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THURSDAY, JANUARY 4, 1872

BRITISH PREPARATIONS FOR THE APPROACHING TRANSIT OF VENUS

IN nearly all those countries of Europe in which Astronomy is nationally cultivated, preparations are being made for thorough observation of the first of the coming Transits of Venus, which will occur on December 8, 1874. In Russia, whose territory presents many favourable points for observation of the phenomenon, a committee, organised by Professor Struve, has had under consideration during the past two years the establishment of a chain of observers at positions 100 miles apart along the region comprised between Kamschatka and the Black Sea. The principal astronomers of Germany have held two conferences, each of several days' duration, which have resulted in a decision to furnish four stations for heliometric observation of the planet during its transit: one of these will be in Japan or China, and the others probably at Mauritius, Kerguelen's and Auckland Islands; and three of these, with the addition of a fourth station in Persia, between Mascate and Teheran, will be equipped for photographic observations also. A French commission on the subject sat before the war, and reported to the Bureau des Longitudes that it was desirable for their government to provide for observing stations at Saint Paul's Islands, and Amsterdam, Yokohama, Tahiti, Noumea, Mascate, and Suez. Since the close of the war the subject has been reverted to, and lately the Academy of Sciences applied to the Government for the requisite funds; but these could not be granted till next year, the budget for 1872 having been disposed of.

The British preparations, to which we shall chiefly confine our remarks, are, we believe, in a more advanced state than those of any other country. This forwardness may probably be ascribed to the circumstance that they have from the first been directed by a single mind, and have thus been freed from the inevitable delays of a committee. The Astronomer Royal first called attention to the Transits in 1857 and again in 1864. In 1868 he commenced to shape definite plans, selected the observing stations which were in all respects most suitable for British occupation, and opened communications with the Government upon the financial requirements of the undertaking.

Presuming a general acquaintance with the phenomenon under notice, and its availability for determination of the parallax of Venus, and that of the Sun (a subject that has been well popularised), we merely remark that there are several methods by which observers at opposite points on the earth may measure the parallactic displacement of Venus upon the Sun's disc: (1) by durations of Transit (Halley's method); (2) by absolute local times of ingress and egress (Delisle's method); (3) by heliometric measures of the planet referred to the limbs of the sun; (4) by similar measures obtained from photographs of the sun with the planet on his disc. The first of these has been considered disadvantageous for the 1874 Transit, which is the one that immediately concerns us. The third and fourth are of recent suggestion, and have been regarded as of doubtful accuracy, especially the fourth, whose reliability is still the subject of experimental inquiry. The second was the one which demanded foremost attention. The Astronomer Royal, therefore, as a first step, set down the stations best available for its application. These had to be selected in order to combine a sufficient altitude of the sun with the maximum attainable acceleration of ingress and retardation of egress on one side of the earth, and retardation of ingress and acceleration of egress on the other side of the earth. And after weeding the lists for each phase of such stations as were expected to be provided for by foreign governments, and of those already occupied by established colonial observatories, it was found that there were five stations which it was desirable that England should prepare to equip. These were Woahoo (for observation of accelerated ingress), Kerguelen's and Rodriguez Islands (for the retarded ingress), Auckland in New Zealand (for the accelerated egress), and Alexandria (for the retarded egress).

Now, as at all these places the absolute local time of the phenomenon is required, it is indispensable that the longitude of each be very exactly known. In no one case does a sufficiently accurate determination of this element exist, and provision must therefore be made in each case for obtaining it. This vastly increases the extent of preparations for the instrumental equipment of the stations, and renders necessary a three or four months' sojourn of the observers at each. Of the methods for determining longitude which were open to choice, the Astronomer Royal decided to employ that by vertical transits of the moon, and for observing these he resolved upon supplying altitude instruments with fourteen-inch circles and telescopes of twenty inches focus. For time determinations he proposed three-inch transits, of thirty-six inches focus, with clocks of moderately high class For observing the phenomenon he elected to employ at each station one six-inch equatorial and one four-inch portable telescope. For these an observatory of three rooms was required. With the exception of one altazimuth, two clocks, and two or three four-inch telescopes, which the Greenwich Observatory could furnish, all the specified instruments and the observing rooms had to be specially provided. An estimate for their purchase and construction, amounting to 2,154*l.*, was therefore submitted to the Admiralty. Some needful chronometers and meteorological instruments were available from home stores. To the above estimate for material requirements were added others, prepared by Admiral Richards, for the personal expenses, the conveyance, residence, pay, and contingencies, of the observing parties. These amounted, for the Woahoo detachment, to 2,500*l.*, for the Rodriguez and Kerguelen's parties to 2,000*l.* each, for the Auckland party to 1,000*l.*, and for Alexandria to 750*l.*, making a total of 8,250*l.* The grand total of 10,500*l.* was asked of the Treasury in May 1869, and immediately granted.

The construction of the requisite instruments and clocks was forthwith commenced, by Messrs. Troughton and Simms and Messrs. Dent. Three six-inch equatorials happening, however, at the time to come into the market, they were at once purchased; one of the three being that which is known to fame as the "Lee Equatorial," and is the instrument used by Admiral Smyth in the preparation of his "Celestial Cycle." The observatories were put in hand also. They are somewhat substantial structures, formed of a stout wooden framework, covered with weather-boarding and roofed with zinc and roofing-felt. Each instrument has a separate hut. The transit huts are ten feet square, with walls six feet high, and with the shutter openings a little on one side of the centre, so as to leave good room for mounting the clock, &c. The altazimuth huts are planned on a nine-feet hexagon. Their domes are hexagonal pyramids erected on circular frames, which are grooved to run on six-inch rollers. These rollers, six for each dome, are mounted on the wall-curbs. One flap-back shutter gives sky view from the horizon to the zenith. Each hut is made portable by being constructed in sections which are connected together by bolts and nuts. For the transit instruments massive Portland stone piers and foundation slabs have been provided; for the altazimuths stone pier-caps only will be sent out, leaving the piers to be provided on the spot. Every part of each observatory and every packing case has been numbered and marked by stencilling, with a letter to denote the station for which it is destined.

These transit and altazimuth observatories, with their instruments and the primary clocks, are, with trifling exceptions, in perfect readiness for use. The equatorials are generally ready, though their final completion has been interrupted by the loan of portions of them to the observers of the recent solar eclipse. The telescopes will be supplied with the Astronomer Royal's prismatic eye-piece for correction of atmospheric dispersion, which will necessarily be considerable at the low altitudes at which some of the contact observations must be made. The equatorial observatories are not yet constructed; the plans for them are under consideration as we write. The four-inch telescopes, some second-class clocks for use with the altazimuths and equatorials, and the small accessories, have also to be provided.

It is early to speak of the *personnel* of the various observing expeditions. Officers of the army and navy will probably compose a large proportion of the observing corps. Several gentlemen of the Royal Artillery have already commenced practice at Greenwich with the time and position instruments; but, with the object of forming a more accessible school of observation for them, a temporary observatory has been fitted up near to their headquarters at Woolwich.

Photography was not included in the Astronomer Royal's original plans. But from the time that his preparations were first mooted, the probable advantages of photoheliometry of the planet during transit were strongly insisted upon. The plans for photography were advanced from photographic quarters; astronomers of the exact class who were not photographers were somewhat sceptical at the outset concerning its accuracy. They anticipated that uncertainties would attach to the photographic measurements: in the first place from optical distortion of the image formed by the camera-telescope; in the second place, from mechanical distortion produced by unequal shrinkage of the collodion film, which must receive its impression in the wet state, whereas the measurements must be taken when it is dry; and in the third place, it appeared doubtful whether sufficiently accurate scale measurements could be secured to make the results equally reliable with those to be obtained from eye observation of the contacts. No method of secondary accuracy could be tolerated, since the received value of the solar parallax ($8''\cdot95$) is probably much less than 1 per cent. in error. It is considered that an eye-observation of contact, *i.e.* of formation or rupture of the "black drop," can well be made with no greater error than 4 seconds of time. As Venus moves over the sun at the rate of about $2''$ in a minute of time, the 4 seconds correspond to a displacement of $0''\cdot12$ of arc in the direction of motion, or about $\frac{1}{14000}$ of the sun's diameter. Can the measurements from a photograph, with all the above noted chances of error, be relied upon for such small quantities? It is argued that they can. The probable error of a single micrometric measurement of the photographic distance of the images of a double star is cited by Mr. Asaph Hall* to be $0''\cdot12$, and Mr. De La Rue, who is naturally the English referee in such matters, has no hesitation in saying that the measurements from a solar photograph *may* be depended upon, with all due precautions, to the $\frac{1}{16000}$ of the sun's diameter. He is of opinion that the shrinkage of the collodion film takes place only in the direction of its thickness, and he considers that if any optical distortion exists, it may be determined, and the correction for it found, by photographing a scale of equal divisions upon different parts of a plate, and comparing micrometric measurements of the various images. Upon this point he is about to make some crucial experiments with a large scale erected upon the Pagoda at Kew, and photographed from the Kew Observatory with the image in all positions on the sensitive plate. Herr Paschen is also investigating the matter on the part of the German Commission, using for his test-scale a glass plate divided into squares by diamond-ruled lines. Some preliminary trials have convinced him that should it be impossible to get rid of distortion, it will yet be easy to correct for it as accurately as may be desired.

Although the thorough reliability of the photographic method has not yet been satisfactorily established, the doubts concerning it have been in part removed, and it has appeared undesirable to neglect photography in the face of the circumstance that it might be the means of obtaining some useful record of the transit at stations where from atmospheric causes the observations of contact may be lost or vitiated. Moreover, as other nations had decided to employ the photographic method, it seemed incumbent upon Britain to work in harmony if not in actual concert with them; for although there has as yet been no formal proposal for international co-operation, there have been communications between the astronomical authorities of the various countries concerned, which have prevented the formation of very divergent plans. The Astronomer Royal therefore laid the subject before the Board of Visitors of the Greenwich Observatory, at their meeting in June last, and it was fully discussed by them. They resolved that it was desirable to furnish all the English stations chosen for eye observations with the necessary photographic appliances, and an application was shortly afterwards addressed to the Treasury for a grant of 5,000*l.* to defray the expenses of the additional equipment. The money was granted, and the construction of the photo-heliographs—five in number—was forthwith placed in Mr. Dallmeyer's hands. These instruments will be of generally

* *Silliman's Journal*, vol. cii., p. 26.

similar design to one made by the same artist for the Wilna Observatory, which has produced sun-pictures that, so far as the eye can judge, leave nothing to be desired in point of sharpness of definition and freedom from such distortion as the photographed cross-wires can exhibit. The object-glasses will be of about 4in. diameter, giving focal images of the sun about half an inch in. diameter. The focal image will be amplified to about 4in diameter on the photographic plate, and, in applying the enlarging lens, Mr. Dallmeyer is confident that he can entirely destroy the spherical aberration. The camera-telescopes will be mounted on equatorial stands, with latitude adjustment of 80° range; and they will be furnished with driving clocks.*

For the general photographic organisation, the Astronomer Royal has secured the co-operation of Mr. De La Rue, under whose able supervision the instruments above mentioned will be constructed, and by whom the various details of the photographic scheme will doubtless be arranged. Of the five stations already selected for eye observation of contacts, three are well suited for photographic record. These are Rodriguez, Kerguelen's, and Auckland, at all of which the whole transit will be visible. The Hawaiian station and Alexandria, though they are available, are less advantageous than the rest, because only a portion (about half) of the transit will be visible from each, and the photographs, besides being thus limited, must be obtained at low altitudes of the sun. It may become a question whether the heliographs provided with a view to furnishing these two stations cannot be more advantageously located. But before the positions are finally decided upon, it appears desirable that the intentions of other nations should be fully known, or, as would be preferable, that the ultimate distribution of observers of all kinds—telescopic, heliometric, and photographic—should be made the subject of an International Conference.

J. CARPENTER

* There are grounds for hoping that the same artist will construct some precisely similar photo-heliographs for other countries, for use on the Venus Transit. There would manifestly be great advantage in the employment by all photographing observers of instruments whose optical portions at least are of identical material and manufacture.

JUKES'S MANUAL OF GEOLOGY

The Student's Manual of Geology. By J. Beete Jukes F.R.S. Third edition, re-cast, and in great part re-written. Edited by Archibald Geikie, F.R.S. (Edinburgh: A. and C. Black, 1872.)

IF there be any one feature more strongly marked in the present age than another indicative of progress and intellectual advancement, it is the superiority of most (we will not say of all) of the books intended to promote education. School books and class books of all kinds, instead of being merely reprints, as in the days of yore, now really undergo revision every five years or so, or are superseded by new ones; whilst the introduction of natural science teaching into our Universities and public schools has created a demand for text-books to an extent greater even than the supply.

Among the various writers of the day on the science of Geology, Sir Charles Lyell must undoubtedly be placed in the front rank, as having done more than any other man to promote its study, and his "Principles" and "Elements" of Geology still hold the highest places in our estimation; but we must not forget that Phillips, Dana, and Jukes have also furnished us with geological manuals, more elementary in their style and arrangement, and therefore more serviceable for beginners than are Lyell's works. In order, however, to remedy this, Sir Charles Lyell has lately brought out a "Student's Elements of Geology," 8vo. pp. 624 (Murray), being an abridged edition of his larger work. This will no doubt prove a very useful book to beginners as an introduction to the higher class books.

Jukes's "Student's Manual of Geology" was born in 1857, and has already gone through two previous editions, each time, as is the sad fate of such books, growing more corpulent, till the poor student turns pale before the vast array of facts, neatly arranged for him to "cram," in the smallest possible type.

The original design contemplated in 1854 was an article on Geology for the "Encyclopædia Britannica," to have been carried out by the late Prof. Edward Forbes and Mr. J. Beete Jukes conjointly; but the death of Forbes for a time deferred the task. It was afterwards inserted in the Encyclopædia under "M," as "Mineralogical Science," and finally appeared as a separate work in 1857. The first edition is comprised in 610 pp., and is illustrated by 74 woodcuts, chiefly diagrams and sections of rocks, &c.

The second edition appeared in 1862, having grown an inch in the size of its page, and added 154 pages to its bulk, partly owing to the addition of 100 more illustrations, 50 of which are of fossils, or rather groups of fossils.

The idea of these figures of "Fossil groups," as they are termed, seems to have been taken from the admirable series of little woodcuts which illustrate the invertebrate portion of Owen's "Palæontology,"* prepared by the late Dr. S. P. Woodward. They are, however, arranged stratigraphically in Jukes's "Manual," not zoologically, as in Owen's "Palæontology."

The third edition, now before us, is only fourteen pages thicker than the second edition, and contains thirty-one more illustrations; but the bulk of matter is vastly increased by the use of smaller type than in the former editions.

The illness which seized Mr. Jukes, and by which he was removed from among us, had already impaired his health so much as to render it desirable he should be relieved of the labour of completing this edition, and the task was accordingly, by the author's own wish, undertaken by Professor Geikie, Director of the Geological Survey of Scotland.

The eighty pages on mineralogy (forming chapters II. and III.) have been entirely re-written by Dr. Sullivan; Chapter XIII., on trap-rocks, has been re-written by Prof. Geikie, as well as many other parts. Mr. Hull has revised the description of the English Coal-measures. Messrs. Bristow, Whitaker, and Judd have looked over the Mesozoic and Cainozoic chapters, and Prof. Huxley has contributed a new synopsis of the animal kingdom.

By a modification of the former edition, a new part is introduced (Part II.) called "Geological Agencies, or Dynamical Geology," a part of which also is from the pen

* Second Edition, 1861 (Edinburgh: A. and C. Black).

of Prof. Geikie, and now appears for the first time. It treats of the origin of hills, lakes, valleys, caverns, passes, fjords, glaciers, river-deposits, sea-action, coral-reefs, and all the many phenomena which either are themselves the cause, or the effect, of geological agents.

We have such a strong feeling against making a reference-book, especially one intended for the use of students, too bulky to be conveniently handled, and even carried about with one, as is frequently needful, that we have looked most closely into the present edition to see in what way it may be reduced without injury, bearing in mind that it only purports to be "a Student's Manual of Geology."

Candidly, then (without the least disrespect to Dr. Sullivan), we think the two chapters on chemistry and mineralogy (chapters II. and III., occupying eighty-one pages) should have been omitted. For these sciences, although so intimately related to, and constantly extending their aid to geology, are equals in rank and importance as sciences, and the student, if intending properly to master them, must possess such text-books as Williamson's Student's Chemistry and Dana's System of Mineralogy, books of equal importance in these sciences to Lyell's or Jukes's geological works.

As might naturally be expected in a text-book framed by a Geological Surveyor deeply versed in all the intricacies of rock structures in the field, and constantly dealing with stratigraphical questions, the book treats most largely of physical geology, not, however, to the exclusion of palæontology; yet exalting petrological science—at present in its infancy—into a far higher place than it has hitherto occupied in any other similar work. We do not wish it to be understood that we desire to undervalue lithological characters, especially in rocks devoid of organic remains; but we find such conflicting opinions prevalent among petrologists, that we are led to doubt the possibility of teaching much of such a branch of geological science to the student until the nomenclature of the principal rocks is settled by a congress of geologists, mineralogists, and chemists, or by some other authoritative body.

If in a new edition the mineralogy is omitted, we would suggest the introduction of a glossary of geological and zoological terms, which the beginner would, we feel sure, be very grateful to find added to the index, as an addition to the valuable tables of classification contributed by Prof. Huxley.

We heartily recommend the book to both intending teachers and students, who will find it a most complete compendium of geological science, and still one of the best Manuals in our language, as it has now been brought by its editor, Prof. Geikie, fairly "abreast of the onward march of science." H. W.

BREHM'S BIRD-LIFE

Bird-Life. By Dr. A. E. Brehm. Translated from the German by H. M. Labouchere, F.Z.S., and W. Jesse, C.M.Z.S. Parts I.—III. (London: Van Voorst, 1871.)

MR. WILLIAM JESSE, at the instigation of his colleague, is doing his best to make a silk purse out of—well, we do not wish to be rude, so let us say, out of materials of which silk purses are not commonly made; for Dr. Alfred Edmund Brehm has the fatal facility of being able to write endless nonsense on a subject which, in better hands, might be made truly instructive. He is so far from being a true naturalist that he is constantly being misled, confounding analogies with homologies. Take his second paragraph, as Mr. Jesse translates it, and translates it very well too:—

"Birds have much in common with mammals; and it is certain that some striking resemblances between individuals of both classes cannot be denied. Every impartial observer must recognise in the eagle the image of the lion, or rather its true representative in the bird-world; in the owl we see the cat; the raven resembles the dog; the vulture, the hyæna; the hawk, the fox; the parrot, the monkey; the crossbill, the squirrel; the wren, the mouse; the butcher-bird, the weasel; the bustard, the stag or antelope; the ostrich, the camel; the cassowary, the llama; the dipper, the water-rat; the duck, the duck-billed platypus; the diver, the otter; the auk, the seal; and so on. In spite of all these resemblances, which, after all, only apply to the external aspect, the bird is always and essentially distinct from mammals" (p. 2).

What, then, is the use of all this? Even the translator has to append a note stating that the author has not truly explained what he is writing about, and, indeed, it is plain that the writer to whom such ideas as the foregoing occurred has no pretension to be accounted a scientific man. Their association jars upon the feelings and contravenes the knowledge of any student of morphology. We have no wish to shock our readers even with the commonest terms of German philosophy, but is it not clear that to draw a parallel between a raven and a dog, and between a butcher-bird and a weasel, while a fox is likened to a hawk and a water-rat to a dipper, is simply a *subjective* process, depending entirely on the fancy of the beholder? Of what use then are any speculations on "Bird Life" by such an one? To most men the observation of the aspects of nature, as exhibited under divers conditions of country and climate, afford a most instructive education. To Dr. A. E. Brehm it seems to be otherwise. He has wandered in many lands, and has seen in their homes the faunas of both north and south. The only effect this seems to have had upon him is to teach him that he lives. "Movement is life" we read (p. 19), "and life is the power of self-motion." Motion is therefore the chief characteristic of birds. "The bird is, of all creatures, the most versatile in its movements; it runs, climbs, swims, dives, and flies" (p. 20). He is careful to add that all these qualities are not to be found in a single species; but may not just as much be said for the insect or the mammal; or even if the dreams of some geologists be well-founded, might they not all have been found "combined in one creature"? A contemporary of the pterodactyls might, with some appearance of truth, have applied to one of them the description of Milton's fiend, who

> O'er bog, or steep, through strait, rough, dense, or rare,
> With hand, head, wings, or feet, pursues his way,
> And swims, or sinks, or wades, or creeps, or flies.

So far as powers of locomotion go, and by "movement" Dr. Brehm plainly means locomotion, the bird is hardly superior to the insect or the mammal. But to return to the extraordinary hypothesis that "movement is life," and the converse. The most miserable savage that ever plucked a mussel from the rock knows better in this respect than Dr. Brehm; and when the latter tells us, *à propos* of the songs of birds (p. 37), that the "voice is still motion," and we connect the statement with a previous assertion (p. 19), that "worlds roll on through boundless space—and live," we feel certain that we ought to hear the music of the spheres, or some other mystical sweet sounds, if we could only elevate ourselves to his exalted ecstasy.

But we think we need not trespass further on the time of our readers. We will conclude by expressing the hope that when Mr. Jesse and Mr. Labouchere next set about translating a German author they will have better luck in pitching upon a subject—and they will easily find one—for their labours than the rhapsodies of Dr. Alfred Edmund Brehm.

OUR BOOK SHELF

Proceedings of the London Mathematical Society. Vol. iii., Nos. 21—40.

THE papers read before this Society still preserve the high character attributed to them in the notice of vol. ii., which appeared in this journal. That such should be the case is not matter for surprise, when we run our eyes over the list of contributors. The principal authors are Prof. Cayley and Mr. Samuel Roberts. The former furnishes three memoirs on quartic surfaces (pp. 59—69; 198—202; 234—266); sketch of recent researches upon quartic and quintic surfaces; rational transformation between two spaces (pp. 127—180); on Plücker's models of certain quartic surfaces. The latter communicates papers on the order of the discriminants of a ternary form; pedals of conic sections (pp. 88—98); on the ovals of Des Cartes (pp. 106—126); on the order and singularities of the parallel of an algebraical curve (pp. 209—259); on the motion of a plane under certain conditions. Prof. Clerk Maxwell contributes a paper on the mathematical classification of physical quantities. Besides the foregoing communications, the above-named gentlemen have laid other papers before the Society. Memoirs have also been presented by Mr. J. Griffiths, Mr. J. J. Walker, Prof. Clifford, Hon. J. W. Strutt, and other members. Some other highly valuable communications, we learn from the "Proceedings," were made to the Society, but no record has as yet been made of them, their authors not having yet sent their completed papers for publication. The Society, from the number and high character of its memoirs, seems to have met a want, and is, perhaps, the only Society before which many of the communications could have been brought. As generally the papers are worked out in some detail at the meetings, members have an interesting opportunity of seeing how some of our foremost mathematicians employ their divers instruments. The Society has lost by death during the past session, its first president, and one of its earliest warm supporters. A slight sketch of Prof. De Morgan and his works appeared in NATURE close upon his death in March last. The eighth session of the Society's existence has just commenced, and we trust its future work may be as good as that it has already achieved. *Floreat.*

Treatise on Terrestrial Magnetism. (Blackwood and Sons.)

THE first half of this book contains a good deal of information, and some inquiries connected with the question of the secular variations in the magnetic elements. The author, on the supposition that the secular changes in the declination are caused by the action of a single, slowly rotating pole on a needle which at each place is locally influenced in a definite and determinable manner, computes the declination at several places, and shows that it agrees tolerably well with actual observation. The rotating pole he places at a constant distance of 23° 30′ from the pole of the earth's axis, and gives to its rotation a period of 640 years. The latter part of the book, however, is taken up with "an hypothesis." The writer of this book, and many other such writers, would do well to remember the words of Newton, "*Hypotheses non fingo.*" The hypothesis referred to is simply this:—that the sun attracts the electric matter in the earth and carries it round in a sort of tidal wave, this causes an electric current from east to west, which causes the magnet to point to the north, and from which the writer also attempts to deduce some of the other phenomena of magnetism. There seems to us to be some ambiguity in the writer's method of expression, so that we do not clearly gather whether he intends this current to account for the whole magnetic action of the world, or only for the variations of it. A consideration of the character of the variations of the needle is sufficient to overthrow the hypothesis announced by our author. The solar diurnal variation is thus explained by him:—The pole of the ecliptic revolves once a day round the pole of the earth's axis, the needle tends to follow this, and hence the solar diurnal variation. Now, we may point out a circumstance which, apparently, entirely overthrows, not only this hypothesis, but any which attempts to account for that variation by anything of the nature of the movement of a magnetic pole. At Point Barrow the needle points N.E., at Port Kennedy it points S.W., yet at each place the solar diurnal variation follows local time and exhibits precisely the same features. Standing, then, at the centre of the needle, and looking towards its marked end, that end would at both places be observed to be moving towards the left hand of the observer between the hours of 8 A.M. and 1 P.M. But since the needles are pointing in opposite directions, this constitutes a movement of the marked end of the one towards the geographical west, and of the marked end of the other towards the geographical east, and this at times when the needles are under precisely the same circumstances with respect to the sun's influence. Now, no movement of the magnetic pole can account for this, it would necessarily entail a movement of the marked end of both these needles in the same geographical direction. The consideration of this phenomenon shows us that if the solar diurnal variation of the declination is to be attributed to a current, it must be one not round the magnetic pole or the geographical pole, but along the magnetic meridian. But this is not the place for us to discuss this question further at present. It would seem to be, however, rather from the consideration of such phenomena as this in a careful and accurate way, and the attempt therefrom, by induction, to arrive at laws, that we may hope to form a theory of terrestrial magnetism, than from "making an hypothesis," and then attempting to apply it to facts. J. S.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Mayer and De Saussure

IN Prof. Tyndall's account of the labours of Mayer, a paragraph is devoted to the bearing of his principles upon the phenomena of vegetable life. It suggests two points of difficulty to me:—

1. It is said that "Mayer's utterances are far from being anticipated by vague statements regarding the 'stimulus' of light, or regarding coal as 'bottled sunlight.'" Nevertheless the paragraph reads almost like a paraphrase of the following passage from Sir John Herschel's "Outlines of Astronomy" (1833), p. 211:—

"The sun's rays are the ultimate source of almost every motion which takes place upon the surface of the earth. By its heat are produced all winds and those disturbances in the electric equilibrium of the atmosphere, which give rise to the phenomena of terrestrial magnetism. By their vivifying action vegetables are elaborated from inorganic matter, and become in their turn the support of animals and men, and the sources of those great deposits of dynamical efficiency which are laid up for human use in our coal strata. By them the waters of the sea are made to circulate in vapour through the air and irrigate the land, producing springs and rivers. By them are produced all disturbances of the chemical equilibrium of Nature, which by a series of compositions and decompositions give rise to new products and originate a transfer of materials."

In a note in Mr. Herbert Spencer's "First Principles" (2nd Ed., p. 496), which first led me to look at this passage, it is remarked that Herschel "expressly includes all geologic, meteorologic, and vital actions, as also those which we produce by the combustion of coal," in the effects of the solar rays. When, therefore, Prof. Tyndall states that Mayer *revealed* the source of the energies of the vegetable world, it appears to me that Herschel anticipated the revelation twelve years previously. Of course, I apprehend that Mayer's merit consisted in seizing at once a physical principle of immense generality, and in applying it to very different phenomena. Herschel began at the other end; but appears equally to have seen the solar energy underlying these phenomena, though in a general way, and without demonstrating numerical relations.

2. De Saussure is credited unreservedly with the observation of the reducing power of the solar rays in the vegetable economy. But he seems to me, as, indeed, he seemed to himself, to have only crowned a theory which other workers had elaborated. Priestley began by ascertaining that air depurated by animals was purified by plants. Ingenhousz showed, what Priestley candidly confesses he missed, that this effect is due "chiefly, if not only, to the light" of the sun. Senebier found that "fixed air" was the ingredient which plants removed from a vitiated atmosphere, and that this underwent elaboration in the tissues, oxygen being set free as the result.* Lavoisier having previously shown that fixed air was a compound of carbon and oxygen, Senebier's results implied the fixation of carbon by plants. This fixation De Saussure actually demonstrated by feeding a plant with carbon dioxide and water alone, and showing that the carbon in the tissues increased. He further found the unexpected fact (and this is what he added to the matter) that the oxygen evolved by plants does not correspond to that contained in the carbon dioxide absorbed, but that it is smaller in quantity.

De Saussure's researches are a beautiful example of quantitative work, but they would have, I imagine, merit of a different order if Priestley, Ingenhousz, Senebier, and Lavoisier had not broken ground before them.

W. T. THISELTON DYER

* Recherches sur l'influence de la lumière solaire pour metamorphoser l'air fixe en air pur par la végétation, 1783.

Phenomena of Contact

IN NATURE of August 24 I objected to, as misleading, the statement by Mr. Newcomb that "we find ligaments, black drops, and distortions sometimes seen in interior contacts of the limbs of Mercury or Venus with that of the sun, described as if they were regular phenomena of a transit, without any mention of the facts and experiments which indicated that these phenomena are simple products of insufficient optical power and bad definition which disappear in a fair atmosphere with a good telescope well adjusted to focus." I asked for references to the facts and experiments by which the statements are justified.

In NATURE of September 28 I find Mr. Newcomb's reply, but without the references which I desired. Mr. Newcomb considers that I controvert the two following propositions:—

1. That the irregular phenomena of internal contact of a planet with the sun, variously described as distortions, black drops, ligaments, &c., are not always present, but are only seen sometimes.

2. That when seen they are due to insufficient optical power or bad definition.

If the word "irregular" is cut out, and the word "seen" substituted for "present" in proposition (1) there can be no doubt about its truth. It will be found that all the arguments adduced by Mr. Newcomb to prove this proposition have no bearing either upon the word "irregular" or "present" in contradistinction to "seen."

It appears to me, therefore, quite unnecessary to allude further to this proposition.

With reference to proposition (2), I believe it to be utterly erroneous. I believe that the phenomena of the fine connecting ligament can only be seen in a fair atmosphere, with a good telescope well adjusted to focus, and with considerable magnifying power. When it is remembered that the fine connecting ligament is confined to within about a second of arc of the sun's limb, I think my statement will at least commend itself to practical observers. Mr. Newcomb appears to regard it as a great difficulty in my view of these phenomena, that some of the observers should see the ligament and some not. I am rather surprised at the persistence with which this point is again and again brought forward in his letter. I thought that it had been answered by anticipation in my letter which appeared in your number of August 24. In all my writings upon the subject I have maintained that the phenomena could only be seen under favourable circumstances and with sufficient power; and in my letter of August 24 will be found this statement, which appears to have been entirely overlooked, at least unanswered, by Mr. Newcomb:—"The optical enlargement by irradiation is a function of the brightness, and can be made insensible by sufficiently diminishing that brightness. Unfortunately, however, when this diminution of brightness is carried to a very great extent, errors in an exactly opposite direction to those of irradiation will come into play, similar, in fact, to the results of Wolf's experiments. The observations of Mercury on the sun's disc in 1868 were made with very different optical means, and some very different methods were adopted for diminishing the sun's glare." In my view those observers who did not see the connecting ligaments failed to see it, either from want of attention to the point as not a contact such as they expected to see, or from the observations having been made under such circumstances that some of the necessary conditions which I have indicated were not satisfied.

The fine connecting ligament is only seen by contrast against the illumination of the sun's disc near the point of contact, and it may well be that some of the observers have pushed the diminution of brightness of the sun's image to such an extent that the contrast was too feeble to attract attention before the apparent contact. To me, and I think to others who will give the matter some consideration, it is clear enough "that an observer with the naked eye, a telescope of low power, would not, in the case of a transit of Venus, see the connecting ligament at all." It is as clear "that without seeing any ligament, the planet, at egress, would appear to touch the limb without distortion, necessarily, earlier than the contact would appear to be established to observers who were watching the transit with good telescopes and with high powers." It appears to me equally clear that, if the brightness of the sun's image be reduced to excess, then the ever-diminishing small portion of the illuminated disc between the sun's edge and the advancing planet, at egress, may be made to disappear, from sheer inability to appreciate so faint a light, before the contact would appear to be established to observers who had not so reduced the brightness of the image. Disturbing causes such as these do exist, and their effects must be recognised. I must apologise for bringing forward such arguments; but, since one observer has published his opinion that the "ligaments, &c.," do not exist in contacts, because he looked at the transit of Mercury with an opera glass and saw nothing of the kind, it would appear necessary to recall attention to a common-sense view of the points at issue.

But whatever may be the opinion of Mr. Newcomb respecting the explanation which I have given of the probable reason why some of the observers have not seen the connecting ligament, he must feel that it will at least be difficult for him to explain away the positive evidence of the numerous observers who profess to have seen the ligament with first-rate telescopes. Some of them, at least, were gentlemen not likely to have forgotten to adjust their eye-pieces to focus, even if such a neglect would have produced the phenomena observed. In cases where the ligament has been seen, it will be found that the earlier lines of contact at egress have been given by observers with the best telescopes and high powers. This is strikingly shown by the Greenwich observations of the transit of Mercury, 1868. It is a result in perfect accord with my views. Very large and systematic discordances will be found to exist between the times of internal contact at the transit of Mercury, 1868, in cases where no connecting ligament was seen at all. This has been passed over in silence by Mr. Newcomb; but it is important. It would be difficult to select from such groups of observers—the French, for example, who saw no connecting ligament—those who saw "the phenomena exactly as we know they are;" and unless this can be done, I am afraid that Mr. Newcomb's somewhat unique argument upon this point might be made as easily to prove the converse as the result he deduces from it. All these observations, in my view, are good; but they are not strictly observations of the same phenomena.

Mr. Newcomb rejects at once the force of the evidence of the observers of the transits of Venus, 1761 and 1769, upon the question of the connecting ligament "till we have better evidence than now exists that their object-glasses were such as Clarke or Foucault would call good." The phenomena connected with the ligament must be far more marked in the case of Venus than in that of Mercury, on account of the large diameter of Venus. To reject therefore by an impossible condition all the evidence in our possession respecting transits of Venus is certainly a bold step; but Mr. Newcomb appears to me to attach far too much importance, so far as irradiation phenomena are concerned, to the improvements effected in modern telescopes. The image of a point of light on the most perfect object-glass which can be conceived is not a point, but a disc, of which the illumination degrades rapidly from the centre, and which is surrounded by concentric rings of light. The law of degradation of the illumination of the central disc has been given by me in the Monthly Notices, November 1865. The result of theory upon these points has been most completely tested by experiment. The existence and regularity of these concentric circular rings is one of the most delicate tests of the perfection of a telescope. Since we have a disc of light corresponding to a point in the most perfect object-glass which can be made, the visible image of the sun formed by such a glass will not terminate with the geometrical image. This result of theory is confirmed by experiment. The optical enlargement found under degrees of illumination similar to those very commonly adopted in observations of the sun is amply sufficient to produce by its destruction near the point of contact the phenomena which so many observers of experience have declared that they have seen. That the optical enlargement is sufficient for the purpose can be seen from the experiments of Dr. Robinson, of Armagh, and from the Greenwich discussions of eclipse observations. This was pointed out in my letter in your number of August 24. With respect to Mr. Newcomb's remark as to the application of this theory of irradiation to a transit of a planet, viz., "we require to know whether the irradiation of an extremely minute thread of light darkened so as to be barely visible is the same with that of a large disc, I am decidedly of opinion that it is not, and if not, the fact that the sun's disc is optically enlarged by the telescope or the eye of the observer cannot be directly applied to the phenomena of transit." I have merely to remark that Mr. Newcomb is undoubtedly right when he asserts that the irradiation from the minute thread of light darkened so as to be barely visible is not the same as that of the large disc. *It is simply because such is the case that the phenomena of the connecting ligament appear.* When the planet is well on the disc, the irradiation around the disc will not be disturbed, but as the planet approaches the edge, the irradiation near the point of contact must eventually be disturbed, and this disturbance, or change, gives rise to the phenomena observed—a black drop, connecting ligament, or whatever name you prefer to give to that apparent cutting out of a piece of the sun's edge near the point of contact which must take place. After the disturbance of the irradiation has once commenced, the connecting ligament must at egress increase in breadth; but I do not profess to be able to give the law of the changing form with any degree of exactness.

The experiments of Wolf and André were, as I stated in my letter of August 24, made upon a disc presenting no sensible traces of optical enlargement. The results can therefore have no bearing upon the question of irradiation. These results are undoubtedly valuable in themselves, as showing experimentally the tendency of errors of observations of contacts under feeble illumination. They may throw light upon those observations at which no connecting ligament was seen, but they are useless to disprove or prove irradiation effects.

My authority for stating that the observations of Wolf and André were made upon a disc showing no sensible traces of optical enlargement, is contained in the memoir itself. If Mr. Newcomb is pleased to call the phenomena of "telescopic irradiation" a species of bad definition, there can be no objection on my part to his doing so; but it is not a species of bad definition "which disappears in a fair atmosphere, with a good telescope well adjusted to focus."

With respect to the ligament not being a celestial reality. The contact is not a celestial reality. My views of the reality of the phenomena are that the reality is neither more nor less than the reality of the phenomena presented at the focus of an object-glass when turned upon a star. The irradiation can in my view be got rid of to the same extent and in the same manner that the central disc corresponding to the star's image can be got rid of. You can reduce its dimensions by cutting down the illumination, and the disc will become a point, but only as it vanishes.

If I may be allowed to give one word of warning respecting the preparations for the transit of Venus 1874, it is *uniformity.* Make such arrangements as you think best, but once made stick to them even if not absolutely the best. The observations which are to be compared must be made as early as npossible under the same optical conditions. The whole success or failure of the work will, in my opinion, turn upon the extent to which this necessary condition is approximated to.

E. J. Stone

Royal Observatory, Cape of Good Hope, Nov. 18, 1871

The Origin of Insects

With your kind permission I will answer Dr. Beale's questions, published in his letter in your issue of December 21, 1871.

Dr. Beale asks me what part of the nervous system of the maggot is present in the fly? My answer is that I have traced the changes of the one directly into the other; and that Weismann has done the same. There is no time in the pupa state when the nervous system is absent; but it is difficult to demonstrate this, as amongst so much molecular matter it is not easily found, and is very easily crushed and destroyed.

Again, Dr. Beale asks me if I have seen any vestige of the mouth organs of the imago in the larva? I reply that the mandibles and maxillæ exist in the egg twelve hours before the young maggot emerges, together with the fore and hind-head segments; that these have all disappeared when the egg hatches; but that the imaginal discs are already formed at that time. Now, I would ask if it bears the slightest aspect of probability that the larval head segments and mandibles, maxillæ, &c., are formed for nothing, and that the imaginal discs are new formations arising contemporaneously with the disappearance of the larval head segments? Dr. Weismann has shown unmistakably that the abdominal segments of the pupa skin are formed from the abdominal segments of the larval skin. Does it appear in the slightest degree probable that the thoracic and head segments have a totally dissimilar origin? I admit that I have not been able to see the imaginal discs in contact with the head segments of the embryo; but I have found the imaginal discs immediately after the egg is hatched, and they are then too much like the embryonic structures alluded to, to have had any other probable origin. The proboscis is formed from cells laid down within these discs; of that there is not the smallest doubt. Dr. Weismann makes the same assertion, and, although I did not know it to be so at the time I wrote my work on the fly, I acknowledge it is so now, and that in my description of the origin of the proboscis I was wrong. In the Lepidoptera, and in some beetles, imaginal discs may be seen to have their origin in the inner layer of the larval skin.

Again, Dr. Beale says:—"Does Mr. Lowne mean to say, for instance, that he or any one else can adduce any reliable observations to prove that the sexual organs are gradually developed, even from the time when the embryo is enclosed within the egg?' I answer, yes. My own observations confirm those of Weismann on this head, and Dr. Beale will find, on looking again at page 112 of my book on the blow-fly, that he has not correctly quoted my statement. I will also refer Dr. Beale to Dr. E. Bessel's paper, "Studien über die Entwicklung der sexual-Drüsen bei den Lepidopteren," in the "Zeitschrift für wissenschaftliche Zoologie," vol. xvii. I believe future observers will find the sexual organs are always so formed, even as they are in the vertebrata. There is another paper, by Siebold I think, on the same subject in the above-quoted periodical.

Lastly, Dr. Beale asks me to explain what I mean by the sentence occurring at page 116 of my book:—"All the tissues of the larva undergo degeneration, and the imaginal tissues are redeveloped," &c. I apprehend that the redevelopment of all the tissues does not imply also the redevelopment of the insect. That the tissues are all so redeveloped is undoubted, but they are not all redeveloped at once. I have stated in my book again and again that certain organs are redeveloped in a particular manner, and was never under the impression that the whole was a case of alternate generation. I did not know the origin of the imaginal discs in those days.

With your permission I will add a few words in support of the assertion "that the pupa change is analogous to ordinary ecdysis, of which it is a modification." In ordinary ecdysis the muscles undergo degeneration at their points of attachment to the cast skin; in metamorphosis this change is far more marked. In ecdysis in Chloëon, for instance, Sir J. Lubbock (Linn. Soc. Trans., vol. xxiv.) has shown that the wings and thorax are gradually developed through nine successive sheddings of the skin. In the more remarkable metamorphosis of Lepidoptera they are developed in two ecdyses, these two being called metamorphosis. Prof. Owen believed, and the assertion is now widely known, that the larvæ of such insects as the Orthoptera, Neuroptera, &c., exist in the maggot form in the egg; but the observations of Mr. Newport on Meloe, and of Fritz Müller, of Weismann, and many others, go far to prove that this is not so—that the maggot form is intermediate, the half-developed embryo and the pupa or perfect insect, being most alike.

The subject is one of great interest, and therefore I trust you will excuse this long trespass on your pages.

99, Guilford Street BENJAMIN T. LOWNE

In Re Fungi

YOUR sarcastic correspondent "F. L. S." is quite incompetent to reply to my former letter. I did not call in question the correctness of the determination of *Agaricus cartilagineus*, but merely drew attention to the absurdity of the statement that the said determination was made from a mere "mass of mycelium," and that such a statement should come from a journal specially devoted to Botany.

In the original report of the occurrence of *Agaricus cartilagineus* (*Journal of Botany*, vol. iii. p. 28) special reference is there made to the "many-headed pileus;" now some of these "pilei" (not the "mycelium," "F. L. S.,") were forwarded to the Rev. M. J. Berkeley for examination, and from *these materials* he (and not the writer of these lines) made out the plant to be *A. cartilagineus*. Certainly I included the species "without hesitation" in the list of Middlesex Fungi, because I knew the plant referred to had not been determined from a mere "mass of mycelium," but that Mr. Berkeley had examined the perfected parts.

I fail to see why "F. L. S." is so anxious to "allay my alarm as to the decay of Fungology in England," especially as I have never expressed any "alarm" on that head. I do not look upon the *Journal* as such an infallible weathercock as to connect its wrong statement with a national breakdown in Botany; neither do I see how I have "missed the point" of its paragraph. I am more inclined to think that I have *hit it* in a friendly way, and rather hard too. W. G. S.

Mr. Baily on Kiltorkan Fossils

IN your last number Mr. Baily is said to have brought forward at a meeting of the Geological Society of Dublin "some strong facts to prove that the Irish palæontologists had not misled Prof. Heer, as stated by Mr. Carruthers at a recent meeting of the London Geological Society."

At the meeting referred to, Prof. Heer placed the Irish beds at the base of the Carboniferous series, mainly because *Sagenaria Veltheimiana*, a coal measure plant, was found in them.

Into this error I said "Prof. Heer had been led chiefly by the erroneous determination of the Kiltorkan *Lepidodendron* by the Irish palæontologists." I will not burthen your columns with the strange history of the nomenclature of this plant, as I shall have an opportunity of doing this elsewhere ere long. The point before us is this, that Mr. Baily alone has the credit of erroneously determining the Kiltorkan plant to be the same as an already described Carboniferous species. And the proof of this is easily adduced. In 1864, Mr. Baily, in his "Explanation of Sheets 187, &c., of the Irish Survey," figures the fossil, and describes it unhesitatingly as "*Sagenaria Veltheimiana*, Sternb. sp." This he repeated in a paper by the lamented Prof. Jukes in 1866 (*Journ. Geol. Soc. Ireland*, i. pp. 123, 124), as well as in a paper by himself read to the Natural History Society of Dublin in the same year (p. 2). Prof. Heer acknowledged his obligations to Mr. Baily for the Irish specimens he had examined. I have examined specimens so distributed by Mr. Baily, and they were named *Sagenaria Veltheimiana*.

In the volume of the British Association Reports, published in 1869, Mr. Baily says (p. 59) that the *Sagenaria* is named by Schimper *S. Bailyana*. More recently (Nov. 1871), in his "Figures of British Fossils" (p. 84), he names it *Knorria Bailyana*. It is not much to the purpose to say that it is neither a *Knorria* nor a *Sagenaria*, or further that the specific designation *Bailyana* must give place, with some dozen other synonyms, to the original name given by Dr. Haughton in 1855. But it is to the purpose to notice that *Sagenaria Veltheimiana* is *not* a Kiltorkan fossil, though said to be so by Mr. Baily, and that this error, now acknowledged by Mr. Baily himself, was the main foundation of Prof. Heer's argument.

I am not a little curious to know what are the "strong facts" which will overthrow a plain narrative that fully justifies my statement, but at the same time compels me to make it more personal than the truth seemed to me to demand when I made it some months ago. WILLIAM CARRUTHERS

ZOOLOGICAL RESULTS OF THE ECLIPSE EXPEDITION

A STEAMER is eminently unqualified for observations on marine zoology. Owing to the high rate of speed, it is impossible to use a towing net with any success, and to a zoologist it is perfectly tantalising to see swarms of Medusæ, &c., sail past the ship without being able to obtain a single specimen. In Peninsular and Oriental ships the only practicable method is to keep the tap of the baths constantly running through a fine gauze net. In this way quantities of Entomostraca may be obtained. Since we have been in the Red Sea, the water has been splendidly phosphorescent every night, the light being most brilliant where the hot water from the condensers is shed out into the sea, the animals being probably killed by the heat, and emitting in the act one last brilliant flash. If the water be turned on into one of the baths at night, most gorgeous flashes of light are obtained, and the animals causing them may be caught in small vessels and kept for examination. They are at present almost exclusively Entomostraca of the genera *Cypris*, *Cyclops*, and *Daphnis*. When the light is examined spectroscopically, it gives a spectrum in which only the green and yellow are present, the red and blue being sharply cut off. Several species of the Entomostraca obtained contain a brilliant red pigment, which gives unfortunately no absorption bands when examined with the micro-spectroscope. At Suez I obtained a number of Echinodermata of the usual dark purple tint, a splendid *Comatula* in abundance, two species of *Echinus*, and one or two star-fishes. The colouring matter of these animals is readily soluble in fresh water or alcohol, as is that of the common British feather-star. Though its colour is extremely intense, it gives no absorption bands, but when a strong solution is used, the spectrum is reduced to a red band, all the rest of the light being absorbed. Apparently parasitic on a large flat *Spatangus*, were obtained a number of *red* Planarians, about one-eighth inch long, which gave the characteristic absorption bands of hæmoglobin with great intensity. The existence of hæmoglobin in Planarians is a fact of considerable interest, and I believe quite new. On taking a boat excursion round the shores, where I obtained abundance of large Gasteropods and the Echinodermata mentioned above, I was remarkably struck by the absence of Actinias. Though I was out nearly the whole day, I did not see a single specimen, nor indeed did I observe any large Medusæ. This absence of these latter may perhaps, however, have been due to the set of the wind or tide.

Of the Suez Canal fauna we were able to observe very little, except that the canal perfectly swarms with fish from one end to the other. A good many were taken with hand-lines in two spots, one close to Port Saïd, the other in the middle of the Great Bitter Lake. They were all of one species, a sort of mullet, but there are no books at hand to determine the species. The mud brought up from the bottom of the Great Bitter Lake by the chain cable was absolutely devoid of any traces of life. The *Mirzapore* has been visited on her voyage by various land birds. One hen chaffinch accompanied us from Cape Finisterre to Port Saïd, not leaving the ship when she was anchored at Malta, and was to be seen every day hopping about the deck and feeding. At present the ship is surrounded by vast flights of flying fish. They fly generally up wind, and sometimes go as far as one hundred yards.

It is hoped that this short note may be found of some interest, and that it will be borne in mind that it is impossible to travel about with a library sufficient to determine species on the spot.

H. N. MOSELEY

MELTING AND REGELATION OF ICE

An observation made yesterday caused me to present to my class, in a lecture on Heat this morning, the following experiment. A piece of wire gauze was laid on a convenient horizontal ring, and on this a lump of ice. A flat board was placed on the ice, and pressure was applied by means of weights put upon the board. I put 12 lbs. upon a piece of ice as large as an apple. This was done at the commencement of the lecture, and before the conclusion I found a considerable quantity of ice on the lower side of the gauze, apparently squeezed through the meshes. The temperature of the class-room was about 15° C. (59° Fah.). The experiment was continued for eight or ten hours, fresh ice being supplied when necessary to the upper side of the gauze, and, in spite of the continual surface melting and dripping away of water, a very large quantity of ice was formed below the gauze. The ice below the gauze was firmly united to that above. I tried with my hands to break away the upper from the lower, and to break either of them off at the place where the wire gauze separated them; but I was not able to do so. The ice that has passed through the meshes has a kind of texture corresponding to that of the network, and the small air bubbles appeared to be arranged in columns.

The phenomenon is a consequence of the properties, announced from theory by Prof. James Thomson, and then exemplified by an experiment; and the explanation depends on the theories put forward by him—the first (1857) founded on the lowering of the freezing point of water by pressure, and the second (1861) founded on the tendency to melt given by the application to the solid ice of forces whose nature is *to produce change of form* as distinguished from forces applied alike to the liquid and solid. The stress upon the ice, due to its pressure on the network, gives it a tendency to melt at the point in contact with the wire, and the ice, in the form of water intermixed with fragments and new crystals, moves so as to relieve itself of pressure. As soon as any portion of the mass is thus relieved, freezing takes place throughout it, because its temperature is reduced below that of the freezing point of water at ordinary pressures, by melting of contiguous parts. The obvious tendency of the ice under the pressure from above is thus, by a series of meltings and refreezings, to force itself through the meshes.

The next experiment that I tried I was led to by that just described. I supported a block of ice on two parallel boards, placed near to each other, and passed a loop of wire over the ice. The loop hung down between the boards, and weights were attached to it. The first wire tried was a fine one (0·007 inches diameter) and a two-pound weight was hung on the loop. The wire immediately entered the ice, and it passed right through it and dropped down with the weight after having done so, but it left the ice undivided, and on trying it with a knife and chisel in the plane in which the cutting had taken place, I did not find that it was weaker there than elsewhere. The track of the wire was marked by opacity of the ice along the plane of passage. This opacity seemed to be due to the scattering of air from the small bubbles cut across by the wire. I have not, however, been able to try a piece of ice free from bubbles; and, from the nature of the experiment, air may very possibly pass in along the wire from the outside. I next experimented with a wire 0·024 inches diameter, weighting the loop with 8 lbs., and obtained a similar result; and, finally, I took a wire 0·1 inch diameter, and, putting a 56lb. weight on a loop of it, I caused it to pass through the ice, and the block remained undivided. This, though it follows from theory, has a most startling effect; and during the passage of the thick wire through the ice, I was able to see the bubbles of air across which it cut rising up round its sides. I made careful trials to cut the ice with a knife in the lamina through which the wire had passed, but found no weakness there.

A string was next tried, but, as might be expected, it did not pass through the ice. I considered that the string was not a good enough conductor to relieve itself of the cold in front and pass it back to the water behind. The capillary action of the string also doubtless takes part in the production of the result. It simply indented the ice and froze into it.

On this point of the necessity for a good conductor, and for a way of relieving itself of the cold, a curious observation was made. In one case a thick wire appeared to have stopped (this requires confirmation) as if it were frozen into the ice. On examination it turned out that the ice was so placed that the water formed by the pressure of the wire had flowed away at the first, and a hole was left behind the wire. On supplying a few drops of water to the place from a small pointed bit of melting ice, the water froze instantly on coming in contact with the wire, and the wire moved forward as usual. By this I was also led to try putting a thick wire over a piece of ice having a hollow at the top, so that the wire cutting into the shoulders bridged across the hollow between them. Looking at the wire, which was in front of a window, I dropped some ice-cold water on it, and saw it *freeze instantly* into crystals on the parts of the wire near to the shoulders on which it was pressing. This is notable as the first experimental confirmation of Prof. Thomson's theory on the production of cold by the application of stress.

I have not yet had an opportunity of trying these experiments at a temperature lower than freezing. The amount of pressure necessary to make the wire pass through the ice would of course be very much increased as the temperature is lowered, and it would finally be impossible to cut the ice without breaking it up like any other hard solid. Indeed I saw in one case in which I had a very great weight (80lb. or so) on a thick wire, the ice cracking in front of the wire; apparently the wire was forced too fast through the ice.

These experiments seem to me to have considerable importance in relation to the sliding motion of glaciers. The smallness of the cause has been raised as an objection to the theory of Prof. Thomson. But no one can see the experiments I have described, particularly the first, where a large quantity of ice is squeezed through the meshes of fine wire gauze under small pressure and in a short time, without feeling almost surprised at the slowness of the glacier motion.

JAMES THOMSON BOTTOMLEY

Glasgow University, Dec. 20, 1871

ELECTROPHYSIOLOGICA:

BEING AN ATTEMPT TO SHOW HOW ELECTRICITY MAY DO MUCH OF WHAT IS COMMONLY BELIEVED TO BE THE SPECIAL WORK OF A VITAL PRINCIPLE

I.

ON a white marble slab let into the front of a house in the Strada Felice at Bologna is an inscription showing that, in this house, then his temporary dwelling-place, at the beginning of September 1786, Galvani discovered animal electricity in the dead frog, and hailing this event as the well-spring of wonders for all ages (Luigi Galvani in questa casa di sua temporaria dimora al primi di Septembre dell' anno 1786, scoperse dalle morte rane La Ellettricita Animale—Fonte di maraviglie a tutti secoli). Animal electricity, well spring of wonders for all ages! Yes, said I, as I copied these words a few weeks ago, and as I went into the house repeating them to myself. Yes, still said I, after seeing what was to be seen within the house. Within the house, indeed, there was much to excite the imagination, and to make me more ready to accept these words as the sober utterance of simple truth. Still the same were the common stairs leading from the open outer door to the landing on the first floor, with its two main doors, one on each side, each one opening to a distinct set of apartments, in one of which had lived the discoverer of animal electricity; and the only change of moment was one which served to call back more vividly the memorable past—a portrait in lithograph of Galvani himself hanging upon the wall facing the stair-head. Still the same was a third and smaller door, at which the portrait seemed to be looking, and beyond which were the stairs leading to the belvedere on the roof so common in Italian houses hereabouts. Still the same were these stairs, the lower flights of uneven bricks, the upper of ricketty woodwork, unmended, scarcely swept, since the time when Galvani went up and down them afire with the discovery made in the belvedere to which they led. Still the same was the belvedere itself—the same walls, blank on one side, pierced on the three others with arched openings, two at each end, three at the front, each opening being built up breast-high so as to form the parapet—the same roof overhead with its bare rafters and tiles—and, running across each opening a little below its arched top and parallel with the parapet, the very same iron bar upon which the frogs' limbs had been suspended by copper hooks in the experiment to which the inscription on the slab outside the house refers, and about which Galvani wrote:—"Ranas itaque consueto more paratas uncino ferreo earum spinali medulla perforata atque appensa, septembris initio (1786) die vesperascente supra parapetto horizontaliter collocavimus. Uncinus ferream laminam tangebat; en motus in rana spontanei, varii, haud infrequentes! Si digito uncinulum adversus ferream superficiem premeretur, quiescentes excitabantur, et toties ferme quoties hujusmodi pressio adhiberetur." So little change was there, indeed, that, forgetting the present altogether, I could only think of this experiment in which the existence of animal electricity was divined, and of those myriad other experiments to which it had led, and by which in the end the truth had been made manifest. So absorbed was I in these thoughts that I even forgot to look through the open arches of the belvedere at the blue Italian sky and the other beauties of the prospect. And when at length I came down, I was more than ever in the mind to assent unhesitatingly to the words, "la ellettricita animale, fonte di maraviglie a tutti secoli"—more than ever convinced that animal electricity would prove to be the key by which to unlock not a few of the secrets which are supposed to be exclusively in the keeping of life—more than ever resolved still to go on seeking for truth in the path along which I was urged to go by this conviction.

Nor was I long at a loss how to begin to carry out this resolution. I wanted to reiterate briefly and more clearly some of the things which I had said before respecting animal electricity, and the way in which this force may do a work ascribed to life in muscular action and nervous action; and at the same time to make use of certain new facts which were not a little calculated to confirm former conclusions. I wanted to show that the same workings of animal electricity may be detected in the condition called tone, and even in growth, and that these processes, no less than muscular action and nervous action, may have to be looked upon as electrical rather than as vital manifestations. A natural way of carrying out the resolution I had formed was, indeed, to do the work ready for me; and therefore the task I have now set myself is to do this work, beginning with an attempt to set forth a new theory of animal electricity, and then proceeding to say something in turn on the way in which this theory sheds light upon muscular action, nervous action, the maintenance of the state called tone, and the process of growth in cells and certain fibres—something calculated to show that in each of these cases animal electricity may have to do much of what is commonly believed to be the work of a vital principle.

1. *On a theory of animal electricity which seems to arise naturally out of the facts.*

A current, to which the name of muscle-current is given, may easily be detected in living muscle. It may be detected by applying the electrodes of the galvanometer, the one to the surface made up of the sides of the fibres, the other to that made up of either one of the two ends of the fibres, and also, though much less clearly, by examining either of these two surfaces singly, provided only the two points to which the electrodes are applied are at unequal distances from the central point of the surface. It may not be detected, if, instead of applying them in this manner, the electrodes are applied so as to connect either the two surfaces made up of the ends of the fibres, or two points equidistant from the central point of the surface made up of the sides, or of that formed by either one of the ends of these fibres. A current may or may not be detected under such circumstances, and when it is detected its direction is such as to show that the surface made up of the sides of the fibre is positive in relation to that made up of either one of the two ends, and that the former surface is more positive and the latter more negative as the distance increases from the line of junction between these two surfaces. In this way the galvanometer makes known the existence of points of similar and dissimilar electric tension in living muscle; and the only inference from the facts would seem to be that there is a current when the electrodes are applied so as to bring together points of dissimilar tension, but not otherwise. The facts are not to be questioned. The inferences arising from them can scarcely be mistaken.

This current is to be detected in living muscle, but not in muscle which has passed into the state of rigor mortis. As muscle loses its "irritability," indeed, it ceases to act upon the galvanometer, and no trace of the current is to be met with after the establishment of rigor mortis. As a rule, too, nothing is to be noticed except a gradual failure of current; but now and then (though not in the frog) there may be a reversal in direction in the last moments preceding the final disappearance.

When muscle passes from the state of rest into that of action, there is also a change in the muscle current to which the name of "negative variation" is given by its discoverer Du Bois-Reymond. Thus, when a galvanometer is connected with the gastrocnemius of a frog so as to respond to its muscle-current during the two states of rest and action in the muscle, the needle, which may have stood at 90°, or thereabouts, during the state of rest, is seen to fall back, and take up a position at 5° or nearer still to zero, during action. This change it is which is spoken of as "negative variation." It is a change indicating, not reversal of the current, but simple weakening; for the idea of reversal, which is readily suggested to the mind by the way in which the needle swings back past zero when the state of action is first set up, is at once corrected by the position which the needle takes up a moment or two later, and also by the fact that when the muscle-current of the *contracted* muscle is admitted into the coil of the galvanometer while the needle is resting at zero—when, that is, the experiment is not complicated by the muscle-current of the *relaxed* muscle being in the coil when the state of contraction is set up in the muscle—the needle is found to move in the same direction as that in which it moved under the current of the *relaxed* muscle, but not to the same distance from zero by a very great deal. So that, in fact, this "negative variation" of the muscle-current is nothing more than a sudden disappearance or failure of this current, and no good is gained by retaining a name which only serves to confuse and perplex.

Substituting the new quadrant electrometer of Sir William Thomson for the galvanometer, tensional changes are detected which are in every way parallel with the current changes which have been mentioned.

With this instrument, it is found that the surface made up of the sides of the fibres in living muscle, and that made up of either one of the two ends of these fibres, are in opposite electrical conditions, the ray of light marking the movement of the aluminium needle passing in the direction indicating positive electricity under the charge supplied by the former surface, and in the direction indicating negative electricity under the charge supplied by the latter surface—passing, that is to say, not in one direction only, as it would do if the needle were acted upon by charges differing, not in kind, but in degree only, but to the right in the one case and to the left in the other. It is found, indeed, not only that the surface made up of the sides of the fibres of living muscle is positive, and that made up of either end of these fibres negative; but also that the former surface is more positive and the latter more negative as the distance increases from the line of junction between these surfaces. With this instrument, too, it is found that these indications of free electricity fail *pari passu* with this failure of the "irritability" of the muscle, that they have disappeared altogether before the advent of rigor mortis, and also that there is a change which serves to point to discharge, more or less complete, when muscle passes from the state of rest into that of action. Thus—in illustration of this latter fact—if the ray of light on the scale stand at 30° under the charge supplied to the electrometer by either one of the two surfaces of living muscle during the state of rest, it will stand at 5° only, or still nearer to zero, under the charge supplied by the same surface during the state of action. The difference is always marked, and always of the same character; and, being so, the proof of discharge during action would seem to be as complete as may be, seeing that the instrument only takes cognizance of electrical changes of the nature of charge and discharge.

These, then, are the facts which may be looked upon as fundamental. There are the facts brought to light by Du Bois-Reymond through the instrumentality of the galvanometer—the muscle-current, present in living muscle during the state of rest, suddenly disappearing when the state of rest changes for that of action, gradually disappearing as muscle loses its "irritability," and absent altogether in rigor mortis; there are the facts which I myself have been able to make out for the first time by means of the wonderfully sensitive new quadrant electrometer of Sir William Thomson—the two opposite charges of electricity, one positive, the other negative, present in living muscle during the state of rest, disappearing suddenly when this state changes for that of action, gradually disappearing before, and altogether absent in, rigor mortis. And this is all that need be said upon this subject at present.

And as in muscular so in nerve tissue, there is the current, in this case called the nerve-current, and there are the two opposite charges, positive and negative, this current and these charges being present during life, disappearing suddenly when the state of rest changes for that of action, disappearing gradually *pari passu* with the "irritability," and absent altogether at the time when rigor mortis has seized upon the muscles; and in truth every particular in the electrical history of the muscle is repeated with strict exactness in the electrical history of the nerve.

In these two tissues, muscle and nerve, there is no difficulty in arriving at a knowledge of these facts; in other tissues the case is different. In other tissues, indeed, all that can be said is that faint indications of electricity are to be detected during life only, and that in some of the fibrous structures there are differences between the surface made up of the sides of the fibres and that made up by either one of the two ends, which correspond to those met with in muscle and nerve.

These then being the fundamental points in the history of animal electricity, the question is as to their meaning. To what theory do they point?

In order to account for this muscle-current and nerve-current, Dr. Du Bois-Reymond supposes that the muscle-fibre and nerve-fibre (the same law applies absolutely to both) are made up of what he calls peripolar molecules—of molecules, that is to say, which are (with the exception of certain moments in which these electric relations may be reversed) negative at the two poles and positive in the equatorial belt between those poles. He supposes that the sides of the fibres are positive because the positive equatorial belts are turned in this direction, and that the two ends are negative because the negative poles of the molecules face towards the ends. He supposes also that the muscle-current and nerve-current are merely the outflowings of infinitely stronger currents ever circulating in closed circuits around the peripolar molecules of the muscle and nerve respectively. And this view no doubt has much to recommend it.

But another view may be taken of this matter—a view according to which this electrical condition of living muscle and nerve during rest is, not current, but static; and this view is that which recommends itself to my mind as in every way more simple, more comprehensive, and more to the point practically.

In taking this view the great resistance of the animal tissues to electrical conduction serves as the starting point. I assume that parts of these tissues may be bad enough conductors to allow them to act as *dielectrics*. I assume that the parts which are thus capable of acting as dielectrics are the sheaths of the fibres in muscle and nerve, or the cell-membrane of the contractile cells of those fibres in muscle which have no proper sheath. I assume that a charge, usually the negative, may originate in the molecular reactions of the contents of the sheath or cell-membrane, and that this charge, acting upon the inner surface of the sheath or cell-membrane, may *induce* the opposite charge upon the outer surface of the sheath or cell-membrane, and that in this way the sheath or cell-membrane during rest is virtually a charged Leyden-jar. I assume that this charge is discharged when the state of rest changes for that of action. I assume that the surface made up of the sides of the fibres in muscle and nerve is positive because positive electricity has been *induced* upon this surface, and that the surface made up of either cut-end of the fibre is negative, because the negative electricity, developed upon the inner surface of the sheath or cell-membrane, is *conducted* to these ends by the contents of the sheath or cell.

All that I assume, indeed, may be readily illustrated upon a small cylinder of wood, left bare at its two ends, and having its sides covered with a coating which may be charged as a Leyden-jar is charged—a threefold coating, formed of an inner and outer layer of tinfoil, with an intermediate layer of gutta-percha sheeting, the latter layer projecting a little towards the two ends of the cylinder, so as to secure the necessary insulation of the inner and outer metallic surfaces; for by charging the inner layer of foil with negative electricity, this cylinder, which may be regarded as a model of a muscular fibre, is found to be, not only positive at the sides and negative at the two ends, but more positive at the sides and more negative at each end as the distance increases from the line of junction between the sides and ends. With this model thus charged, indeed, it is easy to imitate all the phenomena of the nerve-current and muscle-current, provided the electrodes of the galvanometer be applied in a suitable manner, and the charge kept up. With this model thus charged, it is also easy to imitate all the tensional phenomena of nerve and muscle which are made known by the electrometer. And thus the nerve-current and muscle-current, instead of being out-flowings of infinitely stronger currents ever circulating around peripolar molecules, may be secondary phenomena only, the accidental result of certain points of dissimilar electric tension upon the surface of the fibres of muscle and nerve being brought into relation by means of the galvanometer or the electrometer, as the case may be.

In this view, I have assumed that certain parts of nerve and muscle were sufficiently bad conductors to enable them to act as dielectrics, but I had not, it is easy to see, the firmest ground for this assumption. It was certain that these tissues were bad conductors; it was not certain that they were bad enough conductors for my purpose. Here, then, was occasion for new work—for work which must be done before I could hope to gain a secure footing for my theory; and this, therefore, was the task I set myself a few months ago, and about which I have now to say something.

In this work I have made use of a Wheatstone's Bridge having on each side resistance coils of the value respectively of 10, 100, and 1,000 B. A. units, of a set of resistance coils capable of measuring up to 1,000,000 of the same units, and of a battery consisting of six medium-sized Bunsen's cells. With this apparatus I have measured the resistance of muscle, tendon, yellow elastic ligament, brain, and spinal cord, the portion measured in each case being a parallelogram an inch in length by $\frac{1}{20}$ of an inch in breadth, formed by making a slice with a Valentin's knife, of which the blades were $\frac{1}{20}$ of an inch apart, and then cutting a strip from the slice by moving the knife, with its blades still separated to the same degree, at right angles to its surface. In order to eliminate the resistance due to secondary polarity, I measured each of these bodies at ·25, ·50, and ·75 of the inch, as well as at the full inch, the fact being, as was pointed out by Sir Charles Wheatstone in his first great paper on the means of measuring electrical resistance, that while the resistance of a conductor increases with its length, the resistance due to secondary polarity remains the same everywhere. Thus, *at* ·25 it is impossible to say how much of the resistance met with belongs to the body itself, and how much to secondary polarity; but not so *after* ·25, *at* ·50, or ·75, or 1·0; for the resistance belonging to secondary polarity being the same at ·50, ·75, and 1·, as at ·25, it follows that by deducting the resistance at ·25 from the resistance at ·50, ·75, and 1·0 the difference at each of these points will represent the resistance of the body itself between ·25 and that particular point.

Of these measurements those which I made last of all will serve as well as any others for the text of what I have now to say, and these are as follows:—

	Inch.		B. A. units
Muscle (ox)	at ·25	=	17,000
	·50	=	27,000
	·75	=	36,000
	1·0	=	46,000
Tendon (ox)	at ·25	=	19,000
	·50	=	43,000
	·75	=	69,000
	1·0	=	99,000
Yellow elastic ligament (ox)	at ·25	=	160,000
	·50	=	300,000
	·75	=	820,000
	1·0	=	1,000,000 and more.
Brain (ox)	at ·25	=	11,500
	·50	=	16,100
	·75	=	23,000
	1·0	=	32,000
Spinal cord (ox)	at ·25	=	8,300
	·50	=	14,200
	·75	=	17,500
	1·0	=	22,500

I had made several measurements before these, corresponding more or less closely with them in results, and I was proceeding to make others, with a view to arrive at some common mean of numbers, when I found that the resistance went on continually altering, every moment becoming higher and higher, until in the end it was beyond the reach of my means of measurement.

Thus, in the strip of spinal cord, the resistance at ·25 inch, which at first was 8,300, was 180,000 in five hours, and more than 1,000,000 twelve hours later.

Thus, the resistance of the strip of brain, which at first was 11,500 at ·25 inch, was 25,000 five hours later, and upwards of 1,000,000 after the still further lapse of a dozen hours.

And so, likewise, with muscle, and tendon, and yellow elastic ligament, there was a corresponding increase of resistance when the measurement was repeated at these different times after the first trial.

Nor was this the only proof of a change of this sort; for on repeating these measurements on the same specimens some days later, after they had become thoroughly dried up, I found that the very shortest length which could be got for measurement—a length so short, that the two electrodes conveying the measuring current were all but touching—gave a higher resistance than that which could be gauged by the means at my disposal.

These, then, being the facts, it was evidently useless to go on searching for any numbers which could express anything like a common mean of resistance. It was evident, indeed, that the soft tissues, one and all, apart from moisture, were to be looked upon as insulators, rather than as conductors. Nay, it was possible that they might be insulators rather than conductors even in the fresh state; for it is quite supposable that in this fresh state the walls of the fibres and cells forming these tissues may be virtually dry, with moisture on each side, not with moisture percolating from side to side, and that the degree of resistance presented by these tissues, when fresh, is not that which would be encountered if the current passed *across* these walls, but that which is encountered by the current in passing along their outer moistened surface. It is quite supposable that the measuring current may not pass *across* the walls of the cells and fibres at all, but may glide over and between them only. All this is supposable; and therefore, the facts being as they are, I am, as I conceive, at liberty to assume that the walls of fibres and cells are sufficiently non-conducting to justify me in adopting the theory which I have ventured to propose—a theory, according to which, the electrical condition of muscle and nerve during rest is, not current, but static—the sheath of the fibre, or membrane, taking its place, being always charged as a Leyden-jar is charged, except during the time of action, when there is a discharge of this charge—a theory which, to say the least, has a less visionary foundation than that which rests on peripolar molecules seeing that it rests upon structural facts which cannot be called in question—a theory also which, as will be seen in due time, has this in its favour,—that it will simplify not a little several important problems in physiology.

C. B. RADCLIFFE

ICE-MAKING IN THE TROPICS

THE most marked example of the influence of radiation of heat on temperature is its influence on the production of artificial ice by the natives of India.

The fields in which the ice is made are low, flat, and open; and the ice is produced in large quantities when the temperature of the air is 16° or 20° F. above the freezing point; and the plan followed is an interesting example of accurate observation applied to practical purposes by a people now ignorant of science. The same process has been employed from time immemorial in India with scientific accuracy; and while the theory was explained by Dr. Wells,* the practical application was not so well understood; and this first led me to investigate the subject in India.†

The following method is employed by the natives of Bengal for making ice at the town of Hooghly near Calcutta, in fields freely exposed to the sky, and formed of a black loam soil upon a substratum of sand.

The natives commence their preparations by marking out a rectangular piece of ground 120 feet long by 20 broad, in an easterly and westerly direction, from which the soil is removed to the depth of two feet. This excavation is smoothed, and is allowed to remain exposed to the sun to dry, when rice straw in small sheaves is laid in an oblique direction in the hollow, with loose straw upon the top, to the depth of a foot and a half, leaving its surface half a foot below that of the ground. Numerous beds of this kind are formed, with narrow pathways between them, in which large earthen water-jars are sunk in the ground for the convenience of having water near, to fill the shallow unglazed earthen vessels in which it is to be frozen. These dishes are 9 inches in diameter at the top, diminishing to $4\frac{3}{10}$ inches at the bottom, $1\frac{3}{10}$ deep, and $\frac{3}{10}$ of an inch in thickness; and are so porous as to become moist throughout when water is put into them.

During the day the loose straw in the beds above the sheaves is occasionally turned up, so that the whole may be kept dry, and the water-jars between the beds are filled with soft pure water from the neighbouring pools. Towards evening the shallow earthen dishes are arranged in rows upon the straw, and by means of small earthen pots, tied to the extremities of long bamboo rods, each is filled about a third with water. The quantity, however, varies according to the expectation of ice—which is known by the clearness of the sky, and the steadiness with which the wind blows from the N.N.W. When favourable, about eight ounces of water is put into each dish, and when less is expected, from two to four ounces is the usual quantity; but, in all cases, more water is put into the dishes nearest the western end of the beds, as the sun first falls on that part, and the ice is thus more easily removed, from its solution being quicker.

There are about 4,590 plates in each of the beds last made, and if we allow five ounces for each dish, which presents a surface of about 4 inches square, there will be an aggregate of 239 gallons, and a surface of 1,530 square feet of water in each bed.

In the cold season, when the temperature of the air at the ice-fields is under 50° F., and there are gentle airs from the northern and western direction, ice forms in the course of the night in each of the shallow dishes. Persons are stationed to observe when a small film appears upon the water in the dishes, when the contents of several are mixed together, and thrown over the other dishes. This operation increases the congealing process; as a state of calmness has been discovered by the natives to diminish the quantity of ice produced. When the sky is quite clear, with gentle steady airs from the N.N.W., which proceed from the hills of considerable elevation near Bheerboom, about 100 miles from Hooghly, the freezing commences before or about midnight, and continues to advance until morning, when the thickest ice is formed. I have seen it seven-tenths of an inch in thickness, and in a few very favourable nights the whole of the water is frozen, when it is called by the natives solid ice. When it commences to congeal between two and three o'clock in the morning, thinner ice is expected, called paper-ice; and when about four or five o'clock in the morning the thinnest is obtained, called flower-ice.

Upwards of two hundred and fifty persons, of all ages, are actively employed in securing the ice for some hours every morning that ice is procured, and this forms one of the most animated scenes to be witnessed in Bengal. In a favourable night upwards of 10 cwt. of ice will be obtained from one bed, and from twenty beds upwards of 10 tons.

When the wind attains a southerly or easterly direction, no ice is formed, from its not being sufficiently dry; not even though the temperature of the air be lower than when it is made with the wind more from a northern or western point. The N.N.W. is the most favourable direction of wind for making ice, and this diminishes in power as it approaches the due north, or west. In the latter case more latitude is allowed than from the N.N.W. to the north. So great is the influence of the direction of wind on the ice, that when it changes in the course of a night from the N.N.W. to a less favourable direction, the change not only prevents the formation of more ice, but dissolves what may have been formed. On such occasions a mist is seen hovering over the ice-beds, from the moisture over them, and the quantity condensed by the cold wind. A mist in like manner forms over deep tanks during favourable nights for making ice.

Another important circumstance in the production of ice is the amount of wind. When it approaches a breeze no ice is formed. This is explained by such rapid currents of air removing the cold air, before any accumulation of ice has taken place in the ice-beds. It is for these reasons that the thickest ice is expected when during the day a breeze has blown from the N.W., which thoroughly dries the ground.

The ice-dishes present a large moist external surface to the dry northerly evening air, which cools the water in them, so that, when at 61°, it will in a few minutes fall to 56°, or even lower. But the moisture which exudes through the dish is quickly frozen, when the evaporation from the external surface no longer continues radiative; a more powerful agent then produces the ice in the dishes.

The quantity of dry straw in the ice-beds forms a large mass of a bad conductor of heat, which penetrates but a short way into it during the day; and as soon as the sun descends below the horizon, this large and powerfully-radiating surface is brought into action, and affects the water in the thin porous vessels, themselves powerful radiators. The cold thus produced is further increased by the damp night air descending to the earth's surface, and by the removal of the heating cause, which deposits a portion of its moisture upon the now powerfully radiating, and therefore cold surface of the straw, the water, and the large moist surface of the dishes. When better radiators of heat were substituted, as glazed, white, or metallic dishes, the cold was greater, and the ice was thicker, and the dishes were heavier in the morning than the common dishes. Any accumulation of heat on their surface from the deposit of moisture is prevented by the cold dry north-west airs which slowly pass over the

* Essay on Dew, 1814.
† Experimental Essay; Jour. As. Society, Calcutta, vol. ii, p. 80.

dishes. The wind quickly dries the ground, and declines towards night to moderate airs. The influence of these causes is so powerful that I have seen the mercury in the thermometer placed upon the straw between the dishes descend to 27°, when three feet above the ice-pits it was 48°.

So powerful is the cooling effect of radiation on clear nights in tropical climates, that in very favourable mornings, during the cold season, drops of dew may sometimes be found congealed in Bengal upon the thatched roofs of houses, and upon the exposed leaves of plants. In the evening the cooling process advances more rapidly than could be supposed by one who has not experienced it himself, and proves the justness of his feelings, by the aid of the thermometer. In the open plain on which the ice is made, I have seen the temperature of the air, four feet above the ground, fall from 70·5° to 57°, in the time the sun took to descend the two last degrees before his setting.

The tropical rains are succeeded by the cold season, when the night is cold, the sky quite clear, and the air becomes a bad conductor of electricity, from the dry northern winds which then prevail. This is proved by the rapidity with which evaporation proceeds, by the dispersion of clouds, and by the more evident proofs which the hygrometer exhibits. During the cold season vegetation proceeds, and electricity continues to be evolved by living bodies, and during their decomposition.

These remarks will enable us to explain the process by which the ice is prepared in Bengal.

1st. The large quantity of dry straw and moist dishes rapidly become cold, by their powerfully radiating surfaces, at the same time that the large body of dry straw strongly attracts positive electricity, and the descending currents of air deposit moisture in the dishes of water. Hence, during a cold and clear night, with airs from the N.N.W., the cooling process will advance more rapidly in proportion to the non-electric or attractive nature of the body, which, with the radiating power of the surface, regulates the cold and the quantity of dew deposited upon the body.

2nd. The high and dry situation and free exposure of the ice-fields to the sky, and the absence of all causes which could interrupt the influence of the large body of non-electrics, and the extensive surface of powerful radiating substances, sufficiently accounts for the degree of cold produced in the ice plates; and

3rd. The cool, dry north-west airs slowly pass over the ice-beds, absorbing the accumulation of moisture and of heat, which is given off by the liquefying of a large quantity of water that would otherwise accumulate over the beds; and, thus retaining the air clear and dry, allows the full operation of the other causes, particularly radiation.

T. A. WISE

NOTES

THE Academy of Sciences in Paris publishes the following telegrams received from M. Janssen. One dated Ootacamund, 18th December, 1h 6m P.M., says: "Great hydrogenous atmosphere very rare beyond chromosphere." The other, received on the 19th December by the Minister of Public Instruction, but not dated, simply says: "Eclipse observed; important results."—The Royal Academy of Sciences at Amsterdam has received the following telegram from one of its members, Dr. Oudemans, of Batavia:—"Preliminary results: Corona distinctly seen, pure white rays, dark rifts as far as the moon's limb; no outline of chromosphere; radial polarisation of Corona; no magnetic disturbances; moving shadows positively observed."

AT the meeting of the French Academy, held on Saturday last, to fill up the four vacant chairs, M. Thiers, M. de Remusat, Minister for Foreign Affairs, and M. Dufaure, Minister of Justice were present and voted. The first election was for a successor to Montalembert, and the Duc d'Aumale received 28 votes, one blank vote being recorded. For M. Villemaine's chair there were three candidates, M. Littré, who obtained 17 votes; M. Taillandier, 9; and M. de Viel Castel, 3. There were six candidates for M. Prévost-Paradol's chair. M. Camille Rousset had 17 votes; M. de Viel Castel, 7; M. de Mazade, 3; M. de Lomenie, 1; M. Taillandier, 1; and M. Mary-Lafon, 0. The choice of a successor to Prosper Mérimée was only made after two ballotings. At the first essay M. Edmond About obtained 13 votes; M. de Lomenie, 13; M. de Viel Castel, 2; M. de Mazade, 1; and M. Mary-Lafon, 0. At the second ballot M. de Lomenie received 15 votes, and M. Edmond About 14. Previous to the election a protest in the form of a lengthy pamphlet was distributed among the Academicians by the Bishop of Orleans, who, while professing the utmost respect for the personal character of M. Littré, declared that now, as in 1863, he opposed the admission into the Academy of one who in his writings was the defender of Materialism, Atheism, and Socialism. We learn that in consequence of M. Littré's election, Monseigneur Dupanloup has resigned his seat in the Academy.

WE greatly regret to hear of the death, announced by telegram, from choleraic diarrhœa, of the Venerable John Henry Pratt, M.A., Archdeacon of Calcutta. He was educated at Caius College, Cambridge, where he took his B.A. degree in 1833, when he was third wrangler, the Masters of Christ's and Sidney Sussex Colleges being also wranglers, with Dr. Boustead, afterwards Bishop of Lichfield. In 1838 he was appointed to a chaplaincy in connection with the East Indian Company, and in 1850 was nominated to the Archdeaconry of Calcutta, which he held up to the time of his death. He was well-known for his researches of the interior structure of the earth, and had been a frequent contributor to our columns.

DR. GUSTAV RADDE, Director of the Natural History Museum at Tiflis, has just returned to that town from an interesting journey to the head waters of the Euphrates. Mr. H. E. Dresser has received a letter from him, dated Tiflis, Dec. 14, from which we translate the following extract, viz.:—"Early in August I ascended, in company with Dr. Siewers, a young geologist, the Great Ararat, and we reached an altitude of 14,233 feet above the sea level. Our journey extended over three months, and we have brought back a splendid botanical collection, many good insects, and geological specimens. You will read full particulars ere long in Petermann's 'Mittheilungen.' As regards ornithology, I have not, I am sorry to say, time now to write further respecting the good materials we gathered together, and am just leaving home for another month."

THE Professors to the Newcastle-on-Tyne College of Physical Science have determined to institute evening classes, to commence immediately after the winter vacation, for the purpose of giving instruction in their respective subjects to persons who are unable to attend their day classes. The Professors wish it to be understood that the instruction given in these classes will be such as to require a certain amount of real study on the part of those who attend them.

THE Curator of the Clifton College Museum, Mr. Barrington Ward, has issued a circular asking for donations, to which we are glad to call attention. The following extract will show the very wise limitation placed on the acceptance of specimens:—"It has been decided, with the approval of the Head Master, that the museum shall be essentially a British one, and shall illustrate the natural history and antiquities of our land by good specimens, systematically arranged, under the departments of zoology, botany, geology, mineralogy, and archæology. In addition to this there will be a collection of rare and curious objects, derived from all sources, which may be considered useful for the purposes of scientific teaching, and a large typical series to be used at the lectures and demonstrations given in the College on Comparative Anatomy and other branches of Natural History. The committee of management will only accept of such specimens as can be classed under some one of these heads." In the Botanic Garden attached to the College nearly a thousand species of hardy herbaceous plants are now grown.

WE have received the Preliminary Report by Mr. Sidney I. Smith, on the dredging in Lake Superior; and a reprint from the *American Journal of Science and Art* for December, of Mr. S. I. Smith and Mr. A. S. Verrill's notice of the Invertebrata dredged in the same expedition. The main facts of these reports are already before our readers.

A SOCIETY of Arts, Sciences, and Letters, has just been started at Winona, Minnesota, in connection with the first State Normal School in that place, having for its object the collection of facts and materials looking toward the determination of the natural history, archæology, and general literature of the United States.

DR. HOY, in a paper read before the Wisconsin Academy of Sciences, Arts, and Letters, remarks, in reference to the mammals of Wisconsin, that the elk existed in that State as late as 1863, but is now probably extinct. The moose is still found in considerable numbers. The last buffalo was killed in 1832. Antelopes were also found in Wisconsin in the time of Father Hennepin, although now, of course, driven far to the west. Most of the wild animals are diminishing very rapidly in number, the panther and deer being almost exterminated. The otter and beaver, however, are very persistent. The last wild turkey was killed in 1846 near Racine.

A SCIENTIFIC commission in the interest of the Government of Peru has lately been investigating the guano deposits of the Lobos Islands; and it is reported that the result of their inquiries has been very satisfactory, and that immense quantities of very rich guano, equal, if not superior, to that of the Chincha Islands, have been observed. The analyses of samples are said to have yielded over 13 per cent. of ammonia. Should this be the fact, Payta, as being the nearest port, will probably become a place of considerable importance.

THE Report presented to, and read before, the Board of Visitors appointed by Government for the Royal Observatory, Edinburgh, after summarising the work done at the Observatory during the year, calls attention to the very inefficient manner in which the establishment is provided with funds for its necessary work, and to the scanty salary of its director and assistants. The Board of Visitors estimates the increased annual expenditure necessary to ensure the efficient working of the establishment at 1,050*l.*, including 300*l.* increase in the salary of the Astronomer Royal. The report is accompanied by a coloured plan of the Observatory, showing the position of the various instruments, and diagrams of the quarterly means of the earth thermometers from 1837 to 1869; annual mean temperatures, for four several sub-annual epochs, of the rock at the Observatory in the same years; annual means of Schwabe's sun-spots, the earth thermometers, and others at Edinburgh; and eleven-year means for every successive year, from 1842 to 1864, of Schwabe's sun-spots and Edinburgh earth temperatures.

THE seventh Report of the Board of Visitors of the Observatory at Victoria, with the Annual Report of the Government Astronomer, is printed. The report of the buildings and instruments is in every respect satisfactory.

MR. W. H. ARCHER has brought down his records of patents and patentees for the colony of Victoria to the end of 1869; and the Reports of the Mining Surveyors and Registrars for the same colony are printed for the quarter ending June 30, 1871.

THE Report of the New Zealand Institute for its fourth session, 1871, contains the Annual Address, delivered by Sir G. F. Bowen, and a list of donations and deposits in the Museum, and the Laboratory Report for 1870–71. Captain Hutton has prepared a complete catalogue, with a diagnosis, of each species of bird in New Zealand; and arrangements have been made for the publication of similar catalogues of the insects, fishes, and other branches of zoology in the island.

WE have received the last two Annual Reports of the Plymouth Institution and Devon and Cornwall Natural History Society, forming together vol. iv. of its Transactions. Though many of the papers and lectures reported refer to subjects which do not come within our scope, the volumes bear evidence of the zeal and success with which the natural and physical sciences are pursued in the Western counties. Among the papers specially deserving of mention, we may notice, "Degeneration of our Deep-sea Fisheries," by Mr. J. N. Hearder; "The Fulgarator," a new electrical apparatus for producing electric sparks of very great length, by the same; "Rain," by W. Pengelly, F.R.S.; "Mistletoe on the Oak," by T. R. Archer Briggs; "The principles on which ships' sail-carrying power and steadiness in a sea-way depend," by W. Froude, F.R.S.

A PROSPECTUS is issued of a third enlarged and improved edition of Von Cotta's "Geology of the Present." Special reference will be made in this edition to the bearing on geological questions of the recent discoveries of Darwin, Mayer, and Helmholtz.

THE first number lies on our table of "The Mining Magazine and Review; a Monthly Record of Mining, Smelting, Quarrying, and Engineering," edited by Mr. Nelson Boyd. The principal articles in this number are—"The Coal Commission," by the editor; "Boiler Explosions," by E. B. Marten; "The Importance of Nitro-glycerine Explosives for Underground Quarrying Purposes," by S. J. Mackie; and "The Progress of Mineralogy," by F. W. Rudler. It contains also reviews, records of scientific progress, and miscellanea.

A LITTLE pamphlet by Mr. J. G. Fitch, entitled "Methods of Teaching Arithmetic," a lecture addressed to the London Association of Schoolmistresses, and published at the request of the Association, deserves a far wider circulation than among schoolmistresses only. We venture to say that if the admirable plan suggested in the lecture were generally adopted by teachers, of explaining in a rational manner the principles of the simple rules of arithmetic, which are generally learned by rote without the least exercise of intelligence on the part of either teacher or pupil, the teaching of arithmetic would soon cease to be the drudgery which it now is in both boys' and girls' schools, and the results, as exemplified by the reports of the Cambridge examiners and elsewhere, would be very different.

THE "Proceedings of the South Wales Institute of Engineers," Vol. vii., No. 4, contains an important paper by Mr. Thomas Joseph, "On Colliery Explosions in the South Wales Coal Field," which is also reprinted in a separate form. We find in it also many other papers and discussions of value to the engineering and coal interests.

MR. E. PARFITT reprints from the "Transactions of the Devonshire Association for the Advancement of Science, Literature, and Art" two interesting papers—"The Fauna of Devon, Part vii: Cirripedia," and "On the Boring of Molluscs, Annelids, and Sponges into Rock, Wood, and Shells."

WE have received from Messrs. Nelson and Sons some specimens of Pictorial Natural History, consisting of packets of cards with coloured pictures of birds and some short account of each appended; they are as a series unusually good and elegant, though of unequal merit. Any of them would make a charming present for an intelligent child.

ON November 10 there was an earthquake in Salvador in Central America, and on the 12th a stronger one. At Simla there was an earthquake on November 25. Two sharp shocks were felt at Macedonia on November 26 at 11 P.M.

WHAT is called the Iquique earthquake took place on Oct. 8, at 1 A.M. Although alarming and lasting two minutes, with a terrible shaking of the earth, first vertical and afterwards oscillatory, it did no damage at Iquique. It was, however, simultaneously felt elsewhere, and has destroyed or damaged the towns of Tarapaca, Usmagama, Guasquina, Pica, Matilla, and the village of Pachica. Some persons were injured, but only two lost their lives.

AT a recent meeting of the Scientific Committee of the Horticultural Society, a letter was read from Mr. Anderson-Henry (printed in the *Gardeners' Chronicle* for Dec. 9), in which he gave some curious results of his observations on climbing plants. Mr. Henry stated that certain climbers evince a partiality for some other species, stretching out their tendrils or branches so as to come in contact with them, while to other species they have as strong an aversion, avoiding them and never touching them, though they may run up the same wall side by side. The subject is a curious one, and deserves further investigation.

"THE Fortunate Isles," translated from the French of Ogier, is an account of the Canaries. A chapter on the celebrated dragon tree contains the two passages here transcribed. Written apparently in sober earnest, they are, perhaps, not the least remarkable contribution to the scientific literature of the year now ended. "It is an undoubted fact that before the great Mediterranean deluge, and to a certain point even after it, strange creatures brought forth in transitional periods, inhabited the marshy grounds or those shallow seas which still remained warm. This epoch, called by modern geologists the Reptile Period, produced creatures belonging at once to the animal, vegetable, and mineral kingdoms, or to two only; monstrous products of creative forces; birds, quadrupeds, fish, plants, reptiles, all at once, either united or distinct; the greater number of these have been restored for us by geologists. . . . . The dragon has existed. The first men saw the last survivors of these prodigious creatures, and the memory of them has been preserved. The struggles of mankind with the mighty creatures which overran the earth must have been terrible. The excessive alarm of men possessing no weapons in the first ages, gave rise to the traditions of formidable beings attacking mankind and destroyed by the demi-gods, strong and brave men."

FROM the *Elizabeth Daily Journal* of New Jersey of Nov. 28 we have a marvellous story of a carrier pigeon, which we commend to the notice of Mr. Tegetmeier. It performed the journey from Sopus Farm, Warren Co., N.J., to Sandusky Ohio, a distance of 400 miles, in exactly an hour, and its condition on its arrival at the latter place is thus described:—"I found the greatest excitement had followed the arrival of the pigeon. Mr. Smythe told me that at precisely two o'clock the bird came like an arrow into his house. His movement was more like a blue streak than a well-defined bird. He seemed but little exhausted, although nearly all the feathers were off his body, except the small patch held on his back by the gutta-percha which fastened the note. A few miles more would have worn every feather from his wings, and then he would have to depend upon the momentum already acquired to carry him on his journey, and to steer by a tailless rump, and perhaps be killed in attempting to alight." No wonder the owner offers to match this pigeon "when he has grown a new suit of feathers" for 1,000 dollars against any carrier pigeon that has not done this distance in an equal time.

PERIODICITY OF SUN-SPOTS*

IN the short account of some recent investigations by Prof. Wolf and M. Fritz on Sun-spot phenomena, which has been published lately in the "Proceedings of the Royal Society" (No. 127, 1871), it was pointed out that some of Wolf's conclusions were not quite borne out by the results which we have given in our last paper on Solar Physics in the Philosophical Transactions for 1870, pp. 389-496. A closer inquiry into the cause of this discrepancy has led us to what appears a definite law, connecting numerically the two branches of the periodic sun-spot curve, viz., the time during which there is a regular diminution of spot-production, and the time during which there is a constant increase.

It will be well, for the sake of clearness, to allude here again, as briefly as possible, to Prof. Wolf's results before stating those at which we have arrived.

Prof. Wolf has previously devoted the greater part of his laborious researches to a precise determination of the mean *length* of the whole sun-spot period, but latterly he has justly recognised the importance of obtaining some knowledge of the average character of the periodic increase and decrease. Hence he has, as far as he has been able to do so by existing series of observations, and his peculiar and ingenious method of rendering observations made at different times and by different observers comparable with each other, endeavoured to investigate more closely the nature of the periodic sun-spot curve, by tabulating and graphically representing the monthly means taken during two and a half years before and after the minimum, and applying this method to five distinct minimum epochs, which he has fixed by the following years:—

1823·2
1833·8
1844·0
1856·2
1867·2

In a table he gives their mean numbers, expressing the solar activity, arranged in various columns; and arrives at the following results:—

(1) It is shown now with greater precision than was previously possible, that the curve of sun-spots ascends with greater rapidity than it descends. The fact is shown in the subjoined diagram, which it may be of interest to compare with the curves given previously by ourselves in the above-mentioned place. The zero-point in this diagram corresponds to the minimum of each period; the abscissæ give the time before and after it, viz., two and a half years, or thirty months; the ordinates express the amount of spot-production in numbers of an arbitrary scale. The two finely dotted curves are intended to show the actual character of a portion of two periods only, viz., those which had their minima in 1823·2 and 1867·2; the strongly dotted curve, however, gives the mean of all periods (five) over which the investigation extends.

(2) Denoting by x the number of years during which the curve ascends, and presuming that the behaviour is approximately the same througnout the whole period of 11·1 years as during the five years investigated, we have the proportion

$$x : 11 \cdot 1 - x :: 1 : 2,$$

whence $$x = 3 \cdot 7,$$

or the average duration of an ascent is 3·7 years, that of a descent 7·4 years.

(3) The character of a single period may essentially differ from the mean, but on the whole it appears that a $\left\{ \begin{matrix} \text{retarded} \\ \text{accelerated} \end{matrix} \right\}$ descent corresponds to a $\left\{ \begin{matrix} \text{retarded} \\ \text{accelerated} \end{matrix} \right\}$ ascent. Thus the minimum of 1844·0 behaved very normally; but that of 1856·2, and still more that of 1823·2, shown in the following diagram, presents a retarded ascent and descent; on the other hand, the minimum of 1833·8, and still more in that of 1867·2, also shown in the diagram, both ascent and descent are accelerated.

Finally Prof. Wolf arranged in the manner shown in the following table the successive minima and maxima, in order to arrive at some generalisation which might enable him to foretell the general character and length of a future period. Taking the absolute differences in time of every two successive maxima, and

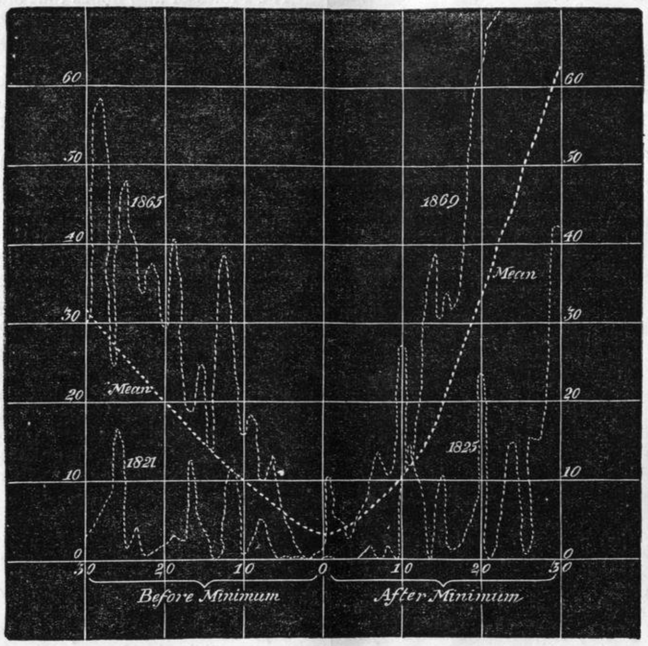
* Abstract of paper read before the Royal Society December 21, 1871. "On some recent Researches in Solar Physics, and a Law regulating the time of duration of the Sun-spot Period." By Warren De La Rue, F.R.S., Balfour Stewart, F.R.S., and Benjamin Loewy, F.R.A.S.

the mean differences of every two alternating minima, he shows that the greatest acceleration of both maximum and minimum happens together. This result strengthens our own conclusions, to be immediately stated, by new evidence, as it is derived from observations antecedent to the time over which our researches extend.

Minima.	Differences of alternating Minima.	Means.	Maxima	Differences of successive Maxima.
1810·5				
			1816·8	
1823·2	23·3	11·65		12·7
			1829·5	
1833·8	20·8	10·4		7·7
			1837·2	
1844·0	22·4	11·2		11·4
			1846·6	
1856·2	23·2	11·6		11·6
			1860·2	
1867·2				

From this Prof. Wolf predicts for the present period a very accelerated maximum—a prediction which seems likely to be fulfilled.

Comparing now M. Wolf's results with our own, it must not be overlooked, in judging of the agreement or discrepancy of these two independently obtained sets, that our facts have been derived from the actual measurement and subsequent calculation of the spotted area from day to day since 1833, recorded by Schwabe, Carrington, and the Kew solar photograms, which measurements are expressed as millionths of the sun's visible hemisphere, while the conclusions of M. Wolf are founded on certain "relative numbers," which give the amount of observed spots on an arbitrary scale, chiefly designed to make observations made at different times and by various observers comparable with each other. This will obviously, in addition to the sources of error to which our own method is liable, introduce an amount of uncertainty arising from errors of estimation, and the possibility of using for a whole series an erroneous factor of reduction. Nevertheless we shall find a very close agreement in various important results, and this seems a sufficient proof of the great value and reliability of M. Wolf's "relative numbers," especially for times previous to the commencement of regular sun observations.



The following is a comparison of the data of periodic epochs, as fixed by ourselves and M. Wolf:—

Minima epochs.	I.	II.	III.	IV.
De La Rue, Stewart, and Loewy	1833·92	1843·75	1856·31	1867·12
Rudolf Wolf	1833·8	1844·0	1856·2	1867·2

Maxima epochs.	I.	II.	III.
De La Rue, Stewart, and Loewy	1836·98	1847·87	1859·69
Rudolf Wolf	1837·2	1846·6	1860·2

It will be seen from this comparison that only one appreciable difference occurs, viz., in the maximum of 1847, which M. Wolf fixes nearly one and a quarter years before our date.

The mean length of a period is found by us to be 11·07 years, which agrees very well with M. Wolf's value, viz., 11·1 years.

We found the following times for the duration of increase of spots during the three periods, and for the corresponding decrease, or for ascent and descent of the graphic curve, beginning with the minimum of 1833:—

	Time of ascent.	Time of descent.
I.	3·06 years.	6·77 years.
II.	4·12 ,,	8·44 ,,
III.	3·37 ,,	7·43 ,,
Mean	3·52 ,,	7·55 ,,

Prof. Wolf gives 3·7 years and 7·4 years for the ascent and descent respectively; and considering that he derived these numbers only from an investigation of a portion of each period, the agreement is indeed surprising, and would by itself suggest that the times of ascent and descent are connected by a definite law.

M. Wolf has expressed in general terms the following law with reference to this relation of increase and decrease of spots:—

"The character of a single period may essentially differ from the mean behaviour, but on the whole it appears that a {retarded / accelerated} descent corresponds to a {retarded / accelerated} ascent."

We, on the other hand, have, by an inspection of our curves (*vide* Phil. Trans. 1870, p. 393), been induced to make the following remark on the same question:—

"We see that the second curve, which was no longer in period as a whole than either of the other two, manifests this excess in each of its branches, that is to say, its left or ascending branch is larger as a whole than the same branch of the two other curves, and the same takes place for the second or descending branch. On the other hand, the maximum of this curve is not so high as that of either of the other two—in fact, the curve has the appearance as if it were pressed down from above and pressed out laterally so as to lose in elevation what it gains in time."

Although both statements appear to lead up to the same conclusion—viz., that ascent and descent are connected by law—still they differ essentially in this respect, that if A, B, C represent the three following consecutive events, descent, ascent, descent, Prof. Wolf's law refers to the connection between A and B, while our remark refers to B and C. We consider two successive minima as the beginning and end of a single period, while M. Wolf, at least in this particular research, places the minimum within the period, and compares the descent from the preceding maximum with the ascent to the next one.

We have considered the connection thus indicated of sufficient importance to apply to it the following test. If, using the previous notation, a definite relation exists between A and B, the *ratio* of the times which the events occupy in every epoch ought to be approximately constant; similarly with respect to B and C; and this ratio should not be influenced by the *absolute* duration of the two successive events. It is clear that the greater uniformity of these ratios will be a test of their interdependence. The following is the result of the comparison :—

a. Prof. Wolf's law: comparison of A and B.

	Periods.	Duration of descent (A).	Periods.	Duration of ascent (B).
I.	1829·5 to 1833·8	4·3 years	1833·8 to 1837·2	3·4 years.
II.	1837·2 to 1844·0	6·8 ,,	1844·0 to 1846·6	2·6 ,,
III.	1846·6 to 1856·2	9·6 ,,	1856·2 to 1860·2	4·0 ,,

	Ratio $\frac{A}{B}$.		Difference from mean.
I.	1·265	Mean 2·093	−0·728
II.	2·615		+0·522
III.	2·400		+0·307

These differences from the mean are so considerable that in the present state of the inquiry a connection between any descent and the immediately *succeeding* ascent appears highly improbable. A very new and apparently important relation seems, however, to result from a similar comparison of any ascent and the immediately succeeding descent, or between B and C.

b. Comparison of B and C.

	Periods.	Duration of ascent (B).	Periods.	Duration of descent (C).
I.	1833·92 to 1836·98	3·06 years	1836·98 to 1843·75	6·77 years
II.	1843·75 to 1847·87	4·12 ,,	1847·87 to 1856·31	8·44 ,,
III.	1856·31 to 1859·69	3·38 ,,	1859·69 to 1867·12	7·43 ,,

	Ratio $\frac{C}{B}$.		Difference from mean.
I.	2·212	Mean 2·151	+0·061
II.	2·044		—0·107
III.	2·198		+0·047

PROF. AGASSIZ'S EXPLORING EXPEDITION *

WE have already announced the departure of the United States Coast Survey exploring steamer, *Hassler*, upon that scientific mission which, under the direction of Prof. Agassiz, will doubtless be productive of very important results. Just before starting on the expedition, Prof. Agassiz addressed a communication to the Superintendent of the Coast Survey, in which he ventured to assume the character of a prophet by stating in advance what it was probable would crown their efforts in the way of discovery.

The Professor makes this communication in the hope of showing within what limits natural history has advanced toward that point of maturity when science may anticipate the discovery of facts. Basing his expectations upon the ascertained principles of science, and taking into consideration the relationships between different forms of animal life, and the succession of geological epochs, and in view of the very interesting results of later deep-sea dredging expeditions in the North Atlantic, he anticipates the discovery, "from the greater depth of the ocean, of representatives resembling those types of animals which were prominent in earlier geological periods, or bear a closer resemblance to younger stages of the higher members of the same types, or to the lower forms which take their place nowadays."

Making no suggestion in regard to mammals, he remarks that if reptiles exist in the deep waters, they must be only such as are related to the extinct types of the Jurassic periods, such as the ichthyosauri, plesiosauri, and pterodactyles; but even of these he thinks there is very little probability that any representatives are still alive.

Among the fishes he expects to discover some marine representatives of the order of ganoids of the principal types known from the secondary zoological period. Among the sharks he thinks he shall find new forms allied to *Cestracion*, or *Hybodon*, or *Odontaspis*, as also new genera of chimæroids; and among ordinary fishes the allies of *Beryx*, *Elops*, &c. It is among the molluscs and radiates that objects of the greatest interest will probably be met with: and chief among these will be nautiloid cephalopods—perhaps even ammonites—and forms only known hitherto in the fossil state. Among *Acephala* he anticipates the discovery of a variety of forms resembling those from the Jurassic and Cretaceous deposits; while *Rudistes* will take the place of oysters, and brachiopods be found very abundant.

Among *Crustacea* it is not at all impossible that forms may be found resembling trilobites; while among echinoderms he confidently expects to meet with spatangoids approaching *Holaster*, and others akin to *Dysaster*, &c.

A careful comparison of the members of the deep-sea faunæ of the northern and southern hemispheres will probably prove of the greatest interest, and, judging from the peculiarities of the land and shore fauna of Australia, it is likely that the adjacent deep-sea animals will be equally divergent, and will represent remarkable forms, and especially of an extremely antique type.

The Professor also hopes that much light will be thrown upon the subject of the geology of the southern hemisphere, and upon the general features of the drift, since all the phenomena related to the glacial period must be found in the southern hemisphere with the same essential characteristics as in the northern, yet with this difference, that everything must be reversed; that is, the trend of the glacial abrasion must be from the south northward; the lee side of the abraded rocks must be on the north side of hills and mountain ranges, and the boulders must have been derived from rocky exposures lying to the south of their present position. This point, however, must be established by observation. The Professor thinks this will be found to be the case, with the exception, perhaps, of the present glaciers of Tierra del Fuego and Patagonia.

In reply to the possible inquiry as to what the question of drift has to do with deep-sea dredging, he remarks that the connection is closer than may at first appear. If drift is not of glacial origin, but the product of marine currents, its formation at once becomes a matter for the Coast Survey to investigate; but he expresses the belief that it will be found that, so far from being accumulated by the sea, the drift of the lowlands of Patagonia has been worn away to its present extent by the continued encroachment of the ocean, in the same manner as the northern shores of South America and of Brazil have been.

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SCIENTIFIC SERIALS

Annalen der Chemie und Pharmacie, clix., August 1871. Fittig and Remsen communicate a second paper "On the Constitution of Piperine and its decomposition products, Piperic Acid and Piperidine;" in the former paper two oxidation products were described, piperonal and piperonylic acid, which stand to each other in the relation of aldehyde and acetic acid. In the present communication several new reactions of these substances are described. — The second note, "A Reaction of free Phenol-hydroxyls," shows that the benzene derivatives, containing hydroxyl associated with this nucleus, give colours with a neutral solution of ferric chloride; the intensity of the colour produced seems to bear some proportion to the number of free hydroxyl atoms, the more intense colours being produced by bodies containing more than one hydroxyl.—A paper "On the relations between the Glycerin and Allyl compounds," by Huebner and Mueller follows. They show that the dichlorhydrin prepared by Berthelot's method is a mixture of two isomeric bodies, one of which boils at 174° and can be obtained in a pure state by the action of hydrochloric acid on epichlorhydrin, the other boils at 182° and is identical with dichlorallyl alcohol. Both of these compounds yield allyl alcohol when acted on by sodium in the presence of ether. Kraut and Popp have found that if sodium amalgam containing 3 per cent. sodium is placed in potassic hydrate solution, hard cubes are formed, which, however, possess no definite composition; by the action of sodic hydrate solution long needles are obtained, having the composition $Na_2 Hg_{12}$.—A lengthy paper by Hoffmeister follows "On Phenyl Ether and Diphenyloxide." The former is prepared by the action of nitrous acid on aniline sulphate, the product from which is mixed with phenol when nitrogen is evolved and phenyl ether formed. It can also be produced by the dry distillation of cupric benzoate. Diphenyl oxide is produced by acting on phenol with phosphoric chloride, and again acting on the product with potassic hydrate. A number of substitution products of the two bodies have been prepared, and are here described. —The next paper is "On the Conversion of Acetone into Lactic Acid," by Linneman and Zotta. This is accomplished by heating dichloracetone with water to 200°, when a considerable proportion of lactic acid is obtained. Ladenburg has prepared stannic triethyl phenyl by the action of sodium on bromobenzol, and stannic triethyl iodide, mixed with ether. It is a colourless liquid, boiling at 254°, which is easily oxidised in the air; it reduces an alcoholic solution of silver nitrate, diphenyl being produced in the reaction. Hydrochloric acid forms with it, benzole and stannic triethyl chloride.—An interesting paper by Friedel and Ladenburg, "On Silico-propionic Acid," follows. By the action of absolute alcohol on silicic chloride, the chloride of triethylsilicic acid is obtained; sodium added to this compound, mixed with zinc ethyl, yields, on heating, ethyl orthosilico-propionate, $Si\,C_2H_5\,(OC_2H_5)_3$. Silico-propionic ether, on treatment with aqueous potassic hydrate, yields silico-propionic acid. It is a white powder resembling silica, from which it is easily distinguished by being combustible. It is soluble in hot potassic hydrate solution, but insoluble in boiling sodic hydrate. This acid is the first representative of a new series of acids, containing the group $Si\,O_2H$ in the place of CO_2H.—Translations of two papers by C. E. Monroe follow, the originals of which have already appeared in the American Journals.— The number concludes with a short note "On the Preparation of Creatinine hydrochloride from urine," by R. Maly. It is purified by combining it with mercuric chloride and decomposing the compound with sulphuretted hydrogen.

SOCIETIES AND ACADEMIES

London

Anthropological Institute, January 1.—Sir John Lubbock, Bart, F.R.S., President, in the chair.—Messrs. J. Thallon and J. Jeremiah, jun., were elected members.—Mr. C. Staniland Wake read a paper entitled "The Adamites." The object of this paper is to show, by reference to evidence extraneous to the Hebrew Scriptures, what peoples are entitled to be classed as Adamites. The name of the primitive race from which the Chaldeans sprung—the *Akkad*—proves that they must be thus classed. Akkad would seem to mean "sons of Ad;" the first syllable of the word being the same as the Gaelic *Mach* or *Ach*. The first Babylonian dynasty of Berosus was Median; and Sir Henry Rawlinson says that the name by which the Medes are first noticed on the Assyrian monuments is *Mad*. This people, the initial letter of whose name may be treated as a prefix, was doubtless the primitive stock from which the *Akk-Ad* were derived. The Medes had also the distinctive title *Mâr;* and many of the Aryan peoples appear to have retained a remembrance of the traditional *Ad*. The first part of the Parsee work known as *The Desatir* is called "the Book of the Great *Abad,*" *i.e.*, Father *Ad*. The Puranas of the Hindus refer to the legendary king, *It* or *Ait*, who is supposed to be the same as the Greek *Aetus*. The primitive Celtic race of Western Europe was called *Gaidal*, *i.e.*, the progeny of Gaid or Aid, who may be identified with *Dis*, the mythical ancestor, according to Cæsar, of the Gauls. Dis (the Greek *Hades*) was also "Lord of the Dead" among the Chaldeans, and may well, therefore, have been the same as the legendary ancestor Ad. Among Hamitic peoples, the original Arab stock trace their first orign to Father Ad, who is probably referred to also in the name of the Egyptian deity, *At-um*. The paper also mentions certain facts showing that the name of the legendary ancestor of the Adamites may be traced in the names of the deities of Turanian and American peoples, and also among the Polynesian Islanders, whose word for "spirit" is *atua*, or *akua*, and whose Great Ancestor is called *Ta-ata*. Dividing all the races of mankind, according to the simple classification of Retzius, into brachycephali and dolichocephali, the conclusion arrived at by the paper is, that Ad was the legendary ancestor of the former, the Adamites, therefore, embracing all the actually brachycephalic peoples, and those whose brachycephalism has been lost by intermixture with the long-headed stock. The Adamites extend through the whole of the northern hemisphere, and are found in various parts of the southern hemispheae, on both the old and the new continents. The names "Adam" and "Eve" were, however, merely expressions of the philosophical notion of the ancients that the male and female principles pervade all nature, and originated all things and personifications of the ancestral idea in relation to the human race.

Chemical Society, Dec. 21, 1871.—Prof. Williamson, F.R.S., vice-president, in the chair.—After the usual business of the society had been transacted, the chairman announced that the celebrated Italian chemist, Prof. Canizzaro, had consented to deliver the Faraday lecture. A paper was then read by Mr. H. Bassett, "On Eulyte and Dyslyte," two beautifully crystalline compounds obtained by the action of nitric acid on citraconic acid, a product of the dry distillation of citric acid. Both these substances contain nitrogen, but owing to the comparatively small quantity obtained, namely, less than two ounces from thirty pounds of citric acid, the author has, as yet, been unable thoroughly to investigate their nature.—Prof. H. E. Armstrong also read a paper "On the Nitration of the Dichloro-Sulphonic Acids," being a continuation of his researches on the isomeric nitrochloro-phenols and their derivatives; after which the meeting adjourned until January 18, 1872.

Paris

Academy of Sciences, Dec. 18, 1871.—M. Chasles read a continuation of his theorems relating to the harmonic axes of geometrical curves, and presented a note by M. Halphen on right lines which fulfil given conditions.—M. H. Resal presented a memoir on the conditions of resistance of a fly-wheel, and M. Combes a note by M. Haton de la Goupillière on the transformation of the potential by reciprocal *radii vectores.*—Telegrams received from M. Janssen, with regard to his solar observations at Ootacamund, were communicated to the Academy. —Several members referred to the prevalence of cold during the first half of the month of December 1871.—M. Delaunay called attention to the remarkable concurrence of a change of barometric pressure with an alteration in the temperature of different parts of Europe between the 6th and 9th of December, the latter date showing the maximum of cold at Paris. The great cold of the 9th of December was also the subject of a note by M. E. Becquerel, who gives a minimum temperature of $-25°{\cdot}5$ C. ($= -13°{\cdot}9$ F.) at Montargis, and of $-27°{\cdot}5$C. ($= -17°{\cdot}5$ F.) near Courtenay in the department of the Loiret. M. C. Sainte-Claire Deville remarked upon the concordance of this statement of M. E. Becquerel's with the minimum of $-26°$ C. ($= -14°{\cdot}8$ F.) recorded at Nemours. He also presented a table of minima obtained at various places in France from 7th to 15th December. —MM. Becquerel presented a memoir on the influence of snow on the temperature of the soil at various depths, according as it is covered with turf or denuded, founded chiefly on observations made from the 5th to the 15th December. The authors found that the temperature under the turfed soil, within two or three centimetres of the surface, was always above 0° C. (= 32° F.), and as constantly below that point in the naked soil.—M. Pasteur presented a note on a memoir by M. Liebig, relating to fermentation, in which he defended his views as to the nature of the phenomena of fermentation from certain criticisms upon them published by Prof. Liebig. Upon this subject M. Fremy also spoke at considerable length in opposition to M. Pasteur, who replied.—M. Bussy communicated a note by M. E. Bourgoin on the complex nature of cathartine, in which the author states that this substance, regarded as the active principle of senna, is in reality composed of three distinct substances, namely, chrysophanic acid, a dextrogyrous glucose, and a new principle to which he gives the name of chrysophanine.—M. Daubrée communicated a note by M. F. Gonnard, on the dolerites of the Chaux de Bergonne and the zeolites which they contain. In this paper the author ascribes very peculiar magnetic properties to the solid dolerite of this locality, and states that the cavities of its lower amygdaloidal parts contain three zeolites (christianite, phacolite, and mesole).—M. Trécul presented a note on the remarkable arrangement of the stomata in various plants, and especially in the petiole of ferns, in which he mentioned the occurrence of stomata upon the piliform appendages of the petiole in *Philodendron Lindenianum*, and noticed their existence in unusual positions in many ferns.—A note by M. P. Bert, on the influence of different colours on vegetation, was communicated by M. Milne-Edwards. His general results are as follows:—green is nearly as fatal to plants as total darkness, red is very injurious, and yellow less so than red, but more so than blue, but any colour taken isolatedly is injurious to plants.

December 26, 1871.—A note by M. Brioschi, on the equation of the fifth degree, was read.—A note was read on the tension of the vapour of mercury at low temperatures, by M. Regnault, in which he claims to have proved long ago that mercury gives off vapours even below the freezing point of water. Upon this paper M. Boussingault made some remarks.—M. P. A. Favre presented a paper "On the Electrical Conductibility of Liquids without Electrolysis," in which he gives the details of certain experiments which seem to show that liquids have a conductibility of their own.—M. S. Meunier read a note on the co-existence of two lithological types in the same fall of meteorites. The author stated that the specimens in the Museum at Paris, from the falls of Sigena in Spain, on November 17, 1773, and of Trenzano in Italy on November 12, 1856, each includes two forms of rock, one, the Indian meteoric stone, described by Maskelyne under the name of *bustite*, the other identical with parnallite. He remarked upon the singularity of this phenomenon, which, he thinks, indicates that the stones which fell at Trenzano and Sigena were derived from the same deposits, and that bustite and parnallite have been stratigraphically related.—M. W. de Fonvielle presented an explanation by means of the theory of fringes of the appearance of luminous halos observed during balloon ascents.—M. Berthelot communicated a further series of thermo-chemical investigations upon the state of bodies in solutions, in which he discussed his researches upon the double decomposition of certain metallic salts.—A note was read on an apparatus for measuring the temperature of alterations and detonations of explosive compounds by MM. L. Leygue and Champion. This apparatus consists of a bar of metal to be heated at one end, upon various parts of which the explosive compounds may be placed.—M. F. Pisani communicated an analysis of the amblygonite (montebrasite) of Montebras, showing that the only difference between this mineral and the amblygonite of Arnsdorf consists in its containing a little less soda.—M. A. Trécul read an important memoir on the origin of the lactic and alcoholic yeasts, upon which M. Pasteur made some remarks.—M. H. Sainte-Claire Deville presented a note by M. F. Cailletet on the origin of the carbon fixed by plants containing chlorophyll, which he regards as wholly derived from the carbonic acid of the atmosphere; and M. Béclard referred to memoirs presented by him in 1858 on the influence of violet light upon vital phenomena.

BOOKS RECEIVED

English.—Researches of the Calculus of Variations: I. Todhunter (Macmillan and Co.).—Volcanoes, the Characters of their Phenomena: J. P. Scrope (Longmans).—A Vision of Creation, a Poem: C. Collingwood (Longmans).—Hymns for Modern Man: H. Noyes (Longmans).

Foreign.—Principes de Biologie appliqués à la Médecine; Dr. Ch. Girard (Baillière et fils).

PAMPHLETS RECEIVED

English.—Journal of the Iron and Steel Institute, Vol. II., No. 4.—Quarterly Journal of Amateur Mechanical Science, No. 4.—Science Directory of the Department of Science and Art.—Meteorological Notes for use in Science Classes; J. H. Collins.—Remarks on certain Oceanic Explorations: W. L. Jordan.—On Ocean Currents, Part 3: Jas. Croll.—The Quarterly German Magazine for November.—Inaugural Address before the Scottish Arboricultural Society: R. Hutchison.—Public School Reforms: M. A. B.—The Fauna of Devon, Part 7: E. Parfitt.—On the Boring of Molluscs, &c.: E. Parfitt.—Transactions of Engineers and Shipbuilders in Scotland.—Eight Days with the Spiritualists: Jas. Gillingham.—Report of the Board of Visitors to the Royal Observatory, Edinburgh.—Figures of Characteristic British Fossils, Part 3: W. H. Baily.—Method of Teaching Arithmetic: J. G. Fitch.—On the Relation of Therapeutics to Modern Physiology: R. Madden.—On the Method of Measuring the Lateral Diffusion of a Current: J. G. H. Gordon.—The Power above Matter: D. de B. Hovell.—Annual Report of the Council of the Institution of Civil Engineers.—Mining Magazine and Review, No. 1.—Ordinary Meetings of the Newcastle-on-Tyne Chemical Society, 1871-72.—Annual Report and Transactions of the Plymouth Institute, Vol. II., Part 2; Vol. III., Parts 1, 2; Vol. IV., Parts 1, 2.—Denudation in relation to Sedementary Stratification: G. Race.—List of Members of the Royal Microscopical Society, 1871.

American and Colonial.—Notes of some Cretaceous Vertebrates: E. D. Cope.—Preliminary Catalogue of the Bright Lines in the Spectrum of the Chromosphere: C. A. Young.—Monthly Notices of Papers and Proceedings of the Royal Society of Tasmania. 1870.—A Catalogue of the Birds of New Zealand: F. W. Hutton.—Remarks on the Adaptive Colouration of Mollusca: E. S. Morse.—Transactions of the Entomological Society of New South Wales, Vol. II., Part 3.

Foreign.—Ofversigt af kongl. Vetenskaps Akad. Förhandlingar, Nos. 3, 4, 8, 9, 10.—Zeitschrift für Ethnologie, No. 5.—Zeitschrift für Meteorologie No. 23.—Giornale di Sicilia, No. 268.—Nova plantarum species: A. Kerner.—Können aus Bastarten Arten werden: A. Kerner.—Ueber Iris Cengialti Ambrosi; A. Kerner.—Ueber den Einfluss der Winde auf die Verbreitung der Samen: A. Kerner.—Association Scientifique di France, No. 218.—Gazzetta Chimica Italiana, No. 9.—Sul bromuro di etilidene: E. Paterno.—Sintesi due nuovi clorobromuri di carbonio: E. Paterno.—Azione del bromocloruro di fosforo al clorales: E. Paterno.

DIARY

THURSDAY, January 4.

London Institution, at 4.—The Philosophy of Magic. 3. The Magic of the Mediums: J. C. Brough, F.C.S.

FRIDAY, January 5.

Geologists' Association, at 8.—On the Overlapping of Several Geological Formations on the North Wales Border: D. C. Davies.

SATURDAY, January 6.

Royal Institution, at 2.—On Ice, Water, Vapour, and Air: Dr. Tyndall. (Juvenile Course.)

SUNDAY, January 7.

Sunday Lecture Society, at 4.—On Atoms; being an explanation of what is definitely known about them: Prof. W. K. Clifford, M.A.

MONDAY, January 8.

Royal Geographical Society, at 8.30.—On Bunder Murayah, Somal Land: Capt. S. B. Miles.—On a Journey to the Murut Country in Northern Borneo: Lieut. De Crespigny.—On a Description of Fernando Noronha: Dr. A. Rattray.

Victoria Institute, at 8.—Chance Impossible: Dr. J. H. Wheatley.

TUESDAY, January 9.

Photographic Society, at 8.—On Photography in the Printing Press: J. R. Sawyer.

WEDNESDAY, January 10.

Geological Society, at 8.—On the Foraminifera of the family Rotalinæ (Carpenter) found in the Cretaceous formations, with Notes on their Tertiary and Recent Representatives: Prof. T. Rupert Jones, F.G.S., and W. K. Parker, F.R.S.—Notes on the Geology of the Plain of Morocco and the Great Atlas: Geo. Maw, F.G.S.—Further Notes on the Geology of the Neighbourhood of Malaga: M. D. M. d'Orneta.

THURSDAY, January 11.

Royal Society, at 8.30.

Society of Antiquaries, at 8.30.

Mathematical Society, at 8.—On the Surfaces the loci of the vertices of cones which satisfy six conditions: Prof. Cayley.—On the Constants that occur in certain summations by Bernouilli's series: J. W. L. Glaisher.—On the Construction of large tables of divisors and of the factors of the first differences of prime powers: W. B. Davis.—On the Parallel Surfaces of Conicoids and Conics: S. Roberts.

London Institution, at 4.—The Philosophy of Magic. 4. The Magic of the Laboratory: J. C. Brough, F.C.S.

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