can water have the same effect as aqueous vapour? I think it can. If M. Janssen had condensed the steam in the 330 feet tube, and only had a thickness of somewhere about three inches of water to look through, then looking through three inches of water he would have seen no absorption whatever. In other words, where you have a liquid converted into a vapour a given weight of it seems to absorb a great deal more as vapour than it does as liquid. Sometimes we see this even in the visible part of the spectrum.

When you have a cold north-east wind, you have an atmosphere very free from moisture, and you are able to go down in the infra red regions very far; but when you have a change of

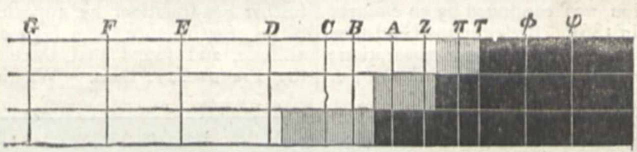


Fig. 19.—Absorption spectra of different thicknesses of water.

weather and a south-west wind, for instance, you have a remarkable absorption setting in, and you have the limits of the solar spectrum ending somewhere about τ, and you may try your utmost and yet on one of these days you will not be able to go one jot farther than that particular line. In other words, the general absorption due to atmospheric water in some form or another comes in, and not special absorption.

In the diagram, Fig. 7 (see p. 188), we have two bands, ϕ and ψ, at the extreme right of the ultra red, and only on two days in two years have I ever been able to obtain a photograph of those particular bands. They were obtained in both cases in March, and it was when the air was particularly free from aqueous vapours, and it was very cold. To sum up, where you have a west wind, or a south-west wind and high temperature, a general absorption takes place in the ultra part of the spectrum. It is only when you have very cold air, with a north-east wind or dry wind, that you are able to get even as far down as we have got.

The next point to which I wish to call your attention is, Is it probable that in the ultra red part of the spectrum we should ever be able to obtain evidences of metallic lines, or lines which are due to metallic vapours? I think that it is highly improbable that we shall. The few metals that we have tried have given almost a negative result except in one or two cases. Thus in looking through the list of lines due to the metallic elements, there are very few that go even as far as 700 in the scale of wave-lengths.

They generally end somewhere about 650 wave lengths. Of course there is one well known exception, which is the potassium line, which has the wave-length of 770 (in fact below the A line), and is visible with a certain amount of difficulty. But as a rule all the bright metallic lines end above 700, and when they get towards 700 they are always thin lines and poor lines. When you come to think of it, it seems highly improbable that you should obtain lines very low down except in the case of metals of low fusing point. If you have a metal of low fusing point, of course it is much more likely that you get lines of low refrangibility than you would if the metals have a high fusing point, and as a fact, taking a metal which has got a high fusing point such as iron, you find no line in the ultra red part of the spectrum, whereas in the case of sodium, which has a low fusing point, we do find a pair of solitary lines about W L 840.

When you heat platinum wire by a current of electricity, at first though it may be hot to touch, it remains dark. Then, as you increase the current, it gets red and hotter still, and new waves —green—put in an appearance; and finally we get white light. At 550 degrees centigrade the body shows redness. At a white heat we have the whole of the visible spectrum present. Whether all the waves exist at ordinary temperatures in the platinum wire is a matter for future consideration. It is, I think, possible that such may be the case, provided the amplitude be very small indeed. At all events, the molecules of the body on which the source of radiation falls must be in a state ready to vibrate with the higher wave-length. Each wave as it puts in its appearance has a certain amount of energy, and a comparison between the energy of the two waves may be shown by photography as well as by their heating effect. But the heating effect is the true comparative measure of energy if the body on which it falls completely absorbs the radiations. Lamp black, perhaps, is the most perfect absorbent of all radiations, and the energy is shown by the heating effect on it. This heating effect in its turn is converted into an electric current by the use of a thermo-pile. Here is a thermo-pile which is capable of movement by a screw in any required direction, and we will take a very brilliant spectrum and cast it upon its face. It is connected with the galvanometer, and the galvanometer reflects a spot of light on this scale, and when a current passes that spot of light is deflected. You will see whereabouts it is. Now if I move the face of the pile gradually into the yellow I think you will find that it will move slightly up the scale. We will bring it more into the red, and if the galvanometer is in order you will see that it ought to be deflected still more. We have now got it in the infra-red part of the spectrum. The deflection is still greater.

It was by noting the deflections in somewhat this way that Dr. Tyndall was able to construct his spectrum monogram of the electric light, using as his mateiial prism rock salt. The limit of the red is shown by an arrow, on the left is the thermogram of the visible part of the spectrum, and on the right of the invisible part of the spectrum (Fig. 20). Now this thermogram

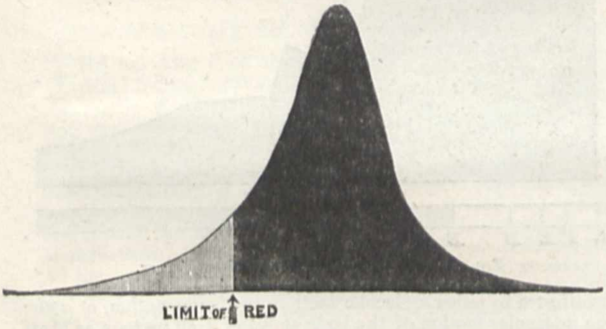


FIG. 20.—Tyndall's thermogram of the spectrum of the electric light.

is a puzzle, or rather was a puzzle, in a great many ways. For instance, if you know the wave-length of any two points you may find the theoretical limit of the spectrum by the method which I showed you in my last lecture. Unfortunately it lies well within the heat curve, or rather the effect of heat which is shown in the diagram.

When working it out in this way it will be found that what I may call the thin tail of the thermogram lies beyond the theoretical limit of the spectrum. But before I go any farther I want to show you a possible cause for this. This rock salt prism has been very finely polished by an optician, and I will mount it in the place of the bi-sulphide of carbon prism which we have so far employed. Now rock salt is supposed to allow the low radiations to pass through much more readily than a glass prism. We will try to get the spectrum tolerably pure. You will see that there is a fairly bright spectrum upon a piece of card.

Now in making observations with rock salt prisms, Col. Festing and myself were rather mortified to find that we got beyond the limits of the spectrum when employing photography.

Our curiosity was therefore raised, and we endeavoured to find out why this was the case. We therefore placed some bichromate of solution of potash in front of the slit of the spectroscope and observed the spectrum. I will repeat the experiment, and you see that all round the spectrum we have a wide spreading yellow halo which is due to the imperfection in the rock salt. The rock salt surfaces are as perfect as grinding can make them, but still there is a certain amount of diffused light which passes irregularly through the prism, and gives us that yellow halo. It is totally different as you will notice when you replace the bi-sulphide prism in position. You get a pure spectrum. You will see that each side the green and the red is tolerably sharp, and when you use a properly adjusted spectroscope the perfections, and for that matter the imperfections, are much more apparent than they are when making a lecture-table experiment. I have shown you this experiment that you may see with what caution measurements taken with rock salt should be received. I may say that we tried not only one prism but three or four, made out of different samples of rock salt, and all gave a like result. The only way we can use a rock salt prism when it is well ground is to allow an excessively narrow beam of light to pass through it. Directly any large surface of the prism (as is the case when a lantern, or condensing lens for condensing the beam upon the slit), is used, the action of diffused light at once renders the results liable to suspicion.

Now I will show you other figures obtained from a thermopile when using very delicate apparatus. I wish to show you how the thermo-pile and photography can work hand in hand. We have a thermogram taken with a glass prism, and you will see that it presents some features of similarity—not quite like Tyndall's thermogram. There is a reason for this difference, which is that the one is a thermogram of the positive pole of a powerful electric light, whereas I believe the other was

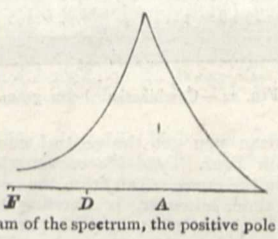


FIG. 21.—Thermogram of the spectrum, the positive pole of the electric light.

taken with the whole of the radiation coming from both of the poles and from a less powerful electric light. The negative pole of the light we used has been calculated to have approximately a temperature of 3,000°, whilst the positive pole approximately a temperature of about 4,000°. When using a source of one temperature, and that temperature of about 4,000°, you will see that the curve forms a cusp, that is at the place of maximum heating effect it comes very nearly to a point, and I believe that if we obtained a spectrum of a source of heat at a perfectly

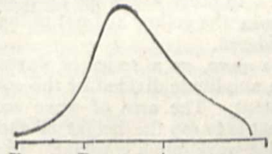


FIG. 22.—Energy curve of the same spectrum as Fig. 21, obtained by means of photography.

even temperature we should get that thermogram with an absolutely sharp point. By taking photographs we are able to check the results of the thermo-pile.

Fig. 22 is an energy curve as depicted by a photograph. You see it does not come to nearly such a sharp point as when a thermo-pile is used, and you ask why. The reason is that at that particular part the bromide of silver—blue bromide—does not entirely absorb the radiations, but allows a certain amount to pass through. Nevertheless you will see that there is a striking similarity between the two.

Now I wish to show you how you may combine the thermographs of two or more temperatures. I must, first of all, show

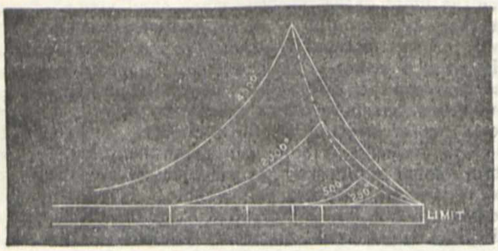


Fig. 23.

you the thermographs of varying temperatures (Fig. 23). We have a temperature of 250°. Next we have a temperature of 500°; next 2,000, next 4,000.

In diagram (Fig. 24) we have a combination between the thermograms of two temperatures, one of about 2,000°, and the other of about 4,000°. By measuring the height of these curves

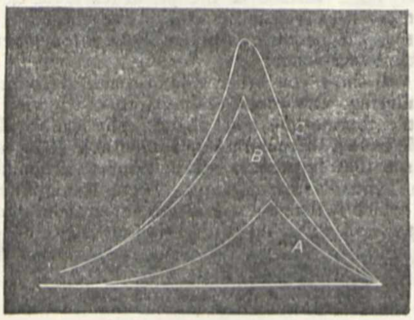


Fig. 24.—Combination thermogram.

and taking the mean you get the central curve. You should compare this with Prof. Tyndall's curve. You see it is not very different from the curve taken from the combination of the two curves. My time, however, is drawing to a close, and I am obliged to go but shortly through this part.

In my last lecture I showed you how the diffraction spectrum was spread out in the infra-red in comparison with the prismatic spectrum, and I think that it may interest you here to show you the way in which a thermogram of the solar spectrum is spread out when the prismatic thermograph is altered into a refraction thermograph or thermogram.

In Fig. 26 I give a diagram of a solar prismatic spectral thermogram (Fig. 25) as obtained by Lamarsky spread out into a diffraction curve. You see that instead of the maximum heating effect of the solar spectrum being beyond the red, it lies well between E and D. In other words the maximum energy of the solar spectrum lies in the yellow and not in the ultra red as has usually been considered.

The energy of a wave, or a series of waves, is measured by the square of the amplitude divided by the square of the wave length into a constant. The area of wave section is equal to the amplitude—that is to say the height of the wave multiplied by the length of the wave into a constant. If these waves have equal sectional areas, the energy varies inversely as the fourth power of the wave length. And what I wish to draw your attention to is this—that starting from the theoretical limit of the prismatic spectrum to the maximum heating effect of any continuous spectrum a law seems to hold that the energy of any portion of the spectrum below its point of maximum energy does vary inversely as the fourth power of the wave length.

I am sorry I have not time to go farther into the detail of this, but it has been the result of some considerable calculation, and experiment.

After my last lecture I was asked whether the photograph taken by the kettle in the manner explained was not due to the heat rays. I am afraid my reply was somewhat short as I said, "There are no such things as heat rays." I think that now may be an opportunity in which to express my views on the subject in a less curt manner than that in which I answered my questioner. It is true that we often do hear of dark heat rays and of radiant heat, and the rays which are principally concerned in the latter definition are taken to lie in the infra-red region of the spectrum. I would ask, "Why give them a name to which, it seems to me, exception can be justly taken?" In 1800, Sir William Herschel proved that these dark rays could be refracted

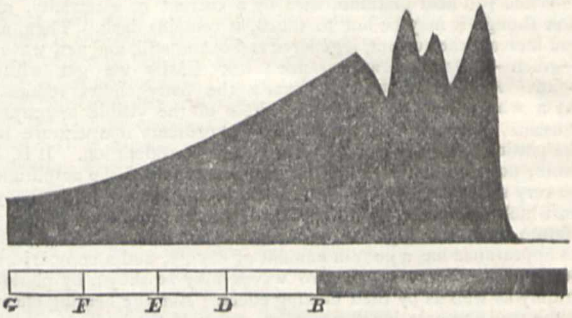


Fig. 25.—Prismatic thermogram of solar spectrum obtained by Lamansky.

and reflected like those rays which, falling on our retina, give us the sensation of colour. Professor Forbes, in his celebrated experiments, proved the same thing; but, in addition, he likewise proved that they could be polarised. I think I have laid before you proofs that these same rays can expend their energy in chemical action causing a disruption of a molecule by their successive impacts. Those rays, by whose agency we see, exercise the same functions as these dark rays. All rays are alike, and whether they cause a rise in temperature, or cause a chemical decomposition of a body, depends solely on the nature of that body on which they fall. The waves, as I have tried to demonstrate, carry energy and nothing else; and they must meet with some obstruction which shall destroy their motion before they can show that they possess energy. The work done by them is

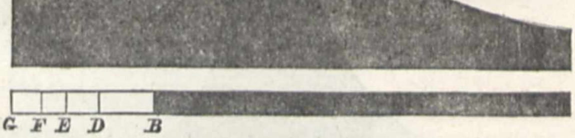


Fig. 26.—Diffraction thermogram from Fig. 25.

manifested by either molecular motion, or atomic motion, or both; the molecular motion of the body showing itself perhaps as heat, and the atomic motion as chemical action. If we must have the word "radiant" tacked on to a definition, (and the word "radiant" is a remnant of the corpuscular theory of heat,) all the wave motion in the ether should be classed under the head of "radiant energy." If a shorter nomenclature is required, let us simply call it "light," including in it the energy carried. Light is an old word understood in one sense by all, and we need only talk of the heating effect of light, and so on. The word "actinism" falls into an equal condemnation. We have unluckily none of our most eminent philosophers who are scientific photographists, if there were I do not believe any would defend the retention of the word "actinic" "or chemical rays" amongst our scientific terms. In the expression actinism and radiant heat, the cause has been mixed up with the effect. To be consistent, given one class of bodies the rays falling on it should be called actinic rays; whilst, given another, they should be called heat rays.

In 1840 Dr. F. W. Draper, of New York, clearly pointed out the identity in quality (if I may so call it) of the light, heat, and actinic rays, and that identity, I hold, has been confirmed more than once by recent investigators. I speak, perhaps, somewhat strongly on this point as no one knows better than myself the immeasurable mischief which a wrong definition causes in the progress of a scientific education. Men matured in science can afford to use any definition since they can carefully guard it by mental reservations as to what they really understand by it; but I hold it as a misfortune of no mean order that definitions, which are not so exact as our present state of knowledge can make them, should be given to the uninitiated whose reasoning powers must at the outset be feeble. A definition containing but half a truth must of necessity lead a student of science to a wrong conclusion at some time or another. If our writers of text-books could but be persuaded to write as they believe in this matter, and as some have written (for instance, Clerk Maxwell), we should have fewer mistakes made in explaining the ordinary phenomena met with in daily life.

I think I have now explained what I meant when the answer was given, "There are no such things as heat rays:" a source of energy may be darkly hot as was the kettle, some of the energy radiating from it was expended in heating bodies round it, but that portion which radiated through the holes perforated in the card and which struck the plate was, at all events, partially expended in converting the silver bromide into the sub-bromide.

In the course of these lectures, which I now finish, it has been my endeavour to show you the principles on which experiment in the infra-red region has been carried out, and also to point out the necessity for further work of no light kind in this part of the spectrum. The preceding lectures will also have shown you that work is required to investigate the visible part of the spectrum, and also in the observation of the various phenomena presenting themselves on the solar surface which must of necessity react upon our earth. It has been ignorantly said that the study of solar physics will be exhausted in ten or twelve years, but from what you have heard my colleagues tell you it will surely last our lifetime. If I live till the exhaustion takes place, my allotted threescore and ten years will, I should say, be greatly overstepped. I prophesy, though it can hardly with decency be called a prophecy, that many generations will pass away before all is known of the exact relationship between solar and terrestrial phenomena. What we do know already is hardly the alphabet of the language in which the sun addresses us, and until that alphabet is mastered the whole story that he would tell us must remain undeciphered.

MORPHOLOGY OF THE TEMNOPLEURIDÆ

THE following is an abstract of a communication read before the Linnean Society, Dec. 15, 1881:—The Temnopleuridæ, a sub-family of oligopores, are remarkable for their sutural grooves and depressions at the angles of the plates. The author examined the grooves and depressions or pits in *Salmacis sulcata*, Agass, and found that these last are continued into the test as flask shaped cavities sometimes continuous at their bases which are close to the inside of the test, but do not perforate. This is the case in the median vertical sutures of the interradium and ambulacrum. Between the interradium and the poriferous plates of the ambulacra are numerous pits in vertical series which are the ends of cylinders closed and often curved within. Altogether the undermining is considerable. The grooves over the sutural margins are losses to the thickness of the test. The edges of the contiguous plates are sutured together, by a multitude of knobs and sockets $\frac{1}{300}$ of an inch in diameter visible with a hand lens. In the vertical sutures there is an alternate development of knobs and sockets on each plate corresponding to a similar development on the opposed plates. Between the horizontal plate edges are sutures remarkable in their distinctness and position. The apical edges of the interradial plates have multitudes of sockets and the actinal edges, knobs: whilst the apical edges of the ambulacral plates have knobs and the actinal have sockets. The ambulacra, on their interradial edge have nothing but knobs and the interradial plates corresponding sockets, so that a great series of knobs and socket "dowelling" prevails. *Temnopleurus torematicus*, Agass., gave similar results modified by the great development of the grooves and the young form was shown to differ from the adult, and to have rows of knobs and sockets, and barely penetrating pits. The arrangement in *Salmacis bicolor* and *Amblypneustes ovum* was considered. The pits have an importance for they increase the superficies of the derm and near the peristome, as indicated by Lovén, they contain *Sphæridia*.

The paucity of knowledge respecting the union of the plates of the Echinoidea was noticed and the nature of the suturing of Echinus and *Diadema* was explained, the first resembling part of that of a young *Temnopleurus*, but it was without knobs and sockets. The author concluded by separating the Temnopleuridæ into two divisions, those with pits and those with grooves without pits. The last are the oldest in time and resemble young modern forms which subsequently develop pits. He reduced the number of genera considerably.

P. M. DUNCAN

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

WE are glad to learn that the number of students who have entered the Chemical Laboratory of Firth College, Sheffield, this session, has been so great, that the present accommodation has been quite insufficient. The Council, therefore, decided at their last meeting to erect working benches for sixteen more students. The University of Edinburgh have recently recognised Dr. Carnelly, Professor of Chemistry in Firth College, as a Teacher of Medicine in Sheffield, whose lectures on Chemistry, and course of instruction in Practical Chemistry shall qualify for graduates in Medicine in that University. The lectures on Chemistry and Laboratory Practice at Firth College have also been recognised by the Royal College of Surgeons and the Royal College of Physicians.

SCIENTIFIC SERIALS

American Journal of Science, October, 1881.—Cause of the arid climate of the western portion of the United States, by C. E. Dutton.—Embryonic forms of trilobites from the primordial rocks of Troy, N.Y., by S. W. Ford.—Observations of comet *b*, 1881, by E. S. Holden.—Thickness of the ice sheet at any latitude, by W. J. McGee.—Notes on earthquakes, by C. G. Rockwood. Marine fauna occupying the outer banks off the southern coast of New England, by A. E. Verrill.—Note on the tail of comet *b* 1881, by L. Boss.—Geological relations of the limestone belts of Westchester Co. New York, by J. D. Dana.

November, 1881.—Jurassic birds and their allies, by O. C. Marsh.—The remarkable aurora of September 12-13, 1881, by J. M. Schæberle.—The stereoscope and vision by optic divergence, by W. L. Stevens.—The electrical resistance and the coefficient of expansion of incandescent platinum, by E. L. Nichols. —Local subsidence produced by an ice-sheet, by W. J. McGee. —Notes on the Laramie group of Southern New Mexico, by J. J. Stevenson.—Polariscopic observations of comet *c* 1881, by A. W. Wright.—The relative accuracy of different methods of determining the solar parallax, by W. Harkness.—The nature of Cyathophycus, by C. D. Walcott.

Journal of the Franklin Institute, December, 1881.—Report of the committee on the precautions to be taken to obviate the dangers that may arise from electric lighting.—Report of committee on fire-escapes and elevators.—Chemical methods for analysing rail steel, by M. Troilius.—Notes on the properties of dynamo-electric machines, by E. Thomson.—Blast-furnace hearths and linings, by J. Birkinbine.—Sand-filtration at Berlin, by W. R. Nichols.—Report of committee on Griscom's electric motor.—Weighing the sun by a soap-bubble, by P. E. Chase.

Bulletin de l'Académie Royale des Sciences de Belgique, Nos. 9 and 10.—*Apropos* of determination of latitude, by M. Folie.— On the origin of Devonian limestones of Belgium, by M. Dupont.—Application of accidental images (second note), by M. Plateau.—A means of measuring the flexure of telescopes, by M. Rouzean.—On the micaceous substance of veins of Nil St. Vincent, by M. Renard.—Reports, &c.

Archives des Sciences Physiques et Naturelles, December, 1881. —International Geological Congress of Bologna, September and

October, 1880, by M. Renevier.—Meteorological *résumé* of the year 1880 for Geneva and Great St. Bernard, by M. Plantamour.—Periodical movements of the ground indicated by the air-bubble of spirit-levels, by the same.—On the movements of the ground, by Col. von Orff.

Reale Istituto Lombardo di Scienze e Lettere. Rendiconti, vol. xiv., fasc. xvii.—On recent discoveries of Silurian fossils in the province of Udine, by M. E. T. Taramelli.—Synthesis of β methylpyridine (β picoline), by G. Zanoni.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, December 15, 1881.—"On the Electromotive Properties of the Leaf of Dioneæ in the Excited and Unexcited States." By J. Burdon Sanderson, M.D., F.R.S., &c. (Abstract.)

The paper consists of five parts. Part I. is occupied by the examination of two experimental researches, relating to the subject, which have been published in Germany since the date of the author's first communication to the Royal Society, namely, that of Prof. Munk on Dionæa, and of Dr. Kunkel on electromotive action in the living organs of plants. According to Dr. Munk, the electric properties of the leaf may be explained on the theory that each cylindrical cell of its parenchyma is an electromotor, of which the middle is, in the unexcited state, negative to the ends, and that on excitation the electromotive forces of the cells of the upper layer undergo diminution, those of the lower layer an increase. He accounts for the diphasic character of the electrical disturbance which follows mechanical excitation by attributing it to the opposite electromotive reactions of the two layers of cells. According to this theory, each cell resembles in its properties the muscle-cylinder ("Untersuchungen," vol. i. p. 682, 1848) of du Bois-Reymond, differing from it in so far that its poles are positive instead of being negative to its equatorial zone.

Dr. Kunkel's experiments have for their purpose to show that all the electromotive phenomena of plants may be explained as consequences of the movement of water in the organs at the surfaces of which they manifest themselves. Neither of these theories is consistent with the author's observations.

Part II. contains a description of the apparatus and methods used in the present investigation.

In Part III. are given the experimental results relating to the electromotive properties of the leaf in the unexcited state, a subject of which the discussion was deferred in the paper communicated by the author (with Mr. Page) in 1876.[1] The fundamental fact relating to the distribution of electrical tension on the surface of the leaf when in the unexcited state is found to be that (whatever may be the previous electrical relation between the two surfaces) the upper surface becomes, after one or two excitations, negative to the under, and remains so for some time. Under the conditions stated, this difference of potential between the two surfaces occurs constantly; the differences of potential which present themselves when other points of the surface of the leaf are compared, may be explained as derived from, or dependent on, it.

Part IV. relates to the immediate electrical results of excitation, *i.e.* to the electrical phenomena of the excitatory process. In investigating these the author takes, as the point of departure, an experiment which includes and serves to explain those obtained by other methods, and is therefore termed the "fundamental experiment." It consists in measuring the successive differences of potential which present themselves between two opposite points on the upper and on the under surface of one lobe of the leaf, during periods which precede, include, and follow the moment at which the opposite lobe is mechanically or electrically excited. In this experiment it is found that, provided that the conditions are favourable to the vigour of the leaf, the changes in the electrical relations of the two surfaces (called the excitatory variation) occur in the following order:—

Before excitation (particularly if the leaf has been previously excited).	*Upper* surface negative to under.
At the moment of excitation.	Sudden negativity of *under* surface, attaining its maximum in about half a second, the difference amounting to not less than $\frac{1}{18}$ Daniell.
After excitation.	Rapidly increasing negativity of the *upper* surface, beginning 1·5″, and culminating about 3″ after excitation, and slowly subsiding.

[1] "On the Mechanical Effects and on the Electrical Disturbance consequent on Excitation, &c.," *Proceedings*, December 14, 1876.

This subsidence is not complete, for, as has been said, the lasting difference between the two surfaces is augmented—the upper surface becoming more negative after each excitation ("after-effect").

When by a similar method two points are taken for comparison on opposite lobes, the phenomena are more complicated, but admit of being explained as resulting from the more simple case above stated, in which only a few strata of cells are interposed between the leading off electrodes.

In Part V. the relation of the leaf to different modes of excitation is investigated. As regards electrical excitation the results are as follows:—If a voltaic current is led across one lobe by non-polarisable electrodes applied to opposite surfaces (the other lobe being led off as in the fundamental experiment) a response (excitatory variation) occurs at the moment that the current is closed, provided that the strength of the current is adequate, and not much more than adequate. No response occurs at breaking the current. When a current of more than adequate strength is used, and its direction is downwards, the response at closing is followed by several others. This effect does not happen when the current is directed upwards. To evoke a response a current must be much stronger if directed upwards than if directed downwards through the same electrodes. Weak currents cease to act when their duration is reduced to $\frac{1}{100}$″; for stronger ones the limit is shorter. Inadequate currents, if directed downwards, produce negativity of the upper surface, which lasts for several seconds after the current is broken. This effect is limited to the surfaces through which the current is led. Its direction shows it is not dependent on polarisation. By opening induction-currents, if their strength does not much exceed the limit of adequacy, a leaf may be excited at intervals for several hours without failure. Weaker currents are more effectual when directed downwards than when directed upwards. If two inadequate induction-currents follow one another at any interval less than 0″·4 and greater than 0″·02, they may evoke a response. In this case a response follows the second excitation. When a leaf is subjected to a series of induction currents at short intervals ($\frac{1}{20}$″) the response occurs after a greater or less number of excitations. If the temperature is gradually diminished the number is increased by each diminution. All of the above statements relating to excitability refer to plants kept in a moist atmosphere at 32—35° C.

From the preceding facts and others which are stated in the paper, the author infers (1) that the difference observed between different parts of the surface of the leaf are the expressions of electromotive forces which have their seat in the living protoplasm of the parenchyma cells. (2) That the second phase of the excitatory variation is probably dependent on the diminution of turgor of the excited cells, and therefore on the migration of liquid; (3) but that no such explanation can possibly be accepted of the phenomena of the first phase, the time relations of which, particularly its sudden accession and rapid propagation, show it to be the analogue of the "negative variation" or "action current" of animal physiology.

Zoological Society, January 3.—Prof. W. H. Flower, F.R.S., president, in the chair.—Mr. W. A. Forbes exhibited and made remarks on the horns of the Prong Buck (*Antilocapra americana*) lately shed by the specimen living in the Society's Gardens. This was, it is believed, the first instance on record of the same individual having shed its horns in captivity in two consecutive years.—A communication was read from Prof. Owen, C.B., on *Dinornis* (Part xxiii.), containing a description of *Dinornis parvus*, a new species of about the size of the Dodo, of which a very complete skeleton (now in the British Museum) had been lately discovered in a cavern in the province of Nelson, New Zealand.—A communication was read from M. L. Taczanowski, C.M.Z.S., containing an account of the birds collected by Mr. Stolzmann during his recent journey in North-Eastern Peru, with descriptions of some new species.—A communication was read from Mr. Martin Jacoby, containing the descriptions of three new genera and fourteen new species of Phytophagous Coleoptera from various localities.—Mr. Oldfield Thomas read a paper on the African Mungooses (*Herpestinæ*), in which he reduced the described species of this group to nineteen, divisible into seven genera.—The Rev. Canon Tristram read the description of a new species of Land-rail obtained at Ribè, East Africa, by Mr. R. C. Ramshaw, which was proposed to be named *Crex suahilensis*.—Mr. W. A. Forbes read a paper on the existence of a gall-bladder in, and on other points in the anatomy of, the Barbets and Toucans (*Capitonidæ*). The peculiar form of the gall-bladder in these birds, as well as other features in their myology now described for the first time, were stated to make the relationship of this group to the Woodpeckers (*Picidæ*) still more certain than it had previously been from the observations of Nitzsch, Kessler, Garrod, and others.

Meteorological Society, December 21, 1881.—Mr. G. J. Symons, F.R.S., President, in the chair.—The following were elected Fellows of the Society:—H. P. Bell, F. B. Edmonds, T. C. Evans, S. L. Fox, J. J. Gilbert, M. Henry, J. B. McCallum, J. Parry, and B. C. Wainwright.—The papers read were—The rainfall of Cherrapunji, by Prof. J. Eliot, M.A., F.M.S. Cherrapunji is notorious for its excessive rainfall, larger in amount it is believed, that any at other place, so far as is known. Cherrapunji is a small Indian station situated in the south-west of Assam, on a small plateau forming the summit of one of the spurs of the Khasia Hills. These hills rise on the south with exceeding abruptness, and have the Bengal plains and lowlands at their base. Cherrapunji stands on the summit of one of these hills, at an elevation of about 4100 feet. The hill on which it is situated rises precipitously from the lowlands of Cachar and Sylhet, which are barely 100 feet above sea-level. During the south-west monsoon the lower atmospheric current advancing across the coast of Bengal has a direction varying between south-south-west and south-east in Lower and Central Bengal. In thus advancing almost directly towards the hills of Western Assam, the mountain ranges cause a very considerable deflection of the current; one portion is forced upwards as an ascending current with a velocity directly dependent upon the strength of the current in the rear, and upon other conditions which need not be enumerated. The rapid diminution of temperature which accompanies expansion due to ascensional movement of air is usually followed by rapid condensation in the case of a moist current, such as the south-west monsoon current. The normal annual rainfall in Cachar and in the plains of Northern Bengal is about 100 inches. The average annual rainfall of Cherrapunji is 493 inches, that is, 393 inches in excess of that at the foot of the hills on which it is situated. The rainfall of Cherrapunji is not due to any abnormal local conditions of atmospheric pressure, air movement, &c., but simply and solely owing to the presence of a vast mechanical obstruction which converts horizontal air motion into vertical air motion.—On the meteorology of Cannes, France, by Dr. W. Marcet, F.R.S., F.M.S.. This is a discussion of the observations made at this celebrated health-resort during the six winter seasons ending 1880.—Report on the phenological observations, 1881, by the Rev. T. A. Preston, M.A., F.M.S.

Royal Microscopical Sociey, December 14, 1881.—Prof. P. Martin Duncan, F.R.S., president, in the chair.—Eight new Fellows were elected and nominated.—Mr. J. Deby exhibited his method of turning the correction-collar of objectives by a worm-wheel, acted upon by a tangent screw with a long arm, and Mr. Crisp exhibited Parkes' drawing-room microscope and two new homogeneous immersion fluids from Dr. van Heurck of Antwerp.—Mr. T. Charters White described a new growing slide devised by him, and Mr. Stephenson exhibited scales of *Machilis maritimus* and *Tomocerus plumbeus*, mounted in phosphorus under the binocular, with 1·25 inch objective, showing that the scales were plane on the under side and corrugated on the upper, a view which Mr. J. Beck controverted.—A note was read by Dr. Anthony on the statoblast of *Lophopus crystallinus* as a test for high powers.—Mr. Guimaraens exhibited the Echinorhynchus of *Lota vulgaris*, suggested to be a male specimen containing ova described as "dedans par hasard."—Mr. A. D. Michael read a paper, further notes on British *Oribatidæ*, which Prof. Huxley and others state to be wholly viviparous. He finds, however, that they are chiefly oviparous, as stated by Nicolet and others, and that the young are brought to maturity in at least four different modes; (1) the egg is deposited in a slightly advanced stage, as in insects; (2) deposited with the larva almost fully formed; (3) the female is occasionally viviparous (in these modes only one egg is usually ripe at a time); (4) several eggs are matured at once, but not deposited. The mother dies, the contents of her body, except the eggs, dry up, and her chitinous exterior skeleton forms a protection throughout the winter to the eggs. The occurrence of a deutovium stage in the egg is recorded, *i.e.* the egg has a hard shell, which splits into two halves as the contents increase in volume, the lining membrane showing between, and gradually becoming the true exterior envelope of the egg.—Several new and interesting species were described and figured, and exhibited under microscopes, Mr. W. H. Symons also read a paper on a hot or cold stage for the microscope.

Geological Society, December 21, 1881.—Mr. R. Etheridge, F.R.S. president, in the chair.—Messrs. Charles Duffin Barstow and Joseph Lundy were elected Fellows, and Prof. E. D. Cope, of Philadelphia, a Foreign Correspondent of the Society.—The following communications were read:—The Torridon Sandstone in relation to the Ordovician rocks of the Northern Highlands, by Mr. C. Callaway, M.A., D.Sc., F.G.S.—The Precambian (Archæan) rocks of Shropshire, part 2, by Mr. C. Callaway, D.Sc., F.G.S.—The red sands of the Arabian Desert, by Mr. J. A. Phillips, F.R.S., F.G.S. The author described the general characters of the Nefüd, of great red desert of Northern Arabia, which consists of a series of parallel ridges of considerable elevation, no doubt at some period piled up by the action of strong winds, but now no longer undergoing much change of position, as is evidenced by the fact that sticks and stones remain for many days uncovered on the surface, and that the landmarks made use of in crossing the desert appear to be permanent. A specimen of the sand of this desert received by the author from Lady Anne Blunt, is composed of well-rounded red grains from 1-50th to 1-30th of an inch in their longest diameter, which are rendered colourless by treatment with hydrochloric acid, the material thus removed amounting to ·21 per cent., or a little more than 1-500th of the total weight operated upon, and consisting of ferric oxide with a small quantity of alumina. The sand dried after the action of hydrochloric acid gave on analysis:—

Silica	98·53
Protoxide of iron	0·28
Alumina	0·88
Lime, magnesia, and alkalies	trace
	99·69

The external coating of ferric oxide must therefore have been deposited subsequently to the rounding of the grains; it could not have been derived from an external decomposition of the grains themselves; and it becomes difficult to imagine in what manner the superficial red coating can have been produced. The author compared these grains with those of the millet-seed sandstones of Triassic age, with which they closely agree in character, but remarked that the conditions of their occurrence were apparently quite different.—Analyses of five rocks from the Charnwood Forest district, by Mr. E. E. Berry, communicated, with notes, by Prof. T. G. Bonney, F.G.S., Sec. G.S.

EDINBURGH

Royal Society, December 19, 1881.—Mr. D. Milne Home, vice-president, in the chair.—The Makdougall Brisbane prize fur the period 1878–80 was presented to Prof. Piazzi Smyth, Astronomer-Royal for Scotland, for his extremely valuable paper on "The Solar Spectrum in 1877-78."—Sir Robert Christison communicated a short paper on the application of the rocks of the great precipice of Ben Nevis to ornamental work, in which he drew attention to the little-known but most magnificent view of the great precipice from below, characterising it as the grandest in the whole island. From the various kinds of granitic and porphyritic rocks there found, all of which are susceptible of a high polish, he had got constructed a very graceful obelisk, which was shown to the Society.—Dr. D. J. Hamilton exhibited and described certain physical experiments bearing on the circulation of the blood-corpuscles, from which he explained many points hitherto unexplained. Thus the rapid gliding central motion of the coloured corpuscles, and the slower rotational peripheral motion of the colourless corpuscles were to be explained by the fact that the latter were specifically lighter than the blood plasma, while the former were of the same specific gravity as the fluid in which they were borne along. Such a physical difference was sufficient to explain the phenomenon; and that such a difference existed could easily be demonstrated by observation as to the parts of a blood-vessel in which the colourless corpuscles abound. The second part of the paper dealt with more purely pathological questions, referring, for example, the migration of the blood corpuscles from the bloodvessels into the surrounding tissues simply to the increase of fluid pressure caused by stasis, and not to the amœboid movements of the corpuscles, which are generally urged as the true cause. Dr. R. S. Marsden read a paper on the state of carbon in iron and steel, in which it was argued that the molten metal held the carbon in solution, and that, on cooling, the carbon crystallised out in minute diamond crystals, so giving to the metal its peculiar hardness and temper. Much would depend on the size and number of the crystals, and the size was obviously a function of the rate of cooling; so it was quite conceivable that too much, as well as too little, carbon might have a deleterious effect upon the physical properties of the metal.

Boston, U.S.A.

American Academy of Arts and Sciences, December 14, 1881.—Prof. J. Lovering, president, in the chair.—Prof. C. L. Jackson and Mr. A. E. Menke presented the results of an investigation upon curcumin. The formula was shown to be $C_{14}H_{14}O_4$. By the study of the potassium salts it was proved to be a diatomic monobasic acid. Powerful oxidising agents destroy it; weaker agents, not in excess, give vanillin, but in too small quantity for purification; by oxidising diethylcurcumin, however, with potassic permanganate the authors obtained ethylvanillic acid, with melting-point at 195°.—A paper on a comparison of the Harvard College Observatory Catalogue of Stars for 1875, with the fundamental systems of Auwers, Boss, Safford, and Newcomb, was read by Prof. William A. Rogers.—Dr. Wolcott Gibbs announced the discovery of the following new complex acids:—Arsenoso-molybdic acid, arsenoso-tungstic acid, antimonoso-molybdic acid, antimonoso-tungstic acid, vanadoso-molybdic acid, vanadoso-tungstic acid, vanadio-phosphoric acid, vanadio-arsenic acid, vanadio-antimonic acid. All of these acids have well-defined series of salts.—A paper on the law of diffusion of gases was read by Mr. N. D. C. Hodges.

Paris

Academy of Sciences, January 2.—M. Jamin in the chair.—M. Blanchard was elected Vice-President for 1882.—The Academy has lost three members during 1881, viz. MM. Delesse, Deville, and Bouillaud; and two correspondents, MM. Kuhlmann and Pierre.—M. Faye presented the *Annuaire du Bureau des Longitudes* for 1882; it contains, *inter alia*, a complete table, with history, &c., of the comets of the last decade, by M. Lœwy, and a fac-simile of M. Janssen's photograph of the comet of last summer.—On the correction of compasses, and on M. Collet's recent "Treatise on the Regulation and the Compensation of the Compass," by M. Faye.—Craniology of the Mongolian and white races, by MM. de Quatrefages and Hamy. They presented the tenth and last volume of their "Crania Ethnica," and gave a *résumé* of the contents. The different general forms of the human skull are found in each of the three chief races; but while among the black races, globular skulls, and among the yellow, elongated skulls, are rare, among the white the two cephalic types coexist in nearly equal proportions. The authors regard craniology as one of the most powerful means of scientific study of human races.—On the diffusion of solids, by M. Colson. To a given temperature corresponds a constant coefficient of diffusion of carbon in iron. This law holds only so long as the iron is transformed into steel. Among substances that diffuse very easily in carbon, silica holds the first place. Platinum wire, heated long enough with lampblack in an earthenware crucible, becomes crystalline, and has the composition $SiPt_2$ (the silicum being from the crucible, whose silica is diffused in the lampblack). Repeating the experiment with lampblack holding 60 per cent. of precipitated silica, one obtains SiS_2Pt_3.—On the diffusion of carbon, by M. Violle. He had observed, in 1878, a diffusion of carbon in porcelain (temperature under 1500°).—Anchylostoma (duodenal anchylostoma of Dubini) in France, and the disease of miners, by M. Perroncito. The miners' anæmia of Saint Etienne has the same parasitic cause as that of the workmen in the St. Gothard, the Schemnitz miners, &c. The malady may be prevented by cleanliness and treatment of excrementitious matters with heat of 50° C. (to kill eggs, larva, and worms), or better, with concentrated solutions of chloride of sodium, sulphuric, hydrochloric or carbolic acid, or Depernis's insecticide liquid. Patients should be treated with doses of etherised extract of male fern.—On algebraic forms with several series of variables, by M. le Paize.—Integration of certain equations with partial derivatives, by means of definite integrals containing, under the sign s, the product of two arbitrary functions, by M. Boussinesq.—On the theory of motion of planets, by M. de Casparis.—On the determination of the ohm; reply to M. Brillouin, by M. Lippmann.—Measurement of potentials corresponding to determinate explosive distances, by M. Baille. The potential of an electrified plane increases nearly regularly with the explosive distance which can be traversed. The electric densities decrease at first slowly, reaching a constant value about 0.5 ctm. The pressure of electricity on the air when a spark of 0·01 m. passes is only 1-2000th of atmospheric pressure.—Note on the temperatures of the sea observed during the mission to Lapland, by M. Pouchet. In the roadstead of Vadsö the mean sea-temperature rose about 9° in 50 days from June 8 (or abont 0°·2 a day). A cooling influence of the coast was observed to $1\frac{1}{4}$ miles and to a depth of 100m. at the Vadsö anchorage (a difference of about 1° for depths of 20 to 30m.). The temperature always decreased very regularly to the bottom.—On the ratio of potash to soda in natural waters, by M. Clocëez. This relates to water of the Seine, Marne, Dhnis, Vanne, &c. In general the potash counts for more than 1-5th in the sum of alkalis (potash 25, soda 100), and while the potash comes from decomposition of felspathic rocks, the soda is probably from chloride of sodium impregnating all the strata, except granitic soils. The Vanne, rising in the chalk and not meeting argillaceous deposits, has no salts of potash.—On the complex function of morphine, its transformation into picric acid, and its solubility, by M. Chastaing.—On artificial production of the forms of organic elements, by MM. Monnier and Vogt. He obtains cells, tubes, &c., by bringing together two salts in a liquid, forming by double decomposition one or two insoluble salts.—Researches on development of cryptogamic vegetation without and within hens' eggs, by M. Dareste. Such vegetation he found on most of sixty eggs submitted singly to artificial incubation in a small vessel hermetically closed with a caoutchouc stopper. He considers the spores to have entered the oviduct from the cloaca and to have been incarcerated in the egg during its passage in the oviduct. The vegetation may be fatal to the embryo.—On a parasitic tuberculosis of the dog and on the pathogeny of tuberculous follicle, by M. Laulanié. He observed in a dog's lung alterations very like those of tuberculosis, produced by eggs of a nematoid (*Strongylus vasorum*, Baillet).

Vienna

Imperial Institute of Geology, December 6, 1881.—G. Laube, on melaphyry-stones inclosed in the porphyry of Liebenau (Bohemia).—R. Hoernes, on the remains of mammalia found in the brown coal at Goeriach, near Turnan, Styria.—Th. Fuchs, on the relations of heat and light of the ocean.—L. Szajnocha, exhibition of the geological map of Taslo and Krosmo in Western Gallicia.

December 20.—C. Doelter, on the volcanic rocks of the Cape Verde Islands.—R. Hoernes, exhibition of remains of mammalia from the Styrian brown coal-deposits.—G. Stache, new data on the occurrence of olivin-rocks on the gneiss mountains of Southern Tyrol.—V. Uhlig, on the composition of the lime-rocks at Lublau (Hungary).

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