THURSDAY, FEBRUARY 18, 1892.

A COLLECTION OF MEMOIRS ON PHYSICS.

Collection de Mémoires relatifs à la Physique. Publiés par La Société Française de Physique, tome 1–5. (Paris, Gauthier-Villars.)

LA Société Française de Physique in the volumes before us have initiated a movement which cannot fail to be of the greatest service to students of physics. They are publishing collections of memoirs on one subject written by several authors, instead of following the more usual plan of collecting the papers of one author on a variety of subjects. There can, we think, be no question as to which plan is of most service to the student. The collected papers of one author must from the nature of the case be chiefly used as a work of reference, while the study of a collection of the most important memoirs ought to form an essential part of the reading of every advanced student of the subject. Now that the memoirs in which the foundations of the sciences of electrostatic or electromagnetism, and of investigations with the pendulum, are by the enterprise of the French Society so readily accessible, it is to be hoped that there will be a much greater development of the systematic study of the original memoirs than we are afraid prevails at present.

Science, as Maxwell said, is most easily assimilated when it is in the "nascent" condition, and, moreover, it is to be expected that when a long paper by a master of his subject has been condensed in a text-book to a twentieth of its original length, something of importance must be lost. In a text-book there is as a rule but little room for anything beyond the description of the method which ultimately proved successful, all reference to the difficulties met with, and the way they were overcome, has to be omitted, though these are precisely the points most calculated to induce the student to endeavour to make investigations for himself.

The volumes before us are also of especial interest to the student of science, as they contain papers such as those by Coulomb on electrostatics and magnetism, and by Ampère on electromagnetism, which raised the subject with which they dealt from chaos to order.

The first volume of these collected memoirs is edited by M. Potier, and differs somewhat in character from those which follow, inasmuch as it is entirely devoted to papers by Coulomb. It contains the classical memoirs in which he established by the aid of the torsion-balance the fundamental laws of electrostatics, and of the action between permanent magnets. It also contains a memoir on the loss of electricity by an insulated charged body, in which he comes to the conclusion that there is a leakage of electricity through the air; subsequent experiments have, however, shown that this is erroneous, and that there is no such loss when the air is free from dust and the charged body not under the action of ultra-violet light.

It is remarkable, considering the importance of Coulomb's contributions to electricity and magnetism, that his most important memoirs on these subjects were all published within five years, 1785–1789, when he was between 49 and 53 years of age.

The editor gives as an appendix a very clear account of Poisson's and Sir W. Thomson's investigations of the problem of two electrified spheres. Poisson's papers on this subject are not very accessible, and we wish that they had been included in this volume, as his investigation of this problem is surely one of the most elegant pieces of analysis ever written. It is possible, however, that it was deemed to be too exclusively mathematical to be included in this collection of physical memoirs.

The second and third volumes, edited by M. Joubert, are devoted to electromagnetism. They include Œrsted's paper, published in France in 1820, announcing the discovery of the deflection of a magnet by a current, and the marvellous series of papers published almost weekly by Ampère, in which, in a few months after the publication of Œrsted's discovery, the "Newton of Electricity" established the action of a magnet on a current, and of one current on another, and of the identity of magnets and electric currents. In his earlier papers, those previous to 1822, Ampère seems to have been hampered by the erroneous idea that the force between two small elements of current at right angles to the line joining them was indefinitely small compared with that between the same element at the same distance when forming parts of the same straight line. Instead of being infinitely smaller, it is in reality, as he showed later, just twice as great. He soon, however, corrected this mistake, and in 1822 gave to the world a complete theory of the mechanical forces between currents in a memoir which is reprinted in these volumes, and which was described by Maxwell as "perfect in form and unassailable in accuracy."

In addition to the papers we have mentioned, there are many others dealing with points of the greatest interest: thus we find Œrsted, in his paper on the action of a current on a magnet, suggesting that light may be an electrical phenomenon, and Ampère writing in favour of this suggestion. In the second volume we have a paper by Faraday on electromagnetic solution; two papers by Davy, in one of which he describes a curious heaping up of a layer of mercury over the places where a strong current enters and leaves the mercury. This pretty effect is due to the mechanical force between the current in the leads and the current through the layer of mercury. This paper also contains Barlow's description of his electromagnetic wheel, and two papers, not hitherto published, by Fresnel, on Ampère's theory of magnets. Fresnel appears to have been the author of the suggestion that the currents by which Ampère explained the magnetic properties flowed round the molecules of the iron. In the third volume Weber's great paper on electrodynamic measurements appropriately completes the series of papers on electromagnetism, as the measurements made by the methods it develops afford a complete verification of the theory given in the preceding pages by Ampère.

The fourth and fifth volumes, edited by M. Wolf, contain memoirs on pendulum experiments. They commence with a well-written historical account of such experiments. Next we have a bibliography extending over 216 pages, and then follows a series of admirably selected papers of which the names speak for themselves. On pendulum experiments pure and simple, we have memoirs by De la Condamine, Borda and Cassini, Prony, Kater, Bessel, Sabine, and Baily, and in addition to these we have Stokes's paper on the effects of fluid friction on the motion of pendulums.

We must, in conclusion, express our gratitude to La Société Française de Physique for the publication of these volumes, and to MM. Potier, Joubert, and Wolf, for the masterly way in which they have edited them. We hope that the volumes before us are but the first terms in an infinite series. J. J. THOMSON.

THE FORMATION OF BEACHES.

Sul Regime delle Spiaggie, e sulla Regolazione dei Porti. ("On the Formation of Beaches, and the Rules for designing Harbour Works.") By Signor P. Cornaglia, late Inspector of the Royal Corps of Italian Civil Engineers. (Turin: R^a Tipografia Paravia, 1891.)

THE laws that govern the movements of sand along the sea-shores, the formation and corrosion of beaches, the shifting of bars and sand-banks at the mouth of rivers, and the silting up of harbours, have not yet been clearly explained, as the problem is a complex one. These effects result from a combination of various causes, such as the action of sea-waves, of tidal currents, of the natural discharge of rivers, and of the dimensions and specific gravity of the materials that form the beaches and give them a special angle of rest.

To examine this problem, Signor Cornaglia takes it up in its most simple form. He tries to ascertain the laws that regulate the propagation and effects of sea-waves acting in a tideless sea, or nearly so, as is the Mediterranean. These researches are made both theoretically and practically, by the help of mathematical analysis and by verifying the results of theory by direct observations of waves in accumulating the materials that form the beaches. These form the subject of two memoirs, in which are discussed the vertical propagation of waves in liquids, and the origin and action of bottom-waves (*flutti di fondo*) in liquids in a state of undulation.

The results of these researches are thus expressed:—(*a*) The undulatory movement of liquids generates near the bottom an oscillatory movement called *bottom-wave*, or under wave (*flutto di fondo*), which is alternately directed to or from the shore; (*b*) vertically under the crest of a superficial wave, the bottom-wave is directed toward the shore; under the hollow of a wave it is directed toward the sea; (*c*) the force of these bottom-waves increases with the force of the superficial waves, with the greater distance or "*fetch*" from which the waves arrive, and with the greater depth of the sea; (*d*) the energy of these bottom-waves may be great at great depths; (*e*) on a rising submarine slope the force of the bottom-waves directed toward the shore is greater than that of the reverse bottom-waves; (*f*) the bodies resting upon the bottom of the sea and exposed to these bottom-waves are struck in alternate directions; (*g*) the component of the weight of these bodies, parallel to the bottom slope, may counterbalance the effects of the direct bottom-waves, or, added to the effect of the return bottom-waves, these latter may prevail upon the direct ones; (*h*) the line along which the action of each of the two opposite bottom-waves, combined with the component of the weight of the bodies, counterbalances that of the other, is called the *neutral line*; (*i*) all conditions being equal, this *neutral line* is situated at a greater depth the stronger the waves are, and the smaller the slope of the bottom and the size and specific weight of the bodies resting upon the bottom; (*j*) in the Mediterranean this *neutral line* is situated at depths varying from 8 to 10 metres, or 27 to 33 feet; (*k*) on the land side of the neutral line the materials forming the bottom of the sea are pushed by the bottom-waves toward the shore, on the sea side they are drawn toward the greater depths; (*l*) parallel to the shore the materials travel always in a contrary direction to that from which the waves come, and they travel alternately in one way or the other according to the direction of the waves. However, the ultimate direction in which these materials move is that corresponding to the prevailing waves, which is also, more or less, that of the prevailing winds.

In a third memoir the author discusses the formation or corrosion of beaches in a tideless sea, and explains a long series of observations and experiments made principally by himself along the shores of the Riviera, with a view of ascertaining the position of the neutral line.

In a fourth memoir are examined the conditions of the estuary of a river opening into a tideless sea, and the effects due solely to the waves having been previously ascertained, the effects due to the outgoing water of the river are described. Then the author passes on to examine the case of a river opening into a sea subject to the influence of tide and tidal currents; and by separating the effects of the waves and of the outgoing water of the river proper, he tries to explain the effects due exclusively to the action of tides. From this he gives some hints about the probable position of the neutral line resulting from all these different causes, and by comparing the conditions of the Mersey with those of the Thames and other tidal rivers, the author concludes that in the former case the outlet of the river is on the land side of the neutral line, and thus the materials brought down by the river or washed away from the coast are pushed into the estuary, while in the case of the Thames its outlet is on the sea side of the corresponding neutral line, and the materials are drawn into deeper water. In the same way he explains why some harbours in the Mediterranean having their entrance in depths of water superior to 10 metres, and thus outside the neutral line, have kept good for centuries, while other harbours with their entrance inside the neutral line have gradually silted up.

Having thus explained his views on the formation of beaches, Signor Cornaglia makes use of them in laying down in several memoirs the principal rules for designing such maritime works as may be necessary to maintain or improve the navigable channel in the estuary of a river, or to protect the entrance of a harbour, or to prevent the sea from encroaching upon a beach. There are descriptions of works carried out in several Italian harbours during this century, and the results, good or bad, according to the way these works were designed.

A drawback to this book is that it is written in the form of separate memoirs, so that there are many repetitions and some unnecessary details, which rather diminish than add to the clearness of the work. Taken as a whole, however, the book will be read with interest and advantage both by the maritime engineer and by the geologist.

EGYPTIAN HISTORY.

Egypt under the Pharaohs. By H. Brugsch-Bey. A New Edition, condensed and thoroughly revised by M. Brodrick. (London: John Murray, 1891.)

THE science of Egyptology is scarcely seventy-five years old, and published formulated statements of the history of Egypt derived from the comparatively newly-acquired decipherment of hieroglyphics are not yet thirty years old; books on Egyptian history are now so common that the general reader has yet difficulty in deciding which of those available is best for him to read or study. Dr. Birch's "Egypt from the Earliest Times to B.C. 300" printed, we believe, in 1875, is without doubt, the best of all the small histories of Egypt which have ever been written: the facts are to be depended upon, few alterations are necessary, difficulties are not slurred over, and the whole subject is there treated with the breadth of view and learning only to be found in such a scholar. More concise, but written in the same admirable style, is the "Aperçu" of Mariette, which aimed at presenting in a small compass the principal facts of Egyptian history to the visitors to the Exhibition in Paris in 1867; for the general history of Egypt and the relations of her people with foreign nations in the various epochs of her national life, the reader would naturally consult Maspero's "Histoire Ancienne des Peuples de L'Orient," and Lenormant's "Histoire Ancienne de l'Orient." The best examples of scientific histories of Egypt are those of Dümichen and Wiedemann. In the "Aegyptische Geschichte" of the latter scholar, published at Gotha in 1884, the author not only sets before the general reader or student the statements of certain facts, but gives in foot-notes the authorities for the statements, so that his work may easily be controlled.

This was an important step in advance, and has done more to convince people generally that the decipherment of the Egyptian inscriptions has been productive of important results than can be directly estimated. Long, however, before any of the above-mentioned works was written, or probably planned, Dr. Brugsch, the greatest of all living Egyptologists, so far back as 1859, published the first part of a history of Egypt which was to begin with the earliest monument, and to end with our own days; this part was entitled "L'Egypte sous les Rois Indigènes," and was published with 19 plates in quarto, but the two other parts, which were announced to contain the history of Egypt under the Greeks and Romans, and under the Arabs, seem never to have appeared. In 1876 Dr. Brugsch published his "Geschichte Aegyptens unter den Pharaonen," in two volumes, with maps and tables; and in this work he dealt with the history of Egypt as comprised in the thirty dynasties, beginning with the first historical king, Menes, and ending with Nectanebus. The narrative was written in fine German, and nearly every important event in the history of Egypt, as then known, he described by translating the hieroglyphic inscription which referred to it. These translations read easily, and, on the whole, represented very well the sense of the Egyptian inscriptions in the words of a modern language. In 1877 Mr. John Murray published an English translation of this work entitled "A History of Egypt under the Pharaohs," with Maps, by H. Brugsch; the translation was the joint work of Danby Seymour and Philip Smith, and although it was on the whole good, it was only too evident that on certain points of Egyptology the translators had no special knowledge, while the beauty of Dr. Brugsch's style was, of course, lost in the process of translation. In 1881, Mr. Murray issued a second edition of the work, which, in addition to a new preface by Dr. Brugsch, contained a number of notes by Philip Smith, which were helpful to the reader, and several alterations and corrections by the author himself; the thirty-two pages of additions and notes were also most useful. During the past ten years great strides have been made in the science of Egyptology, notwithstanding the loss, by death, of Birch and Lepsius, the fathers of Egyptology; and general investigations into the history and language of Egypt have resulted in the discovery of a host of new facts, many of which have an important bearing upon the received ideas on these subjects. That a new edition of Brugsch's "Aegypten," or of its English translation is called for, is not to be wondered at, and no one has shown himself more sensible of this need than Mr. Murray himself. We venture to submit, however, that any attempt to "condense" or "thoroughly revise" the work by anyone except Dr. Brugsch or some competent hand was a mistake. The "condensed" edition of "Egypt under the Pharaohs" now before us contains 450 pages of text 8vo., and three maps; all Philip Smith's notes, which, as we before said, were useful to the general reader for whom this book is intended, are deleted, Brugsch's article on the Exodus, the additions and notes, the transliteration and translation of the stele of Usertsen III. have been omitted, and the originally full index has been cut down. Each page contains five lines more than the second English edition, hence we necessarily expect the "condensed" edition by Miss Brodrick to be a smaller book; but seeing also that by cutting out clauses and important adjectives, &c., &c., she has succeeded in putting 49 pages of the second English edition into 18 of the condensed edition, we do not understand the statement made in the second paragraph of her preface that nothing has been omitted except the Essay on the Exodus and the transliteration, &c., of the Tablet of Usertsen.

We have no doubt that Macaulay's "History of England" could be condensed to one-fifth of its present size by cutting out all that is explanatory of facts; but what should we gain by this mutilation? Miss Brodrick's condensation has been so vigorous that Brugsch's explanation of the word "Hyksos," which occupies fifty-seven lines in the second English edition, occupies only eight in hers; and where Brugsch gives two references to one event described in the Bible, Miss Brodrick omits the one which refers to the fuller narrative (p. 375, note 4). "The much-vexed question of the nationality of the Bubastites has, so far as possible, been accommodated to Brugsch-Bey's present views" by omitting whole paragraphs which occur in the second English edition.

In the condensed edition the cartouches of Egyptian kings which stood at the head of the chapters in the second English edition have been placed at the beginning of the book, and Miss Brodrick has added five pages of matter on the Dêr el-Bahari mummies.

We have long hoped that Dr. Brugsch would issue a new edition of his "Aegypten unter den Pharaonen," revising his facts in some places, and correcting his statements in others, and also adding the new facts relating to the periods between the VII.-XIth and XIII.-XVIIth Dynasties, which have recently come to light; failing this, which is much to be desired, we hoped that one of his pupils would do the work under his guidance. That, however, the English translation made by Seymour and Smith, mutilated and robbed of its notes, and of the additions of the author, should be issued as a popular text-book of Egyptian history under Brugsch's name is a fact which we deplore.

OUR BOOK SHELF.

The Story of the Hills: a Popular Account of Mountains, and how they were made. By the Rev. H. N. Hutchinson, B.A., F.G.S. (London: Seeley and Co., 1892.)

THIS is a pleasant, chatty book, all the more welcome because wholly unpretentious; not too deep for "human nature's daily food" when roaming among the hills of which it treats. It will be read with pleasure and profit by the tourist, who likes to know just enough about the sundry points of interest connected with the scene of his wanderings to make the enjoyment of his outing intelligent, but who is not haunted by a feverish anxiety to be for ever, in season and out of season, improving his mind. Many who would shrink from a formal scientific treatise with horror or disgust will find themselves able to enjoy this book, and through its channel scraps of useful knowledge may insinuate themselves into their minds which would never have found their way there by any other road.

Part I. is multifarious, and touches on a vast variety of matters more or less connected with mountains, and principally of human interest—mountain races, mountain legends, the uses of mountains to mankind, mountain storms, avalanches, and the plants and animals of mountains. Scientific explanations of facts and phenomena are interspersed: the severe critic may detect a little vagueness and looseness here and there in these, but no very serious lapse. Well-chosen quotations from Ruskin and other authors give brilliancy to the narrative. There are landscape views reproduced from photographs, which have all the excellences and the artistic failings of this class of illustration.

Part II. is mainly taken up with a geological history of mountains. Here all the main geological truths that bear on the subject are expounded clearly, and with great fullness of detail. In fact, an epitome is given of a large number of the leading doctrines of geology, which will suffice for the needs of many a general reader. A separate chapter is devoted to volcanic mountains and volcanic activity. We may note that the three stages in the life of a volcano mentioned on p. 266 are not such as are usually defined by geologists. A. H. G.

The Optics of Photography and Photographic Lenses. By J. Traill Taylor. (London: Whittaker and Co., 1892.)

ALTHOUGH photography is so widely practised at the present day, it is surprising how little is known by amateurs about the principles that underlie the construction of photographic lenses.

The present work will serve as an excellent guide to those who wish to gain this information, and should be found to be of great practical use. The author has dealt with the subject in a very popular manner, and although the mathematics is reduced to a minimum, he has made his meaning very clear throughout.

In the first few chapters the nature and properties of light are discussed, together with explanations of photographic definition, single and achromatic lenses, cause of the inverted image, spherical aberration, nature and function of diaphragms, nature and cure of distortion, optical centres of single and combination lenses, &c. Chapters xi. to xv. treat solely of lenses, including accounts of the non-distorting, wide-angle, portrait, landscape, copying, and universal lenses. As there are thirty-nine chapters in all, we may mention that of those remaining there are many on subjects which may be of special interest to individual readers. Thus we have a chapter dealing with photo-telescopic lenses, a short one on the grinding of lenses, and another on enlarging and projecting in relation to lantern optics.

It will be seen that the author has dealt with a wide range of subjects in which the lens makes its appearance, and the reader will find that the explanations are lucid, while the illustrations bring out the points which they are intended to show with equal clearness. W.

The Evolution of Life; or, Causes of Change in Animal Forms. A Study in Biology. By Hubbard Winslow Mitchell, M.D. (New York and London: G. P. Putnam's Sons, 1891.)

DR. MITCHELL says in the preface to this book that he has accomplished in it "all that can be reasonably expected from a medical man deeply immersed in the duties of his profession." What most people expect from medical men in this position is that they will not write books on vast and complicated subjects, for the proper treatment of which an author must have not only exceptional ability but ample opportunities for philosophic study. So far as we have examined the work, it has neither freshness of thought nor charm of style. Dr. Mitchell mentions that he has travelled in many different parts of the world. If he was determined to write a book, he would have been better employed in recording his reminiscences as a traveller than in tediously discussing questions which have occupied so many of the foremost intellects of the present age.

LETTERS TO THE EDITOR.

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

A Magnetic Disturbance.

OUR attention having been directed for some days past towards a spot of unusual size upon the sun's disk, we were not by any means surprised to observe, as doubtless many of your readers elsewhere also did, an aurora of great beauty on Saturday night last; nor was our anticipation of seeing a magnetic disturbance portrayed upon the magnetograph records disappointed in the morning, for when the sheets were changed and the photographs developed, we saw that perturbations more violent than any which had been recorded at Kew for the past ten years had been in progress since about 5.45 a.m. of February 13.

The magnets were very quiet on Friday, but early on Saturday morning they became disturbed. The easterly declination slightly increased until about 5.40 p.m., whilst both horizontal and vertical forces similarly increased in intensity, more especially between 4 and 6 p.m. They further diminished in force after 10 p.m., and their changes became very rapid from 12 midnight to 2 a.m., whilst at the same time the declination proceeded to its extreme westerly position. Subsequently, the fluctuations in magnetism became much reduced in extent, and the whole disturbance gradually diminished and died out about 4 p.m. of Sunday.

The Kew magnetometers were not able to record the complete extent of the vibrations to which free needles were subjected, nor could the entire change of force be secured in the field of the instrument. The limits, however, clearly recorded were 2° of declination from ·1760 to ·1830 of horizontal force, and from ·4350 to ·4420 units of vertical force expressed in C.G.S. measure in absolute force.

G. M. WHIPPLE,
Superintendent.

Kew Observatory, Richmond, Surrey, February 16.

The New Star in Auriga.

PROF. COPELAND has suggested to me that as I am the writer of the anonymous postcard mentioned by you a fortnight ago (p. 325), I should tell your readers what I know about the Nova.

It was visible as a star of the fifth magnitude certainly for two or three days, very probably even for a week, before Prof. Copeland received my postcard. I am almost certain that at two o'clock on the morning of Sunday, the 24th ult., I saw a fifth magnitude star making a very large obtuse angle with β Tauri and χ Aurigæ, and I am positive that I saw it at least twice subsequently during that week. Unfortunately, I mistook it on each occasion for 26 Aurigæ, merely remarking to myself that 26 was a much brighter star than I used to think it. It was only on the morning of Sunday, the 31st ult., that I satisfied myself that it was a strange body. On each occasion of my seeing it, it was slightly brighter than χ. How long before the 24th ult. it was visible to the naked eye I cannot tell, as it was many months since I had looked minutely at that region of the heavens.

You might also allow me to state for the benefit of your readers that my case is one that can afford encouragement to even the humblest of amateurs. My knowledge of the technicalities of astronomy is, unfortunately, of the meagrest description; and all the means at my disposal on the morning of the 31st ult., when I made sure that a strange body was present in the sky, were Klein's "Star Atlas," and a small pocket telescope which magnifies ten times.

THOMAS D. ANDERSON.

21 East Claremont Street, Edinburgh, February 13.

Nacreous Clouds.

IN the morning of the 30th ult. there was a magnificent display of the nacreous (or iridescent, as they were first called) clouds, which formed such a striking feature of the sunset and sunrise sky for some days in succession in December 1884 and 1885 (vol. xxxi. pp. 148, 192, 316, 360, &c.). They were not exactly the same in appearance, but I should say they were of the same nature. I had not seen them in the interval of six years, and have only noticed them lately on the one day mentioned. They were confined to the southern part of the sky. As the sun rose higher their colours were less visible, and the clouds disappeared about noon; though in the afternoon some reappeared, but never became very striking. At 5h. 44m. G.M.T. there was only one group, which was too far from the sun to show any nacreous colours; its centre was about at hour-angle 1h. 2m. west, and declination 23½° south. Although conspicuous they were no longer very bright, and I should say the sun was evidently not shining on them, for they were the same bluish-green colour as the western sky, and I apprehend were illuminated by the sky.

T. W. BACKHOUSE.

Sunderland, February 9.

The Cause of an Ice Age.

IN his very kindly review Prof. Darwin thinks I might have stated my argument with more completeness if I had preserved its generality by the use of a symbol instead of taking a special case.

No doubt in many ways the treatment he suggests would have been better. It would, for instance, have enabled me to prove the case *a fortiori*. Perhaps, however, the reasons given in the chapter explaining "why the book has been written" may show that for the object I had in view the method actually used was appropriate.

I am also much obliged to the same friend for pointing out that the astronomical theorem proved in the appendix had been given by Wiener, "Über die Stärke der Bestrahlung" (*Zeitschrift der Oesterreichischen Gesellschaft für Meteorologie*, vol. xiv., 1879, p. 129).

ROBERT S. BALL.

Observatory, Co. Dublin, February 9.

Ice Crystals.

WITH reference to the letter on the subject of ice crystals which appeared in NATURE of the 4th inst. (p. 319), it is perhaps worth mentioning that a paper on the subject, entitled "Eine Eiskrystallgrotte," by C. A. Hering, appeared in Groth's *Zeitschrift für Krystallographie und Mineralogie*, Band xiv. (1888), pp. 250-253, and Plate vi.

The crystals occurred in an old mine on the Waschgang near Döllach in Carinthia. Large fans, as much as 300 mm. long × 200 mm. broad, of ice-crystals grow out horizontally from the vertical walls. The stalk, consisting of a series of hexagonal prisms, hollow, like thermometer-tubes, was in the middle 25 mm. thick and thickened towards the point of attachment to the rock. The fan surface was a large hexagonal plate with strong prismatic ribs running from the centre to the angles. The interspaces between the ribs were filled by prisms arranged with the greatest regularity. Upon the ribs of the fan either single crystals or funnel-shaped structures with step-like sides consisting of prisms were borne. The individual crystals were almost all thick tabular forms, with prism, basal pinacoid, and rhombohedral faces.

BERNARD HOBSON.

Owens College, Manchester, February 8.

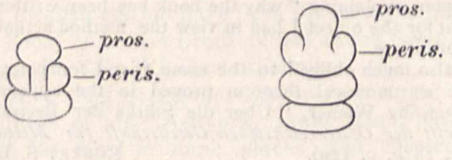
A Rare British Earthworm.

IN the summer of 1890, during my researches into the Vermes of Cumberland, I discovered a species of earthworm which proved to be new to Britain (*Lumbricus Eiseni*, Levinsen). As I have recently had the good fortune to receive specimens of the same worm from another part of the country, it seems desirable to place the same on record. A correspondent writes from Gloucestershire as follows:—

"Last Saturday (January 30, 1892), I walked up to one of my favourite woods here on the Cotswolds, about 700 feet above the sea—a damp old beech wood, the Frith Wood of Withering's "Arrangement," seventh edition, 1830—and seeing a stump of some 10 inches diameter with a growth of the black 'Candle Snuff Fungus' on it, I examined the rotten wood, which gave way to the pokes of my stick. Among this rotten wood I saw some earthworms, two or three of which I inclose, hoping they may prove an addition to our worm fauna."

I have placed on record all the known earthworms of Gloucestershire in *The Field Club* for 1891, to which this may now be added. The worms were small, but in good form for identification, and prove to be specimens of Eisen's worm. I have, unfortunately, been unable hitherto to consult Levinsen's original description; nor have I been able to obtain Rosa's memoir published in the *Boll. Mus. Zool.*, Torino, 1889 (vol. iv., No. 71). I am therefore obliged to content myself with a description of the specimens in my possession.

Lumbricus Eiseni, Lev., as found in Britain, is a small species of earthworm, measuring about 1½ inches in length when adult. It has the usual colour of the allied species—the purple and red worms—being of a ruddy hue, with iridescence. The clitellum or girdle, which occupies segments (24) 25 to 31, is a reddish-brown, being lighter in colour than the anterior portion of the worm's body on the dorsal surface. Ventrally the worm is, as usual, of a lighter shade. No *tubercula pubertatis* have been seen under the girdle, but the first dorsal pore in every specimen examined is clearly detected behind the 5th segment. This may be indicated by the fractional sign $\frac{5}{6}$; and as the most recent researches tend to demonstrate the constancy of this character for each species of earthworm, it is important to note the same. The lip or prostomium has the complete mortise and tenon arrangement which distinguishes Lumbricus from Allolobophora.



The male pore is situated normally on segment 15, but as the papillæ which carry the pores are large, they extend over the adjoining segments on either side. Earthworms vary greatly in this respect. Rosa says that spermathecæ are absent in this species, a peculiarity which has been noted in worms belonging to several other genera. I have not sufficient material to enable me to confirm or dispute this statement at present. I have counted the segments of three specimens, and found them to be in each instance 106. As the year advances I hope to be able to obtain mature adults for dissection, when it will be possible to give a detailed account of the internal anatomy. Meanwhile the external characters are amply sufficient for distinguishing the worm if the girdle is properly developed, as its nearest British ally (*Lumbricus purpureus*, Eisen) has the clitellum on segments 28 to 33.

HILDERIC FRIEND.

Idle, Bradford.

The Implications of Science.

WILL you allow me to say something in answer to Mr. Dixon's letter on this subject in NATURE of January 21 (p. 272)?

(1) I admit that there is a verbal or symbolic "convention" if two (or more) persons agree to understand any given words or symbols in a way arbitrarily chosen by themselves. But the scope of such convention is exceedingly limited: if people wish to be understood, or even to understand themselves, they must use the same words as others use, and use them in the same sense (except in an infinitesimal proportion of cases). If it is said that the common application and use of current words is a mere convention, the word *convention* is taken in an extremely strained and metaphorical sense, since nothing like an explicit agreement has ever been made. The "convention" as to the use of language is as fictitious as the *social contract* of Locke and Rousseau. But in the one case, as in the other, there is a solid basis of facts, to suit which the hypothesis has been produced. Language has been moulded by thought and feeling, which, in their turn, have been impressed by facts; and it is facts and relations of facts that language seeks to express. As Mill says (in the first chapter of his "Logic") names are a clue to things, and bring before us "all the distinctions which have been recognized not by a single inquirer but by all inquirers taken together." No one, I imagine, would say that a *particular* case of the impossibility of affirming and denying a given statement, depends "solely on the law of contradiction"; but in the case of any particular assertion, the impossibility, *in that case*, is seen, and to a mind that has reached the generalizing stage, the universal is discernible in the particular. As regards the question of "real propositions," I will not occupy space with quotations, but will only refer to Mr. Dixon's letter of December 10, in which the passages occur which led me to think that he regarded assertions (or denials) of the existence of particular objects as the only "real" propositions.

(2) As regards induction, I agree with Mr. Dixon that the starting point in induction is hypothesis or discovery. But with reference to the rest of the procedure, and its relation to so-called "formal" logic, I differ from him. For I think that an inductive generalization may be set out syllogistically; *e.g.*,

> What has once produced X will always produce X;
> A has once produced X;
> ∴ A will always produce X (= all A is X).

If space allowed, I should like to consider the justification for the major premiss, and also to say something about the grounds on which the minor (which indicates the hypothesis or discovery) asserts causation [or concomitance] in a given instance.

(3) Mr. Dixon says: "We do not, in mathematics, conclude a universal proposition from a single concrete instance." But it appears to me that, as far as my own experience goes, in every concrete mathematical proposition which I understand this is exactly what happens; and I do not see how, on Mr. Dixon's view, mathematical formulæ could ever have been constructed. "A mathematical formula," Mr. Dixon remarks, "does not imply the existence of any instance whatever of its application, any more than a definition implies the reality of the thing defined." But if a definition is always of a *thing*, what more is wanted? The definition is admitted to be of *something*; and what is *something* must, I suppose, exist *somehow*.

(4) I still think that in the passage in Mr. Dixon's letter which I referred to under (4) he is not consistent. For if, as he asserts, the definition of *four* as = 1 + 1 + 1, makes it false to say that *Twice two are four*, this is surely because the *facts* referred to by *four* are no longer what they were when the statement in question was true. If definitions were purely arbitrary, as Mr. Dixon holds, what would prevent my saying that *Four* (1 + 1 + 1) *means twice two* (1 + 1) + (1 + 1)? It is surely only the reference to *things* which makes it absurd—(and, however *four* (4) may be defined, how is *one* (1) to be understood, except by reference to things?).

That words and symbols used intelligibly do, and must, refer to something beyond themselves, seems to me indisputable. If they did not, no assertion of the form *S is P* could ever be made, for the symbol S is certainly *not* the symbol P. And for *any* statement, of the form *S is P*, to be possible and significant, it is further necessary that S and P should have *identical* application, but *diverse* signification. If application *and* signification were the same, we should get *S is S* and *P is P*; if application were *not* the same, we must say, S is *not* P. Hence, no term can ever be taken in *mere* denotation (or application), nor in *mere* connotation (signification); but both *momenta* of each term have to be taken into account in every assertion. If (to take a case given by Mr. Dixon in his "Essay on Reasoning," p. 8) we "define" *metal* as "the list of *denotation*, iron, copper, tin, zinc, lead, gold, and silver," then *iron*, &c., can only be pointed out by taking some specimen of iron, and saying, *This and all other things which are* LIKE *it in certain respects*. An absolutely arbitrary denotation can be given only if the *whole* of the objects denoted are severally pointed out; and even then, unless they are labelled, they can only be remembered and identified by means of their *characteristics*; if labelled, by *that* characteristic.

Mr. Dixon objects to my attributing to him the view that "mathematical truths in as far as 'real' are obtained by induction, and are therefore not necessary." But in his letter of December 10 he says:—"For example, the assertion 'Two straight lines cannot inclose a space' is certainly not a 'necessary truth.' Either its terms are defined by connotation, so that its truth depends solely on those definitions, or else its terms are defined by denotation, as representing real things in space; *and the truth of the assertion can only be proved by induction from actual experience with those things*. In the first case, the truth is arbitrary, not necessary; and in the second case it might conceivably be false, as was shown by Helmholtz." It was this passage which led me to the opinion which I expressed.

Cambridge, January 31.

E. E. C. JONES.

Vacuum Tubes and Electric Oscillations.

I HAVE not had the advantage of hearing the lecture of M. Nikola Tesla nor of seeing his experiments, but it does not seem out of place to recall the attention of your readers to an article by Dr. Dragoumis in your issue for April 4, 1889, in vol. xxxix. p. 548.

OLIVER J. LODGE.

THE NEW STAR IN AURIGA.

SINCE our last article was written the weather has continued very bad for astronomical observations. The only new results obtained which have reached us consist of a paper read by Mr. Norman Lockyer at the Royal Society on Thursday last, and an important telegram from Prof. Pickering, which appeared in Wednesday's *Standard*.

We will take these in order. Mr. Lockyer's communication to the Royal Society was dated February 8; it stated that two more photographs, containing many more lines than the former ones, were taken on Sunday night, February 7, and it went on to make the important announcement that "The bright lines K, H, *h*, and G are accompanied by dark lines on their more refrangible sides."

This was substantially the substance of the telegram which appeared in the *Standard* on the following Wednesday (February 10), with the additional remark that the Harvard astronomers thought it possible that the phenomena presented by the new star had been caused by the collision of two celestial bodies.

On the next day the detailed observations made on Sunday night at Kensington, together with the approximate wave-lengths of the lines measured on the photographs, were sent on by Mr. Lockyer to the Royal Society. From these we learn that the Nova on Sunday appeared to be slightly brighter than on February 3.

With the 10-inch refractor and Maclean spectroscope, C was seen to be very brilliant, and there were four very conspicuous lines in the green. Several fainter lines were also seen, and a dark line was suspected in the orange. Mr. Lockyer noticed that some of the lines, especially the bright one near F, on the less refrangible side, appeared to change rapidly in relative brightness, and this was confirmed by Mr. Fowler.

Observations of the spectrum were made by Mr. Fowler with the 3-foot reflector and the Hilger 3-prism spectroscope. These call for no special remark.

Twenty bright lines have been measured on the photographs, and their wave-lengths are given in the accompanying table:—

Lines in the spectrum of Nova Aurigæ.				Bright-line stars.	Orion stars (dark lines).	Nebula in Orion (bright lines).
1st Photo. Date, February 3. λ obtained by means of a curve.	2nd Photo. Date, February 3. λ obtained by means of a curve.	3rd Photo. Date, February 7. λ obtained by means of a curve.	3rd Photo. λ obtained by means of a direct comparison with lines in α Cygni.			
K 3933	3933	3933	3933	—	3933	3933
H 3968	3968	3968	3968	3970	3968	3968
h 4101	4101	4101	4101	4101	4101	4101
4128	4130	4127	4128	—	4130	4130
4172	4172	4172	4172	—	4172	—
—	—	—	4202	4200	—	4200
4226	4227	4228	4226	—	—	4226
4268	4268	—	4264	—	4268	4268
—	—	4294	4291	—	—	—
4312	4310	4310	4310	—	—	—
G 4340	4340	4340	4340	4340	4340	4340
—	—	—	4383	—	—	4383
—	—	—	4412	—	4415	4410
—	—	—	4434	—	—	—
—	—	—	4469	4472	4472	4472
4516	4516	4522	4518	4510	—	—
4552	4552	4554	4555	4550	—	—
4587	4587	4584	4587	—	—	—
4618	4618	4625	4625	4620	—	—
—	—	4860	4860 F	4860	4860	4860

The table also shows probable coincidences with the lines in the spectra of the Wolf-Rayet stars as photographed by Prof. Pickering, dark lines in Orion stars photographed at Kensington, and bright lines in the Orion nebula photographed at Mr. Lockyer's observatory at Westgate.

In addition to the lines recorded in the table, the photographs in the spectrum of the Nova show several lines more refrangible than K. They probably include some of the ultra-violet hydrogen lines.

All the lines in the spectrum of the Nova are broad, although in a photograph of the spectrum of Arcturus, taken with the same instrumental conditions, the lines were perfectly sharp. It is also important to note that the broadening of the lines is not accompanied by any falling off of intensity at the edges, as in the case of the hydrogen lines in such a star as Sirius. With the method employed in taking the photographs, long exposures are liable to result in a thickening of all the lines on account of atmospheric tremors. The lines would also be thick if the Nova be hazy. In the photograph, however, all the lines are not equally thick.

If the lines are similarly broadened when a slit spectroscope is employed, the effect must be due to internal agitations, for if different regions of the Nova are moving with varying velocity, or with the same velocity in different directions, a normally fine line might be widened in the manner observed in the photographs.

With regard to the bright and dark lines the paper states as follows:—

"A somewhat similar phenomenon has already been recorded by Prof. Pickering in the case of β Lyræ, and this has been confirmed by a series of photographs taken at Kensington. In this case the bright lines are alternately more or less refrangible than the dark ones, with a period probably corresponding to the known period of variation in the light of the star. The maximum relative velocity indicated is stated by Prof. Pickering as approximately 300 English miles per second.

"In the case of Nova Aurigæ, the dark lines in all four photographs taken at Kensington are more refrangible than the bright ones, so that as yet there is no evidence of revolution.

"The relative velocity indicated by the displacement of the dark lines with respect to the bright ones appears to be over rather than under 500 miles per second. The reduction is not yet complete.

"Should the photographs which may be obtained in the future continue to show the dark lines displaced to the more refrangible side of the bright ones, it will be a valuable confirmation of my hypothesis as to the causes which produce a new star—namely, the collision of two meteor-swarms. On this supposition the spectrum of Nova Aurigæ would suggest that a moderately dense swarm is now moving towards the earth with a great velocity and is disturbed by a sparser one which is receding. The great agitations set up in the dense swarm would produce the dark-line spectrum, while the sparser swarm would give the bright lines."

ELECTRODYNAMIC THEORIES AND THE ELECTROMAGNETIC THEORY OF LIGHT.[1]

IN a former article we endeavoured to give an account of the first part of M. Poincaré's "Électricité et Optique," in which he dealt with the electric and magnetic theories expounded in Maxwell's treatise. In Part II. he now compares the theory of electromagnetic action given by Maxwell with the somewhat more general theory put forward by Helmholtz in his celebrated paper on the equations of motion of electricity (*Pogg. Ann.*, cii. p. 529, or *Wissensch. Abhand.*, vol. i.); discusses the condition which must hold in order that the two theories may coincide; and, after a masterly exposition of the various consequences which flow from Maxwell's theory, finishes with a very valuable analysis of the theoretical and experimental work of Hertz.

In the first chapter M. Poincaré deals with the formula of Ampère for the mutual action of two current elements. The method adopted is founded on the following three principles assumed from Ampère's experiments:—

(1) That a current in a conductor may be replaced by an equal current in a sinuous conductor nowhere deviating from the first by a distance comparable with the distance of the latter from any element of the other conductor acted upon.

1 "Électricité et Optique." II. Les Théories de Helmholtz et les Expériences de Hertz. Par H. Poincaré, Membre de l'Institut. (Paris: Georges Carré, 1891.)

(2) The action of a closed circuit carrying a current upon any current element is normal to the element.

(3) The action of a closed (non-varying) solenoid upon a current element is zero.

It is besides assumed that the action of a circuit upon a current element is the sum, in the dynamical sense, of the individual actions of the elements of the circuit; and that the action between two elements is a force in the straight line joining their centres.

The process used for the deduction of Ampère's formula from these premisses is very elegant. If ds, ds' be the lengths of the two elements, γ, γ' the currents in them, ϵ the angle between the elements, θ, θ' the angles they make with the line joining their centres, the action of ds on ds' may be represented by $f(r, \theta, \theta', \epsilon)\gamma\gamma' ds ds'$. But the action of ds may, by the first principle stated above, be replaced by the actions, of its components dx, dy, dz; so that

$$f = \mathrm{A}\frac{dx}{ds} + \mathrm{B}\frac{dy}{ds} + \mathrm{C}\frac{dz}{ds},$$

where A, B, C are coefficients. Now, f depends upon $r, \theta, \theta', \epsilon$; r and θ' do not depend on the direction cosines of ds; cos θ and cos ϵ are linear and homogeneous with respect to these direction-cosines. Hence f must be linear and homogeneous with respect to cos θ and cos ϵ, that is with respect to dr/ds, and $d^2r/dsds'$. Similarly, f is linear and homogeneous with respect to dr/ds', $d^2r/dsds'$. Hence we have

$$f = \psi(r)\frac{dr}{ds}\frac{dr}{ds'} + 2\phi(r)\frac{d^2r}{dsds'},$$

where $\psi(r)$ and $\phi(r)$ are functions of r.

These functions are determined by the second and third fundamental principles. The second gives $\psi(r) = \phi'(r)$, so that the problem is reduced to the determination of $\phi(r)$. This value of $\psi(r)$, however, permits f to be written in the form

$$2d\mathrm{U}/dr \,.\, d^2\mathrm{U}/dsds',$$

where U is a function of r only, and

$$d\mathrm{U}/dr = \sqrt{\phi(r)}.$$

From this it is then shown that, if T be the so-called electrodynamic potential (electrokinetic energy) of the circuits—that is, the function the space variation of which, for any direction, is the force in that direction between the circuits—

$$\mathrm{T} = \int\int\frac{d\mathrm{U}}{ds}\frac{d\mathrm{U}}{ds'}dsds',$$

the currents being each unity, and the integrals being taken round the circuits.

The determination of U is then effected by means of the third principle. It is first shown that T may be written as the integral of $\mathrm{F}dx + \mathrm{G}dy + \mathrm{H}dz$ round the circuit to which ds belongs, F denoting the integral round the other circuit of $\mathrm{U}'^2 ds' dr/ds' \,.\, (x - x')/r$, and G, H similar expressions. F, G, H are, in this theory, what Maxwell has called the components of vector potential. These values of F, G, H, it is to be remarked, fulfil the relation

$$d\mathrm{F}/dx + d\mathrm{G}/dy + d\mathrm{H}/dz \; (= \mathrm{J}) = 0.$$

By applying the third principle it is proved that, if ∇^2 have its ordinary signification, and $f'(r) = \mathrm{U}'^2/r$, $(\mathrm{U}' = d\mathrm{U}/dr)$, $\nabla^2 f(r)$ must be a constant, in order that the action of a closed non-varying solenoid on a complete circuit may be zero. Since $f(r)$ must be zero at infinity, this gives $f(r) = k/r$; and if the ordinary electromagnetic definition of unit current be taken, k must be unity, so that $\mathrm{U}' = \pm 1/\sqrt{r}$. Hence the attraction between the elements is

$$2\gamma\gamma' dsds'\frac{1}{r^2}(\cos\epsilon - \tfrac{3}{2}\cos\theta\cos\theta'),$$

Ampère's well-known expression.

The above expressions for F, G, H reduce easily to $\int f(r)dx'$, $\int f(r)dy'$, $\int f(r)dz'$, so that, putting in the value of $f(r)$, we get the well-known value of the mutual energy of the two circuits—

$$\mathrm{T} = \int\int\frac{\cos\epsilon}{r}dsds'.$$

The theory of induction is next taken up. After a short discussion of some objections made by M. Bertrand to the received method of deducing the laws of induction from the observed facts of electromagnetism, M. Poincaré proceeds to show that the electrokinetic energy of two currents is equal to the electrodynamic potential, and recalls Maxwell's application of Lagrange's dynamical equations to the theory of inductive action. He then deals at some length with the celebrated theory put forward by Weber for the action between two quantities, e, e', of electricity, as depending on their distance apart and their motion.

This we pass over, with the remark that Poincaré here discusses certain difficulties to which the theory leads in connection with the value it gives for the action between two current elements, and concludes with a short analysis of Maxwell's examination of the theory of induction as deduced from Weber's law. According to Maxwell ("El. and Mag.," vol. ii. p. 445, second edition), Weber's theory gives, for the inductive electromotive force exerted by the circuit in which the current γ flows on the other, the equation—

$$\mathrm{E} = \frac{d}{dt}\int\int\gamma\frac{1}{r}\frac{dr}{ds}\frac{dr}{ds'}dsds',$$

which, for a closed circuit, agrees with the well-known equation—

$$\mathrm{E} = -\frac{d}{dt}\int\int\gamma\frac{\cos\epsilon}{r}dsds.$$

M. Poincaré points out that this apparent agreement of the two theories is due to the fact that Maxwell has overlooked certain terms which contribute to the value of E, and which do not give a zero result when integrated round a closed circuit.

The expressions given by Weber and Neumann for the mutual potential of two current elements are next considered, and shown to be included in the general expression given for the same potential by Helmholtz. By means of this expression Helmholtz's general electrodynamic theory is introduced, and then follows an elaborate comparison of the theories of Helmholtz and Maxwell. It is shown that Helmholtz's theory leads to the value of T for conduction in three dimensions given by the equation—

$$\mathrm{T} = \tfrac{1}{2}\int(\mathrm{F}u + \mathrm{G}v + \mathrm{H}w)d\varpi,$$

where $d\varpi$ is an element of volume, u, v, w the components of currents, and the integral is extended throughout all space. F, G, H, are, of course, the components of vector potential, and in this theory are given by equations—

$$\mathrm{F} = \int\gamma' dx'/r + \tfrac{1}{2}(1 - k)d\psi/dx, \text{ \&c.},$$

where

$$\psi = \int\gamma' ds' \,.\, dr/ds'.$$

If ρ be the density of free electricity at any point,

$$du/dx + dv/dy + dw/dz = -d\rho/dt,$$

and this, when instead of $\gamma' dr/ds'$ is substituted its value in terms of u', v', w', gives, by an application of Green's theorem, the result—

$$\psi = \int r d\rho'/dt \,.\, d\varpi',$$

where $d\varpi'$ is an element of the space in which the current

γ' is flowing. Now, if in the insulating dielectric two like quantities of electricity, e, e', would produce a repulsive force of magnitude $ee'/\lambda r^2$, the electrostatic potential ϕ, due to the free electricity of density ρ', is given by the equation—

$$\lambda\phi = \int \rho' d\varpi'/r.$$

Thus, from the foregoing values of F, G, H, three equations are obtained, viz.,

$$\nabla^2 F = -4\pi u + (1 - k)\lambda d^2\phi/dxdt,$$

with two similar equations for $\nabla^2 G$ and $\nabla^2 H$. These give

$$dF/dx + dG/dy + dH/dz = -k\lambda d\phi/dt,$$

which is zero if k or λ be zero.

If, now, the magnetic inductive capacity be taken for the moment as unity, and the magnetic force (α, β, γ) be so defined that T is γ times the magnetic induction through the circuit in which the current whose numerical magnitude is γ flows, we deduce easily the equations—

$$\alpha = dH/dy - dG/dz, \text{ \&c. };$$

so that there follow

$$\frac{d\gamma}{dy} - \frac{d\beta}{dz} = 4\pi u - \lambda\frac{d^2\phi}{dxdt},$$
$$\text{\&c.,} \qquad \text{\&c.}$$

These coincide with the equations of currents given by Maxwell when the last terms are omitted. We must therefore either put $\lambda = 0$, or (since $d\phi/dt$ is not in general zero) $\phi = 0$ to reduce to Maxwell's theory.

Poincaré next proves that unless the k in Helmholtz's theory be not less than zero, the sum of the electrostatic and electrokinetic energies may diminish indefinitely from an infinitely small value, so that there would be unstable equilibrium. This affords another reason for rejecting the theory of Weber, in which $k = -1$.

In chapter v. Poincaré passes from the theory of Helmholtz to that of Maxwell. He first considers magnetic and dielectric polarization according to a modified and corrected version of the theory of Poisson. He supposes the dielectric space filled with conducting particles separated by other material, the dielectric proper, which completely insulates these bodies from one another. These conducting bodies are supposed to be electrically polarized, so that electric displacement (f, g, h) is set up in the medium. A parallel theory of magnetic polarization is considered, and the electric displacement is simply the electric analogue of the intensity of magnetization of the medium—that is, the magnetic moment at each point per unit of volume. If ϵ be the ratio of the volume occupied by the conducting particles to the whole volume of the dielectric space in which they are embedded, the specific inductive capacity, K, of the medium is found to be $\lambda\phi(\epsilon)$, where $\phi(\epsilon)$ is a function analogous to $(1 + 2\epsilon)/(1 - \epsilon)$ (the corresponding function for the case in which the conducting particles are spheres) in that it becomes infinite when $\epsilon = 1$.

According to Poincaré, λ is the specific inductive capacity of the dielectric medium proper, or insulator between these conducting bodies, and is very small. In order, therefore, that K may be finite, it is necessary that ϵ may be very nearly equal to unity.

The electrostatic potential, ϕ, at any point is that due to the free electricity present on conductors, and to the electricity developed throughout the medium by its polarization. The electrification, in fact, consists of two parts—a volume density on the dielectric depending on the electric displacement, and of amount $-(df/dx + dg/dy + dh/dz)$, and a resultant surface density, $\sigma' = \sigma - (lf + mg + nh)$, where σ is the surface density of the electricity present in the form of charges on conductors, and $lf + mg + nh$ (in which l, m, n are the direction cosines of the normal to the surface directed inwards towards the dielectric) is the surface distribution due to the polarization. By speaking of this as the electrification, what is meant is that this electrification existing in the dielectric medium proper would give the observed potential, ϕ, at each point. Thus

$$\phi = \frac{1}{\lambda}\left\{\int \frac{d\varpi}{\rho\, r} + \int \sigma' \frac{dS}{r}\right\}.$$

The value of the electrostatic energy, U, according to this dielectric theory, is given by

$$U = \frac{\lambda}{8\pi}\int \Sigma\left(\frac{d\phi}{dx}\right)^2 d\varpi + \frac{2\pi}{K - \lambda}\int \Sigma f^2 d\varpi.$$

If there are applied electromotive forces, X, Y, Z, their values are given by

$$X = -d\phi/dx - 4\pi f/(K - \lambda),$$

with similar equations for Y and Z. Hence, if X, Y, Z = 0 —that is, if there is nothing but electrostatic action—the electrostatic energy is given by

$$U = \int \frac{K}{8\pi}\Sigma\left(\frac{d\phi}{dx}\right)^2 d\varpi.$$

If X, Y, Z are not zero, the electrostatic energy becomes $2\pi/K. \int \Sigma(f^2) d\varpi$ (which is Maxwell's expression), provided $\lambda = 0$. We have to inquire what reasons can be adduced for putting $\lambda = 0$ in this theory.

M. Poincaré shows that the velocity of propagation of a wave of longitudinal displacement in Helmholtz's theory is $\sqrt{K/(K - \lambda)k\lambda}$. On the other hand, the velocity of propagation of a wave of transverse displacement is $\sqrt{1/\mu(K - \lambda)}$. Thus there will be no longitudinal wave if one of the conditions, $k = 0$, $\lambda = 0$, $K = \lambda$, hold. The last condition would make the velocity of propagation of waves of transverse displacement infinite, and must be rejected for every medium, even so-called vacuum, in which light is propagated. Poincaré adopts the second hypothesis.

Further, if to λ a positive value sensibly greater than zero be assigned, a wave of transverse displacement will be given a velocity sensibly greater than that of light. Thus, to pass to Maxwell's theory, it is necessary to make λ insensible, as this gives for the velocity of waves of transverse displacement his value $1/\sqrt{\mu K}$, which is known by experiment to be the velocity of light.

The adoption of this value of λ leads to all the electromagnetic equations of Maxwell, and to the conditions $J = 0$, $du/dx + dv/dy + dw/dz = 0$, the last of which expresses that electricity considered as the analogue of a fluid is incompressible—that is, that all currents flowing are closed currents.

It may be pointed out here that this conclusion agrees with that arrived at by Mr. R. T. Glazebrook in his comparison of Maxwell's electromagnetic equations with those of Helmholtz and Lorentz, and in his further paper on the general equations of the electromagnetic field (Proc. Camb. Phil. Soc., vol. v., Part ii., 1884). Mr. Glazebrook's result is that Φ, the electrostatic potential of Helmholtz's paper must be zero everywhere in order to pass from Helmholtz's theory to that of Maxwell. But Φ is what Poincaré has denoted by $\lambda\phi$, so that the conditions are the same.

It has been attempted to make the transition to Maxwell's theory by putting $k = 0$. This would not suffice alone, as Poincaré points out. For, while giving at once $J = 0$, it would fail to give Maxwell's velocity for transverse waves unless further $\lambda = 0$, which by itself would suffice to effect the transition irrespective of the value of k.

Poincaré thus supposes, in opposition to the older dielectric theories, that even vacuum, or the ether of the inter-planetary spaces, consists of polarizable conducting matter embedded in an insulating medium of infinitely small inductive capacity, λ. According to

Mossotti's theory, which is the starting-point of all mathematical theories of polarization, the conducting particles are spherical, and therefore, if ϵ be the ratio of the volume of the spheres to the total volume of the medium, the value of K is $(1 + 2\epsilon)/(1 - \epsilon)$. It is here assumed that the specific inductive capacity of the insulating dielectric is unity. Poincaré, however, sees no reason for making this particular assumption, and takes it as λ, a quantity which, if Maxwell's theory be the true one, must be exceedingly small. This involves, as already stated, $K = \lambda\phi(\epsilon)$, where $\phi(\epsilon)$ is a quantity which becomes very great when $\epsilon = 1$. Thus, according to Maxwell's theory, the conducting particles are separated by infinitely thin insulating partitions, so that they practically fill the whole space. Of course, the physical fact may be very different from that here supposed: the theory only furnishes a picture, not perhaps altogether clear and intelligible, of the structure of the medium and its functions.

It may be said that the infinitely small inductive capacity, λ, of the medium, itself requires physical explanation. This is quite true; but so also does the specific inductive capacity equal to unity assumed for vacuum or air in the ordinary theories. In fact, such dielectric theories as have been put forward, involving merely polarization of the medium, only give an explanation of the difference between the electric behaviour of one medium and another, and furnish none whatever of the real *rationale* of the propagation of electric action.

That the value of ϕ may be finite, it is necessary that the values of the volume density, ρ, and the surface density, σ', may be infinitely small, since

$$\phi = \int \frac{\rho}{\lambda r} d\varpi + \int \frac{\sigma}{\lambda r} dS.$$

Here ρ is the volume density due to the surface distributions on the opposite faces of the partitions between the conducting particles, and this, it is easy to see, will be infinitely small. Also, σ' is the sum of the actual density (surface density of charge) on the surface of the conductors, and the density, which is the surface manifestation of the polarization of the conducting particles, or $\sigma' = \sigma - (lf + mg + nh)$. This also can be conceived as exceedingly small, so that ϕ may have a finite value.

Further reasons for preferring the theory of Maxwell are discussed in chapter vi., which is entitled "The Unity of Electric Force." This chapter consists of an exposition of Hertz's modification of Maxwell's electromagnetic theory—a modification, it is to be remarked, practically given also, but in vector form, by Mr Oliver Heaviside, in various papers in the *Philosophical Magazine*. When made, it exhibits a striking parallelism between the equations of electric and magnetic force, and leads to some remarkable theorems. Using Maxwell's equations, and deviating slightly from Poincaré's mode of presenting the equations, we have, if k now denote electric conductivity of the medium, and P, Q, R components of electric force—

$$\left(k + \frac{K}{4\pi}\frac{d}{dt}\right)P = \frac{1}{4\pi}\left(\frac{d\gamma}{dy} - \frac{d\beta}{dz}\right),$$

with two similar equations for Q and R. But also we have—

$$\frac{\mu}{4\pi}\frac{d\alpha}{dt} = -\frac{1}{4\pi}\left(\frac{dR}{dy} - \frac{dQ}{dz}\right),$$

with two similar equations for β and γ. These last may, by the introduction of a non-existent quantity, g, be written—

$$\left(g + \frac{\mu}{4\pi}\frac{d}{dt}\right)\alpha = -\frac{1}{4\pi}\left(\frac{dR}{dy} - \frac{dQ}{dz}\right),$$

&c., &c.

The quantity g, Heaviside points out, is the proper magnetic analogue to k, and may therefore be called the magnetic conductivity. Its reciprocal would be the true magnetic resistivity of the medium. Of course, in an insulating medium k is also zero.

According to Maxwell's theory, P, Q, R, α, β, γ fulfil the equations—

$$\frac{dP}{dx} + \frac{dQ}{dy} + \frac{dR}{dz} = 0,$$

$$\frac{d\alpha}{dx} + \frac{d\beta}{dy} + \frac{d\gamma}{dz} = 0;$$

the first, since $du/dx + dv/dy + dw/dz = 0$, and the second because the magnetic force in the medium, being supposed purely inductive, must fulfil the solenoidal condition, except at the (vortex) origin of the disturbance.

There is therefore, in Maxwell's theory, a perfect reciprocity of relation between the electric and magnetic quantities. Hence we might infer, from the magnetic phenomena following from electric currents or flow of electricity, an analogous set of electric phenomena following from the flow of magnetism. Now we know that if a magnet varies in strength it produces an electromotive force of components P, Q, R, at every point of the surrounding space. This we may suppose due to a current of magnetism flowing from one end of the magnet to the other, and thus producing the variation in the magnet's strength. The directions of the components of electric force at any point are in fact coincident with those of the components of vector potential produced by the magnet at that point, and are equal to the time-rates of variation of these components. But why should this not be regarded as an electrostatic field in the ordinary sense of the term? For example, a current of electricity, flowing round a closed circuit, produces a magnetic field equivalent to that which would be produced by a magnetic shell of proper strength, and having its edge coincident with the circuit. Of this current a closed solenoid varying continuously in magnetic strength (for example, a closed solenoid in which the magnetizing current is varying in strength) is the magnetic analogue, and ought in the same way to be equivalent to an *electric shell*, in the sense of producing an identical electric field. Such a shell ought to be subject in an electric field to dynamical action; and further, two such varying solenoids ought to exert the same mutual dynamical action, as would the two equivalent electric shells if placed in the same configuration. The second of these conclusions asserts that the dynamical action on such a shell depends only on the electric field in which it is placed, and that its action on the other varying solenoid is due to its producing exactly the same electric field as the equivalent electric shell would produce. This is what Poincaré gives as Hertz's principle of the unity of electric force.

Of course, it is to be noticed that the second conclusion does not follow from the first. We cannot reason that because the mutual action of an electric shell and a varying solenoid is the same as that of two electric shells, therefore the mutual action of two solenoids is the same as that of two electric shells.

If, however, we assume that the dynamical action on a closed varying solenoid depends only on the electric field in which it is placed, we can say that the mutual action of two varying solenoids is the same as that of their equivalent electric shells.

M. Poincaré calculates the work done in effecting a relative displacement of two such varying solenoids, and finds that it is equal to the change in the electrostatic energy of the system, as the change in the electrokinetic energy is all accounted for otherwise. Now, the electrostatic energy of a system is given as we have seen by the equation—

$$U = \int \left\{ \frac{\lambda}{8\pi}\Sigma\left(\frac{d\phi}{dx}\right)^2 + \frac{2\pi}{K - \lambda}\Sigma(f^2) \right\} d\varpi,$$

where

$$f = -(K - \lambda)/4\pi \,.\, dF/dt + d\phi/dx.$$

This may be written

$$U = \int \left\{ \frac{\lambda}{8\pi} \Sigma \left(\frac{d\phi}{dx} \right)^2 + \frac{K - \lambda}{8\pi} \Sigma \left(\frac{d\phi}{dx} + \frac{dF}{dt} \right)^2 \right\} d\varpi.$$

Now, if there be no inductive action—that is, if the field be wholly electrostatic—

$$dF/dt,\ dG/dt,\ dH/dt = 0,$$

and hence, for two electric shells—

$$U = \int \frac{K}{8\pi} \Sigma \left(\frac{d\phi}{dx} \right)^2 d\varpi.$$

On the other hand, if the action be wholly inductive—that is, if we have no so-called electrostatic action—

$$d\phi/dx,\ d\phi/dy,\ d\phi/dz = 0,$$

and for two varying solenoids we have—

$$U' = \int \frac{K - \lambda}{8\pi} \Sigma \left(\frac{dF}{dt} \right)^2 d\varpi.$$

If, then, there be the same mutual action between the two varying solenoids as between their equivalent electric shells, we must have $U = U'$. But because of the equivalence the value of dF/dt, &c., produced at any point by either solenoid, must be the same as those of $d\phi/dx$, &c., produced at the same point when the solenoid is replaced by its equivalent electric shell. Thus we get $U/U' = K/(K - \lambda)$, and therefore $\lambda = 0$. Thus, if the principle of the unity of electric force is true, $\lambda = 0$, and we have Maxwell's theory.

Dr. Oliver Lodge has, as is well known, endeavoured to detect the existence of an electrostatic field produced by varying magnetic action (NATURE, May 23, 1889, and *Electrician*, May 17, 1889), and has reason to believe that he has been successful. It is also possible, as Poincaré suggests, that this kind of electrostatic action may be developed when iron rings, &c., are placed in the field of an alternating electromagnet, as in the experiments of Elihu Thomson.

In a note, which forms a supplement to the comparison of the theories of Helmholtz and Maxwell, M. Poincaré points out that when the mutual action of a varying solenoid and an electric shell is considered, contradictory results are obtained according as the solenoid is regarded as fixed and the shell movable, or the shell fixed and the solenoid movable. Thus the theory of Helmholtz in this application does not give fulfilment of the third law of motion.

Possibly, some such theory as this may throw some light on the electric phenomena of voltaic cells, with their finite steps of potential across the surfaces of separation of dissimilar substances, and help to refer the production of all currents to the single cause—electromagnetic action.

We come now to the discussion which the book contains of the experiments of Hertz. This fills considerably more than one-half of the work, and we cannot, in the space left at our disposal, give an adequate account of it. Of the experiments themselves it is not necessary to say anything, as they have been fully and ably discussed in NATURE by Mr. Trouton (February 21, 1889). Hertz's own theory of the radiation of electric and magnetic energy has also been given in these pages by Dr. Lodge (February 21, 1889, *et seq.*). Poincaré's presentment of the theory is, however, marked by many points of originality, and abounds in acute and interesting remarks.

The theory of the dumb-bell exciter used by Hertz is first considered, then the field produced is discussed, and, last of all, the action of the resonator or receiver is dealt with. Taking the exciter as a couple of spheres, 15 cms. in radius, placed with their centres 150 cms. apart, and joined by a wire ½ cm. in diameter, Poincaré calculates (1) the capacity, (2) the self-induction of the arrangement. The value of the capacity of the arrangement of two spheres used by Hertz in the calculation of the period of the exciter was that of each of the spheres by itself, viz. 15 cms. Now, if one of the spheres were alone in its own field with a charge q and at a potential V, we should have $q/V = 15$. But at any instant when the charge of one sphere is q, that of the other sphere is $-q$; and since the spheres may be taken as nearly without mutual influence, the difference of potential between them is 2V. The capacity is, then, $q/2V$, or 7·5 cms.—half the value used by Hertz. That this is the proper value to use for the capacity is easily verified by a reference to the mode of establishing the equation of oscillation, when it is seen that the capacity is really defined by that equation as the charge on one of the spheres divided by their difference of potential.

The calculation of the self-induction given by Poincaré is interesting. Regarding, as an approximation, the currents in the spheres, and the influence of the spark-interval, as negligible, and taking the wire as of length l (equal to the distance between the centres of the bulbs), and of diameter d, and assuming that the current is wholly on the surface of the wire (which it is approximately, when in rapid alternation) he finds—

$$L = 2l \left\{ \log \frac{4l}{d} - 1 + \frac{k - 1}{2} \right\},$$

where k is the quantity which appears in Helmholtz's theory.

This differs from the value given by Hertz in having -1 for the middle term within the brackets instead of $-·75$, and $(k - 1)/2$ instead of $(1 - k)/2$ for the third term. The first discrepancy arises through the currents having been taken by Hertz as uniform over the cross-section of the wire, and the second probably through an error in sign. The self-induction, L, is 1902 cms. if the term involving k is not taken into account, and (1902 + 150) cms. if k be put equal to zero.

Calculating the period $T (= 2\pi\sqrt{LC}$, where C is the capacity in electromagnetic units), we find it to be $2·51 \times 10^{-8}$ seconds, and multiplying by the ratio of the electromagnetic unit of quantity to the electrostatic unit, or v, we get for the wave-length 7·53 metres. Hertz gives 1·77 for the calculated half period, and 5·31 for the corresponding half wave-length. On account of the error in the estimation of the capacity, it is clear that this value of the half period and half wave-length must be divided by $\sqrt{2}$, and this brings them into agreement with the values first stated. There is, however, a serious discrepancy between the results of theory and experiment, which we shall notice presently.

The calculation of period, &c., of course proceeds on the assumption that the resistance is negligible, and this is no doubt the case to a sufficient degree of approximation. In the theory itself, also, no account is taken of the induction coil or of the displacement currents in the dielectric; further, the energy is dissipated, not merely by the production of heat, but by radiation into the dielectric. That the influence of the induction coil is indeed negligible Poincaré gives reasons for supposing; in fact, on account of the enormous self-induction of the induction bobbin, and the small mutual induction of the exciter and the bobbin, the corrected differential equation is the one formerly found for the oscillation, with the addition of an exceedingly small term, so that the solution is practically the same as before. (Here, p. 162, the expression "*a* étant très grand" should be "*a* étant très petit"). As Poincaré states, the vibration of the exciter is like that of a very small pendulum attached to a massive pendulum of long period; the period of the former is very little affected by its mode of support. For a similar reason the period is very little affected by the very considerable capacity of the bobbin.

Experimenting on the velocity of propagation of electromagnetic waves, Hertz found that the half wave-length in air was about 4·5 metres, the corrected period of the vibrator used being about 2×10^{-8} seconds. This gives a velocity of propagation of $900/(2 \times 10^{-8})$ (or $4{\cdot}5 \times 10^{10}$) cms. per second, exceeding the velocity of light by about 50 per cent. For the wave-length in wires, however, he found a value which gives a velocity of propagation nearly equal to the velocity of light, when the correction of the period for error in capacity is taken into account. (Later, M. Poincaré gives the half wave-length in air for these experiments as 4·8 metres.)

Herr Lechner, experimenting at Vienna, has also found a velocity of propagation in wires very approximately equal to the velocity of light, which thus confirms Hertz's result. On the other hand, MM. Sarasin and de la Rive, experimenting at Geneva in 1890, found that the wave-length observed depends very much on the dimensions of the resonator. But using an exciter exactly similar to that of Hertz, and of the same dimensions, and a resonator 75 cms. in diameter, and therefore nearly an exact copy of that employed by Hertz, they found a half wave-length of 3 metres, instead of 4·8 metres as found by Hertz. Thus there is a discrepancy between the two results which it is difficult to explain. Hertz himself gives a possible explanation, in a letter to M. Poincaré which is quoted in a note on some recent experiments which is printed as an appendix. On account of its interest, we take the liberty here of translating the extract quoted. It is to be noted that what is called the wave-length here is the distance from node to node, or half the complete wave-length.

"It is difficult for me to believe that I have been misled in the second method into finding 4·8 metres instead of 3 metres; but since the result of Messrs. Sarasin and de la Rive has every theoretical appearance of truth, I have endeavoured to find out the cause of the difference. Here are two ways of explaining it. The waves were produced between two parallel walls of a room, and I have taken account of the reflective action of only one of them. Let us suppose, to begin with, that the length of the room is an exact multiple of the wave-length, say three wave-lengths. We shall have two well-marked nodes at the exact distance. If the length of the room is four wave-lengths, we shall have three well-marked nodes. But if the length of the room is intermediate between these, and nearer the former than the latter, we shall have two less distinct nodes at a distance apart greater than a wave-length. This explanation would appear to me satisfactory, if the difference were not too great.

"The other way of explaining the difference is this. My reflecting plate of zinc was fixed in a niche in the wall, and it is possible that the projecting parts of the wall may have had the effect of carrying off the nodes to a greater distance from the wall, and thus of giving too great an apparent length as measured. But it is also true that the niche was from 5 to 6 metres in width, and it does not seem to me very probable that it can have had any great effect.[1]

"I therefore cannot tell precisely the cause of my error; but I believe there must be some way of explaining it. For a long time I have sought in vain to find a probable cause for the difference of velocity in air and in wires; and I had myself found, before Messrs. Sarasin and de la Rive, that there is no difference for short waves of 30 cms. in length. The results of these gentlemen, however, give the same velocity for long waves, and contradict my experiments."

Connected with this point M. Poincaré has some instructive remarks on what Messrs. Sarasin and de la Rive have observed and called multiple resonance, and which has also been observed by Fitzgerald and Trouton. Their supposition is that the exciter gives rise neither to a single vibration of distinct period, nor to a limited number of distinct vibrations, but rather to such a complex of vibrations as would give a wide band of continuous spectrum. Thus all vibrations, agreeing with possible modes of vibration of the resonator would be reinforced. That this explanation is not borne out by the theory is true, but on account of the incompleteness of the theory it is not possible to attach much weight to this fact. It is hard to believe that the vibrations can be perfectly simple.

Poincaré proposes, however, the following explanation. For various reasons, he thinks the logarithmic decrement of the vibrations of the exciter is probably much greater than that of the resonator, and so the vibrations of the exciter diminish in amplitude more quickly than those which by any cause are set up in the resonator. Thus the resonator, being started by the exciter, would continue its vibrations after those of the exciter had become insensible, but would then vibrate in its own proper period, thus giving vibrations of longer period and of greater wave-length than those which excited it. The wave-length, being determined by interference, and used with the too short period of the exciter, would of course give too great a velocity of propagation. With this explanation Hertz has expressed himself as practically in accord, and so a possible way out of the difficulty seems opened up. As Hertz remarks, the oscillations of the exciter, represented graphically, do not give a curve of sines pure and simple, but a curve of sines the amplitude of which gradually diminishes. Such an oscillation will cause all resonators receiving it to vibrate, but those in tune with the exciter more violently than the others. A mathematical investigation of the point is given by M. Poincaré, which explains the result, shown by experiment, that the apparent spectrum found by Sarasin and de la Rive seems more extended when wires are connected to the vibrator, than when the propagation takes place freely in air.

Whether this explanation be satisfactory or not, there can be no doubt, on the whole, that the electromagnetic theory of light is substantially true. The theory is far from complete, and there are many outstanding points which require further theoretical and experimental elucidation. Some of these are touched on by Poincaré in his discussion of the field produced by exciters of different forms, and the theory of the resonator, but especially in a valuable series of notes which he has added to his lectures. These deal with special topics, which are there treated with more detail than was possible in the body of the work. Such, for example, are his notes on multiple resonance, the calculation of the period, and the propagation of waves in sinuous wires.

This article has run to too great a length, and must here close. M. Poincaré's work ought to be read by everyone interested in Maxwell's great scientific generalization—the greatest, perhaps, ever made by a natural philosopher since the days of Newton—and in its remarkable experimental verification by Hertz. There never was, perhaps, a time of greater mathematical and physical activity than the present, but withal it is marked by a care for the scientific student which no previous age ever displayed. It is no small encouragement to humbler scientific workers when masters of analysis like M. Poincaré take the trouble to publish, in a connected form, their lectures and researches on the current scientific questions of the day. Besides earning the gratitude of those who are thus admitted within the circle of their pupils, by immediately communicating their discoveries and expositions in this manner to the general scientific public, they multiply many-fold the direct effect of their work on scientific progress.

A. GRAY.

[1] For an interesting discussion of the effects of reflecting plates of different dimensions, see a paper by Mr. Trouton in the *Phil. Mag.*, July 1891.

ON SOME POINTS IN ANCIENT EGYPTIAN ASTRONOMY.[1]

II.

4.—*The Probable Date of the Founding of Denderah as derived from the Account of the Building Ceremonial.*

SO much having been stated relating to the inscriptions recording building ceremonials, I will now return to the statement regarding Denderah, to see what can be made of it on the view that either the middle or the chief point—that is, the brightest star of the constellation of the Great Bear as we now know it—was the part referred to, and that the cord was stretched to the star on the horizon.

The first question which arises is, Was there any reason why δ Ursæ Majoris at the centre, or α the brightest, should have been used as the orientation point at any time? Was there any reason why any special sanctity should have been associated with either? Certainly not in the case of δ on account of its magnitude, because Dubhe, not far from it, is much brighter And certainly not in the case both of δ and α on account of the time of their heliacal rising. We seem therefore in an *impasse* along this line of inquiry, but a further consideration of the question brings out the remarkable fact that at two different points of time the North Polar distance of α Ursæ Majoris was nearly the same as that of α Lyræ and γ Draconis, so that α Lyræ would be visible at one of the dates and γ Draconis with α Ursæ Majoris at the other—all rising in the same amplitude.

The stars rising at the same amplitude, a temple directed to one would be directed to the other, but the stars would rise at different times. This, it may be suggested, may have been the reason why α was used by the king: the most convenient hour of the night was chosen. But there may have been another reason.

We know enough of the Egyptian priests to imagine it might be to their interest that even the king himself should not know everything, and the question arises whether, knowing the equal amplitudes of the risings of these stars, the secret was retained while α Ursæ Majoris was used.

This, of course, can only be put forward as a suggestion, and to many, no doubt, it will seem to be far-fetched; but from the account given by Herodotus of some of the ceremonials and mysteries connected with the temple of Tyre, it is suggested that the priests used star-light at night for some of their operations very much in the same way as they might have used sunlight during the day. According to Herodotus, in the temple in question there were pillars of gold and emerald which shone at night. Now, there can be little doubt that in the darkened sanctuary of an Egyptian temple the light of α Lyræ, one of the brightest stars in the northern heavens, rising in the clear air of Egypt would be quite strong enough to throw into an apparent glow such highly reflecting surfaces as those to which Herodotus refers.

Supposing such a ceremonial as this used at Denderah, the less the worshippers—who, reasoning from the analogy of the ceremonial termed the manifestation of Rā,[2] would stand facing the sanctuary with their backs to the chief door of the temple—knew about the question of a bright star which might probably produce the mystery the better; so that it may be almost said that the priests had a reason, and a very good one, for using a comparatively faint star for the orientation of a temple, some of the uses of which were to utilize for their own purposes some of the phenomena presented by a bright one rising at the same amplitude. These considerations have, however, only full force as between α Ursæ Majoris and α Lyræ.

Of course, so far, nothing can be said with certainty with regard to either of the stars in question having been chosen for the original orientation, but in an inquiry of this kind no line of evidence is to be neglected.

We next come to possible dates. Taking the amplitude of Denderah as 73°, the dates of foundation given by either α Lyræ or γ Draconis will be those at which these stars had an equal amplitude.

Roughly, and only roughly, these dates would have been as follows:—

	Horizon 2° high
α Lyræ	7200 B.C.
γ Draconis	4400 B.C.

Now, what are the records concerning this temple? We know that the structure now visible to us was built in the time of the last Ptolemies and the first Roman Emperors, and I have already shown that at those dates the Great Bear (the old Thigh) did not rise at all, as it was circumpolar.

But it is also known that there was a temple here in the time of Thotmes III., and even earlier, going back to the earliest times of Egyptian history. King Pepi, of the 6th dynasty (*circ.* 3233 B.C.), is portrayed over and over again in the crypts.

Even this is not all the evidence in favour of a high antiquity. In one of the crypts (No. 9), according to Ebers and Dümichen there are two references to the earliest plans of the temple. One inscription states that the great ground-plan (*Senti*) of Ant (Denderah) was found in old writing on parchments of the time of the followers of Horus (sun-worshippers) preserved in the walls of the palace during the reign of King Pepi. Another inscription goes further, referring to the restoration by Thotmes III. (*circ.* 1600 B.C.) of the temple to the state in which it was found described in old writings of the time of the King Chufu (Cheops) of the 4th dynasty (*circ.* 3733 B.C.). If any faith is to be placed in this inscription, it seems to me to suggest a still higher antiquity. There would have been more reason for describing an antique shrine than a brand new one, and the date 4400 is well within the historical period, according to Mariette.

If, then, I am right in my suggestion as to the word *ak* referring to α Ursæ Majoris, and as to a star rising at an equal amplitude to γ Draconis being for some reason made use of in the building ceremony, we find the closest agreement between the astronomical orientation, the definite statement as to a certain star being used in the building ceremonies, and the inscription in the crypts referring to Cheops as the earliest historical personage who describes the building. I must confess that this complete justification of the double record strikes me as very remarkable, and I think it will be generally conceded that further local observations should be made in order to attempt to carry the matter a stage further.

If the above results be confirmed we have a most important indication of the fact that in the rebuilding in the time of Thotmes III. and of the Ptolemies, the original orientation of the building was not disturbed, and that in the account of the building ceremonies we are dealing as surely with the laying of the first foundation stone as we are dealing with the original plan.[1]

[1] Continued from p. 299.

[2] One of the inscriptions relating to the manifestation of Rā has been translated by de Rougé as follows:—

"Il vint en passant vers le temple de Rā; il entra dans le temple en adorant (deux fois). Le χer-heb [celebrant] invoqua (celui qui) repousse les plaies du roi; il remplit les rites de la porte; il prit le seteb, il se purifia par l'encens; il fit une libation; il apporta les fleurs de *Habenben* [a part of the temple]; il apporta le parfum (?). Il monta les degrés vers l'adytum grand, pour voir Rā dans Habenben; lui-même se tint seul; il poussa le verrou; il ouvrit les portes; il vit son père Rā dans Habenben; il vénéra la barque de Rā et la barque de Tum. Il tira les portes, et posa la terre sigillaire (qu'il) scella avec le sceau du roi. Lui-même ordonne aux prêtres, 'J'ai placé le sceau; que n'entre pas quelqu'un dedans de tout roi qui se tiendra (là).'"—"Chrestomathie Egyptienne," de Rougé, iii. p. 60.

[1] On this point I am permitted by Prof. Maspéro to print the following extract from a letter I received from him:—

"Tous les temples ptolémaïques et la plus grande partie des temples pharaoniques sont des *reconstructions*. Ce que vous avez observé de

Here, however, it is necessary to proceed with caution, for the last word may not yet have been said when we accept γ Draconis.

I have elsewhere pointed out that it is not impossible that a temple once oriented to a certain star, and long out of use on account of the precessional movement, may be utilized for another, and be rehabilitated in consequence, when that same movement brings another conspicuous star into the proper rising amplitude.

In the present case, the orientation fits γ Draconis in the historic period, but it also fits α Lyræ in the times of the Hor-schesu, the dimly seen followers of Horus, or sun-worshippers, before the dawn of the historic period. If we assume that the record is absolutely true (and I for one believe in these old records more and more), and that Cheops only described a shrine founded by the Hor-schesu, then we are carried back to *circ.* 7000 B.C. I am indebted to my friend Dr. Wallis Budge, for the suggestion that the position of Denderah on the highway from the Red Sea—which may soon be reached by a railway from Keneh to Kosseir !—would make it one of the most important places in Ancient Egypt.

In any case the consideration has to be borne in mind that the series of temples with high northern and southern amplitudes at Denderah, Abydos, Thebes, Philœ, Edfu, were nearly certainly founded before the time at which the heliacal rising of Sirius, near the time of the summer solstice, was the chief event of the year, watched by priests, astronomers—if the astronomers were not the only priests—and agriculturalists alike. Now we know from Biot's calculations that this first took place *circ.* 3285 B.C., and that Sirius—though, as I am informed by Prof. Maspéro, *not* its heliacal rising—is referred to on inscriptions in pyramid times. The Sirius temples at Thebes, Philœ, and Denderah, are in all probability of much later foundation than those to which I have referred, and that at Denderah seems from its orientation to be the latest of all.[1]

I will, however, leave for a future occasion the question of the original Hor-schesu shrine, and consider at present an important relation between the chief temple at Denderah and one of the chief temples at Karnak. I am not aware that the relation has been pointed out before.

The amplitude of the temple at Denderah, dedicated to *Hathor*, is 73° N. of E. The amplitude of the temple dedicated to Mut or Maut at Karnak is 71° N. of E., which, assuming the same star to have been used, corresponds to a date (according to the height of the horizon) of *circ.* 3000 to 3500 B.C. This is therefore later than the Hathor temple of Denderah.

Now, we have it from Plutarch (*Isis* and *Osiris*, Parthey, cap. 56) that Isis = Maut = Hathor = Methuer, and this is sufficiently clear from the symbols of these goddesses, without his authority.

It is fundamental for the orientation theory that the cult shall follow the star. But we have here the same cult. Hathor and Maut are merely *local* names associated with local totems. Isis is a *generic* name simply, meaning an accompaniment of sun-rise, whether that light be the dawn, or a heliacally rising star, or even the moon. The generic symbol for Isis is the sun's disk and horns, which I think may not impossibly be a poetic development of the sign for sunrise. The local totem of the special warning star in use at any time or place may be anything: hippopotamus, crocodile, hawk, vulture, lion, or even some other common living thing into which the totem degraded when the supply of the original fell short.[1] Hence, as the number of warning stars was certainly very restricted, they, or rather the goddesses which typified them, had different names in almost every nome. Hence Egyptian mythology should be, as it is in fact, full of synonyms; each local name being liable to be brought into prominence at some time or another owing to adventitious circumstances, relating either to dynasties or the popularity of some particular shrine.

Dendérah, est vrai d'Esnéh, d'Ombos, d'Assouan, de Philæ, &c. Or, si les premiers constructeurs d'un temple—ou chez nous d'une église—peuvent choisir presque à leur gré l'emplacement et par suite l'orientation, la plus convenable, il en est bien rarement de même des *reconstructeurs*. Les maisons accumulées autour du temple les gênaient, d'ailleurs les habitudes du culte et de la population étaient prises : on rebâtissait le temple—comme d'ordinaire chez nous on rebâtit l'église—sur la même orientation et sur les mêmes fondations. J'ai constaté le fait à Kom-Ombo, où les débris du temple décoré par Amenhotpou I. et Thoutmosis III. sont orientés exactement comme ceux du temple ptolémaïque actuel, bâti *sur* les ruines du précédent. Vous avez donc le droit de dire, non seulement pour Dendérah, mais pour beaucoup d'autres temples, qu'ils ont été reconstruits sur l'orientation du temple qu'ils remplaçaient, quand même cette orientation ne répondait plus à la réalité des choses."

[1] I have not yet reduced my own observations of this temple, but Nissen refers to previous measures, *Rheinisches Museum für Philologie*, 1885, p. 44.

Let us concede, then, that we had the same cult. Now about the star. For Denderah we have already found γ Draconis. What about Thebes ? As I have elsewhere pointed out, the temple at Karnak, the date of the building of which is the most certain, undoubtedly pointed to γ Draconis.[2] Its amplitude is 62° N. of E. This was in 1200 B.C. Nor is this all ; as I have also shown, it forms the second of a series with the following amplitudes:—

$$\left.\begin{array}{r}61^\circ\\62^\circ\\68^\circ\\71^\circ\end{array}\right\}\text{N. of E.}$$

Now the last in this series, directed on this view to γ Draconis at Karnak, is precisely the temple of Maut !

So that here we have a very concrete case of the cult following the star, not only in the same place, but at different places, and we are driven to the conclusion that Hathor at Denderah and Maut at Thebes, exoterically different goddesses, were esoterically the same star γ Draconis.

Although it carries us still further into the region of mythology, there is more evidence to be gathered from a consideration of the old constellations. I do not think it saying too much to remark that among these the attention of the early Egyptians was almost exclusively confined to the circumpolar ones. Further, the mean latitude being, say, 25°, the circumpolar region was a restricted one, 50° in diameter, instead of over 100°, as with us. But not quite exclusively, for to them then, as to us now, the Great Bear and *Orion* were the two most prominent constellations in the heavens ; for them, as for us they typified the northern and southern regions of the sky

There can be no question that the chief ancient constellation in the north was the Great Bear, or, as it was then pictured, the Thigh (Mesχet). After this came the Hippopotamus. I had come to the conclusion that this has been replaced on our maps by part of Draco before I found that Brugsch and Parthey were of the same opinion.

The female hippopotamus typified Taurt, the wife of Set (represented by a jackal with erected tail, or hippopotamus), and one of the most ordinary forms of Hathor presents us with the horns and disk surmounting a hippopotamus. There is evidence that the star we are considering, γ Draconis, occupied the place of the head or the mythical headgear.

So far, then, mythology is with me ; but there is a difficulty. According to the theory the cult must follow the star : this must be held to as far as possible. But suppose the precessional movement causes the initial function of a star to become inoperative, must not the cult—which, as we assume, had chiefly to do with the heralding of sunrise at one time of the year or other—change ? And if the same cult is conducted in connection with another star, will not the old name probably be retained ?

There is another temple of Hathor at Thebes—the

[1] Have we such instances of degradation in the cat replacing the lion and the black pig the hippopotamus, to give two instances ?

[2] I refer to the Temple M of Lepsius, built by Ramses III.

temple of Dêr el-Baḥari, founded or embellished by Queen Hatshepset (*circ.* 1600 B.C.). This temple, instead of being oriented 73° N. of E., lies 26° S. of E., it can never, therefore, have faced the star observed at Denderah.

Now, are there any possible explanations? Two have suggested themselves to me.

At Denderah the image of the goddess was taken on a certain day in the year on to the terrace, so that the light of the sun—her father Rā—might fall upon her.

Building a temple so that the sunlight might enter it once or twice a year (which could not happen in the Denderah temple in consequence of its northern outlook) would enable the aforesaid operation to be performed in the sanctuary itself. The Thebes temple on this hypothesis assured this—and at the winter solstice.

The next explanation I submit to Egyptologists with much fear and trembling. It is, briefly, that about 3200 B.C. observations of the star Sirius replaced, or were added to, those made of γ Draconis. Mythologically a new Isis would be born.

I base this suggestion on the following considerations:—

(1) While the Denderah Hathor was represented by the disk and horns on a hippopotamus; at Thebes, the city of the "Bull" Amen, Hathor is represented by a cow with a like headdress.

(2) Sirius, represented originally as a goddess with the two feathers of Amen standing in a boat, is now changed to a cow with the disk and horns.

(3) Hathor was the cow of the western hills of Thebes. It is in these hills that the temple Dêr el-Baḥari lies, and this temple if oriented originally to Sirius would have been founded about 3000 B.C., when Sirius would have an amplitude of 26°.

FIG. 4.—The cow Hathor appearing from the western hills of Thebes.

(4) A temple was built in later times at Denderah oriented to Sirius, and Sirius with the cow's horns and disk became the great goddess there, and when her supremacy all over Egypt became undoubted, her birthplace was declared—at Denderah—to have been Denderah.[1]

(5) In the month list at the Ramesseum the first month is dedicated to Sirius, the third to Hathor. This is not, however, a final argument, because *local* cults may have been in question.

(6) Set seems to have been a generic name applied to the northern (? circumpolar) constellations, perhaps because *set*=darkness, and these stars, being *always visible* in the night, may have in time typified it. Taurt, the hippopotamus, was the wife of Set. The Thigh was the thigh of Set, &c. γ Draconis was associated therefore with Set, and the symbolism for Set-Hathor was the hippopotamus with horns and disk. Now, if, as is suggested, Sirius replaced γ Draconis, and the cow replaced the hippopotamus, the cult of Set might be expected to have declined; and as a matter of fact the decline of the worship of Set, which was generally paramount under the earlier dynasties, and even the obliteration of the emblems on the monuments, are among the best-marked cases of the kind found in the inscriptions.[1]

(7) The *Isis* temples of Denderah were certainly oriented to Sirius; the *Hathor* temple was as certainly *not* so oriented. And yet, in the restorations in later times (say Thotmes III.—Ptolemies), the cult has been made Sirian, and the references are to the star which rises at the rising of the Nile.

I do not see why the Egyptians should have hesitated to continue the same cult under a different star when they apparently quite naturally changed Orion from a form of Osiris (Sah-Osiris) and a mummy (as he was represented when the light of his stars was quenched at dawn at the rising of Sirius) to that of Sah-Horus (when in later times the constellation itself rose heliacally).

J. NORMAN LOCKYER.

[1] Brugsch thus translates one of the inscriptions:—"Horus in weiblicher Gestalt ist die Fürstin, die Mächtige, die Thronfolgerin und Tochter eines Thronfolger. Ein fliegender Käfer wird (sie ?) geboren am Himmel in der uranfänglichen Stadt (Denderah) zur Zeit der Nacht des Kindes in seiner Wiege. Es strahlt die Sonne am Himmel in der Dämmerung, wann ihre Geburt vollbracht wird."—Brugsch, "Astron. Inscription.," p. 97.

[1] Rawlinson, vol. i., p. 317; vol. ii., p. 347 *et seq.*

SUPERHEATED STEAM.

I HAVE noticed a curious misapprehension, even on the part of high authorities, with respect to the application of Carnot's law to an engine in which the steam is superheated after leaving the boiler. Thus, in his generally excellent work on the steam-engine,[2] Prof. Cotterill, after explaining that in the ordinary engine the superior temperature is that of the boiler, and the inferior temperature that of the condenser, proceeds (p. 141): "When a superheater is used, the superior temperature will of course be that of the superheater, which will not then correspond to the boiler pressure."

This statement appears to me to involve two errors, one of great importance. When the question is raised, it must surely be evident that, in consideration of the high latent heat of water, by far the greater part of the heat is received at the temperature of the boiler, and not at that of the superheater, and that, of the relatively small part received in the latter stage, the effective temperature is not that of the superheater, but rather the mean between this temperature and that of the boiler. An estimate of the possible efficiency founded upon the temperature of the superheater is thus immensely too favourable. Superheating does not seem to meet with much favour in practice; and I suppose that the advantages which might attend its judicious use would be connected rather with the prevention of cylinder condensation than with an extension of the range of temperature contemplated in Carnot's rule.

If we wish effectively to raise the superior limit of temperature in a vapour-engine, we must make the boiler hotter. In a steam-engine this means pressures that would soon become excessive. The only escape lies in the substitution for water of another and less volatile fluid. But, of liquids capable of distillation without change, it is not easy to find one suitable for the purpose. There is, however, another direction in which we may look. The volatility of water may be restrained by the addition of saline matters, such as chloride of calcium or acetate of soda. In this way the boiling temperature may be raised without encountering excessive pressures, and the possible efficiency, according to Carnot, may be increased.

The complete elaboration of this method would involve the condensation of the steam at a high tempera-

[2] Second edition (Spon: London, 1890).

ture by reunion with the desiccating agent, and the communication of the heat evolved to pure water boiling at nearly the same temperature, but at a much higher pressure. But it is possible that, even without a duplication of this kind, advantage might arise from the use of a restraining agent. The steam, superheated in a regular manner, would be less liable to premature condensation in the cylinder, and the possibility of obtaining a good vacuum at a higher temperature than usual might be of service where the supply of water is short, or where it is desired to effect the condensation by air.

RAYLEIGH.

TWO AFRICAN EXPLORERS.

WE regret to have to record the death of two of the best-known African explorers—Colonel J. A. Grant and Dr. Wilhelm Junker; the latter comparatively a young man, and the former by no means old. They belonged to two distinct types of African explorers: Grant was the true pioneer, who went out to force his way through an unknown region; Junker was the scientific student, content to spend years in one limited region in order to work out its geography, natural history, and ethnology.

The announcement of the death of Colonel James Grant, Speke's companion in the expedition for the discovery of the source of the Nile, has been received with widespread regret. The stalwart figure and genial, good-natured, boyish face of Colonel Grant has been familiar in London society and in geographical and scientific circles for more than twenty years. In African exploration and in the younger generation of African explorers he took a keen interest to the last. He was a man of chivalrous loyalty to his friends. Speke's memory he almost worshipped, and it need scarcely be said that his feelings to Burton were of a very different stamp. Mr. Stanley had no more staunch supporter than Colonel Grant. Born in the parish manse of Nairn, in Scotland, in 1827, he was educated, like so many other Scots that have distinguished themselves in the service of the country, at the Grammar School of Aberdeen, and at Marischal College. At the age of nineteen he obtained a commission in the East India Company's service, and between that and the end of the Mutiny saw much active service, and won honours for bravery and devotion to duty. It is, however, as an African explorer that he claims notice in these pages. It will be remembered that in 1857 the only great lakes, of which we knew anything, in the map of Africa were Chad and Nyassa, the latter then quite recently plotted for certain by Livingstone. But rumours of other lakes had been filtering down to the coast for years. In 1857, Burton and Speke started from Zanzibar in search of "the Great Lake," as it was vaguely called; and, after a painful march of eight months, found Tanganyika, the first discovered of those great sheets of water which form so marked a feature of the centre of the continent. On the return journey, Speke took a run north, to find another great lake said to exist in that direction. He reached the southern shore of Lake Ukerewe, which has since become so well known as Victoria Nyanza. Though Speke only caught a glimpse of the southern waters of the lake, and had no adequate idea of its amplitude, he conjectured rightly that it must be the source of the Nile. Into the unhappy quarrel of Speke and Burton it is unnecessary to enter. But Speke, and not Burton, was selected by the Royal Geographical Society, in 1860, to lead an Expedition to the lake for the purpose of confirming his conjecture. This Expedition Government subsidized to the amount of £2500. Captain Grant, as he was then, was chosen to accompany Speke. The latter was, no doubt, the leader of the Expedition; but Grant, though he suffered much, and had to be carried a great part of the way, did much to render the expedition of scientific value. The unknown countries to the west and north-west of Victoria Nyanza were explored, though the contour of the lake was very inadequately laid down. Uganda was reached in 1862, and in July of that year the Nile was seen issuing full-born from the lake, and dancing its way north-west over Ripon falls. The two travellers followed the river for 120 miles, but were compelled to quit it, and so missed the discovery of its connection with the Albert Nyanza. They came upon it again 70 miles further down, and reached Gondokoro in February 1863, where they were met and succoured by Samuel Baker.

As might have been expected, the discoverers of the Nile sources received a great ovation on their arrival in England. Grant, like Speke, received the gold medal of the Royal Geographical Society, and was made a C.B. In 1864, under the title of "A Walk across Africa," he published a narrative of the expedition. In 1872 he published "A Summary of the Speke and Grant Expedition" in the Journal of the Royal Geographical Society. Colonel Grant was a careful observer, and his "Walk across Africa" abounds in interesting facts and suggestions on country and people, especially on the latter.

It was he who did the greater part of the scientific work on the Nile Expedition, and among other things he made a considerable collection of dried plants now in the Kew Herbarium. A rough list of these formed an appendix to Speke's "Journal of the Discovery of the Source of the Nile"; and most of the illustrations of this work were from drawings made by Grant. The publication of the first volume of Prof. D. Oliver's "Flora of Tropical Africa" fired Grant with the desire to have a special volume prepared on the flora and fauna of the Expedition. The result was that the whole of the twenty-ninth volume of the Transactions of the Linnean Society was devoted to the flora, and it is one of the most interesting of the series. The purely botanical part was contributed by Prof. Oliver and Mr. J. G. Baker; and the 136 plates (prepared at Colonel Grant's expense) illustrating the new or otherwise specially interesting plants, are some of the best work of the late W. H. Fitch.

In 1871 Grant was elected a Fellow of the Linnean Society, and in 1873 of the Royal Society; and many other distinctions were conferred upon him, including a Companionship of the Star of India for his military services in India and Abyssinia.

Dr. Johann Wilhelm Junker was a different type of explorer. He was born in Moscow, of German parents, on April 6, 1840. He spent his boyhood in Göttingen, attended the German Gymnasium at St. Petersburg, and studied medicine at Göttingen, Berlin, and Prague. After a visit to Iceland, Junker went to Tunis in 1874, and to Egypt in 1875. In that year he began those explorations which, with one or two interruptions, he carried on continuously for twelve years. He visited Lake Mareotis, the Natrön Lakes, and the Fayum. In 1876 he went from Suakin, through the Khor Baraka, to Kassala and Khartum; he explored the lower Sobat, and made successive journeys among the western tributaries of the Nile. In 1876 he proceeded as far westwards as Makaraka, and in 1877 crossed the Tangi River and visited the Wau, thus overlapping the route of Schweinfurth (1869–70). Indeed, Junker, during his many years' journeying, did a great deal to supplement the work of his distinguished predecessor. After a brief visit to Europe (1878), Junker was back in Africa in 1879, this time accompanied by a photographer (Bohndorff), who also lent a helping hand in preparing the numerous natural history collections. He accompanied several of the expeditions sent out by the Egyptian authorities from Khartum. But Junker often wandered almost alone, with very few native companions, and as a rule he was everywhere well received. To him we are indebted for much of our information on the Monbuttus and their country, the A-Sandeh and the various other tribes that inhabit the wide region watered by the Western Nile tributaries and some of the northern feeders of the Congo. Indeed, much of Junker's work lay in that extremely interesting country which forms the water-parting between the basin of the Nile and the Congo, and his hydrographical observations form some of the most important results of his many years' travels. To him may be said to be due the first steps in the solution of what was long known as the Wellé problem. The Wellé, which Schweinfurth came upon near its sources, was, even up to 1886, conjectured by many to be the upper course of the Sharé, which runs into Lake Chad. Junker struck the river at various places, one as far west as 22° 40′ E.; but it was not until Grenfell and Van Gèle followed the Mobangi up from the Congo that the Wellé was proved to be one of the chief northern feeders of the Congo. But this is only one of many services rendered by Junker to the scientific geography of Africa. His investigations into the ethnology of the whole of this intensely interesting region are of the first importance, and his collections both in ethnology and in natural history now form some of the most prominent exhibits in the great Museum of St. Petersburg. Junker was an admirable specimen of the scientific explorer, and his twelve years' researches in the Soudan entitle him to be classed with Schweinfurth and Nachtigal, Wallace and Bates. Junker was in the heart of the Soudan when the Mahdist revolt reached a crisis. He had the greatest difficulty in escaping, and it was only after long detention that he reached Europe *viâ* Lake Victoria, Zanzibar, and Egypt. He made many friends in London when he came here to receive the gold medal of the Royal Geographical Society, which he well deserved.

NOTES.

MRS. ADAMS wishes us to say that she would be very grateful if former friends and scientific correspondents of the late Prof. J. C. Adams would be so kind as to send to her care any of his letters still in their possession. Their doing so would much assist in the preparation of a memoir. All letters so intrusted will be carefully returned.

THE Electrical Committee of the Royal Commission for the Chicago Exhibition are anxious that the electrical department of the Exhibition should contain a good representation from this country. They have accordingly issued a circular letter on the subject, and it may be hoped that this will receive the careful attention of all who are in a position to facilitate the Committee's arrangements. The Committee especially desire that the Exhibition may display the very large share which English electricians have had in the development of electrical science and its practical applications. It is hoped that the fine collection of historical apparatus in the possession of the Post Office may be shown, and this will be supplemented by contributions which will be sought from many other sources. Although practical electric lighting has made greater progress in America than in this country, the Committee think there is much that England can show electricians on the other side of the Atlantic; and firms who devote special attention to the domestic uses of electricity and its artistic application are reminded that it may be to their interest to send to the Exhibition good specimens of their work. There will also be excellent opportunities for the manufacturers of electric railway signals, and for electro-metallurgists.

ON January 1, no fewer than 2082 applications for space at the Chicago Exhibition had been received from intending exhibitors in the United States alone. The number at the Philadelphia Centennial on the corresponding date was 864. Many applications have come from foreign countries, and it is expected that the exhibitors will be more numerous than at any previous "World's Fair." The allotment of space is to be made about June. The reception of exhibits will begin on November 1 next, and continue until April 10, 1893.

WE regret to have to record the death of Mr. H. W. Bates, F.R.S. He was sixty-seven years of age. Of his well-known book, "The Naturalist on the River Amazons," Darwin said that it was "the best work of natural history travels ever published in England." His "Contributions to an Insect Fauna of the Amazons Valley," printed in the Transactions of the Linnean Society, were described by the same high authority as "one of the most remarkable and admirable papers he had ever read in his life." Mr. Bates is widely known as the discoverer of the principle of mimicry in the animal world. For twenty-seven years he was the acting secretary of the Royal Geographical Society.

WE have received an intimation of the death of Dr. Pieter Willem Korthals, at the advanced age of eighty-four years. He was for many years in the Dutch East Indian Service, and published a considerable number of papers on the botany of Sumatra, Borneo, and Java, the most noteworthy of which form a folio volume entitled "Verhandelingen over de Natuurlijke Geschiedenis der Nederlandsche overzeesche Bezittingen," edited by C. J. Temminck, 1839–42. This work contains seventy coloured plates of excellent execution. Dr. Korthals's first botanical paper appeared in 1830, and dealt with the genus Nepenthes; and his last, so far as we are aware, appeared in 1854. He was a Knight of the Dutch Order of the Lion.

THE Council of the Royal Society of Edinburgh have awarded the Keith Prize for 1889–91 to Mr. R. T. Omond, for his contributions to meteorological science; and the Makdougall-Brisbane Prize for 1888–90 to Dr. Ludwig Becker, for his paper on "The Solar Spectrum at Medium and Low Altitudes."

THE Grand Gold Medal of the Paris Geographical Society has been awarded to M. Reclus. A gold medal has also been awarded to the Prince of Monaco for his researches on marine currents.

SIR JOHN COODE, Past-President of the Institution of Civil Engineers, was last Friday elected by the Committee of the Athenæum Club a member, under Rule II., which provides for the annual introduction of a certain number of persons of distinguished eminence in science, literature, or the arts, or for public services.

THE third Congress on anthropology in relation to crime will be held at Brussels from August 23 to September 3 next.

ON Monday Lord Cowper called attention in the House of Lords to the subject of technical instruction, and to "the difficulties in which County Councils were placed by not knowing whether or not they could rely upon a permanent annual Government grant for its promotion." Lord Cranbrook, in the course of his reply, said he did not believe any Government would repeal the Act in accordance with which the Councils had received the money which was being devoted to technical education. So far as the present Government were concerned, they had not the smallest intention of repealing the Act or of taking the money for any other purpose.

AT a Conference on technical education, held in Edinburgh on October 29, 1891, a general committee was appointed to consider the subject. This general committee in turn appointed a sub-committee to report on the amendments necessary in the laws relating to technical education in Scotland. The report of the sub-committee has now been issued; and appended to it is an official statement to the effect that the general committee, while approving of the suggestions made in the report, is of opinion that the Government ought to take an early opportunity of dealing with the question of technical and secondary education in a comprehensive measure, and that, for the efficient supervision of technical education, wider administration areas than the parish are required.

The ceremonies in connection with the tercentenary celebration of Trinity College, Dublin, will begin on the morning of July 5, and conclude on the evening of July 8 next. Invitations were sent to most of the Universities and learned Societies in Europe, America, and the colonies, during last term; and replies have been received from many sending representatives, including Aberdeen, Athens, Bern, Cambridge, Christiania, Edinburgh, Erlangen, Giessen, Heidelberg, Johns Hopkins, Lausanne, Leyden, Madras, Naples, Smithsonian Institute, Sydney, Toronto, &c., and acceptances are daily arriving. A large number of acceptances have also been received to the numerous personal invitations. Among the men of science who intend to be present are Sir F. Bramwell, Geo. Darwin, Thiselton Dyer, J. Evans, D. Ferrier, M. Foster, Sir A. Geikie, H. Hertz, J. W. Judd, Ray Lankester, Sir J. Lister, Sir J. Lubbock, Baron Nordenskiöld, Sir J. Paget, Lord Rayleigh, Sir G. Stokes, and Sir W. Turner. While the programme of the celebration has not yet been finally approved of, it will probably include the following:—On the Tuesday morning there will be a reception by the Provost of Trinity College of all the invited guests; then a short full choral service in St. Patrick's Cathedral; in the afternoon a garden party; and in the evening a civic ball. On Wednesday morning there will be the presentation of addresses of congratulation from the various Universities, and in the evening a grand banquet, at which it is expected that over five hundred guests or members of the University will be present. On Thursday morning there will be a special Commencements for the conferring of a number of honorary degrees; after which there will be an adjournment from the Senate House to the College Park to witness the College races, held under the auspices of the College Athletic Club; in the evening there will be a special amateur dramatic performance. On the Friday morning there will be a gathering of the students to hear short addresses from some of the distinguished visitors; in the afternoon, a concert given by the members of the University Choral Society; and in the evening, the students' ball. Numerous committees are daily engaged working out the multifarious details. One of these has been charged with the superintendence of the publication of an illustrated volume, which is to give the past and present history of the College, the publication of which volume has been undertaken by the firm of Messrs. Marcus Ward and Co.

On April 1, 1841, Sir William Hooker began his duties as Director of Kew. In the year 1891, therefore, the establishment might have celebrated its jubilee as a national institution, and it seemed to Mr. Thiselton Dyer that he might fitly mark the occasion by giving in the *Kew Bulletin* some account of the origin and development of the Royal Gardens as a place of botanical study. This intention he has now partly carried out, the December number of the *Bulletin*—which has just been issued—being entirely devoted to the subject. The narrative is one of great interest, and has evidently cost the author much hard work, as scarcely any authentic records exist of the period before 1840, when the Gardens were a purely private possession of the Crown. He has thus had "to fall back on local traditions, on local histories, the statements in which are often confusing and inaccurate, and on such scattered notices as could be gathered from contemporary literature." In the present instalment, the story is brought down to the year 1841. The history of the last half-century will be given in another number.

In pursuance of his botanical expedition to Persia, Herr J. Bornmüller arrived at Batoum on December 24. Thence he intended proceeding to Teheran by way of Tiflis and Baku, and then, as rapidly as possible, to the south of Persia. The expedition is intended to extend over two years, and Herr Bornmüller does not intend to collect more than from fifteen to twenty sets of the plants obtained. Orders for sets should be sent to Herr R. Huter, Sterzing, Tirol.

Mr. James Britten and Mr. G. S. Boulger intend to issue (to subscribers) in June next, their "Biographical Index of British and Irish Botanists," reprinted, with additions and corrections, from the *Journal of Botany*, and brought down to the end of 1891. As the promises of support at present received will not cover expenses, they will be glad to receive the names of additional subscribers, addressed to the care of the publishers of the *Journal of Botany*.

According to a telegram from New York, one of the finest displays of the aurora borealis ever known in that latitude was observed on the evening of February 13. The phenomenon stretched over a great belt of territory from Iowa to the Atlantic. A peculiar effect was produced on the telegraph system, and for intervals of three or four minutes at a time the wires were so surcharged with atmospheric electricity that between New York and Albany it was possible to send messages without the aid of the regular batteries. The current, however, was intermittent, and the effect unsatisfactory. For nearly two hours ordinary business could not be transacted with any degree of exactness. The aurora seemed to occupy the whole of the northern heavens, and was beautifully marked, the colouring being clear and distinct. People at first feared that a great fire was raging.

Dr. A. Woeikof, of St. Petersburg, who is engaged on an investigation into the cause of the famine in Russia, writes to the *Meteorologische Zeitschrift* that it is chiefly due to drought from August to October 1890, which injured the winter crops; to partial and insufficient snow, which melted early in the spring, and was followed by frost in April; and lastly to droughts and hot winds from May to July 1891. In the southern portion of the Government of Samara the prospects up to June 10 were excellent, but the harvest was destroyed by two days of hot winds, on June 14 and 15. And in the southern central provinces also, where the winter crops had greatly suffered, a moderate harvest was hoped for after the middle of July, but four hot days, from July 13 to 16, quite destroyed the crops.

The Journal of the Scottish Meteorological Society (third series, No. 8) contains a very interesting paper on silver thaw at Ben Nevis Observatory, by R. C. Mossman. The phenomenon is somewhat common at that Observatory, and occurs during an inversion of the ordinary temperature conditions, the temperature being considerably lower at the surface than at higher altitudes, causing the rain to congeal as it falls. In the six years 1885–90, 198 cases of silver thaw were observed, with a mean duration of $4\frac{1}{2}$ hours in each case, and they nearly all occurred between November and March, during times of perfectly developed cyclones and anticyclones. An examination of the weather charts of the Meteorological Office showed that for the 198 days on which the phenomenon was observed, the distribution of pressure was cyclonic on 137 days, and anticyclonic on 61 days. In anticyclonic conditions there was a cyclonic area central off the north-west coast of Norway, while the centre of the anticyclone was over the south of the British Isles. In cyclonic cases, an anticyclone lay to the south, over the Iberian Peninsula. The lowest temperature at which the phenomenon took place was 18°, and was rarely below 27°. Fully 90 per cent. of the cases occurred when the thermometer was between 28° and 31°·9, so that the greater number of cases occurred just before a thaw. The most common type of cloud which preceded both cyclonic and anti-cyclonic cases of silver thaw was cirro-cumulus, frequently accompanied by cirrus and cirro-stratus; and the changes showed that the higher strata of the atmosphere came first under the influence of the moist current, which took from three to eight hours to descend to the height at which cumulo-stratus forms. An examination of a series of storm charts prepared by Dr. Buchan disclosed the somewhat remarkable fact that 73 per cent. of the cyclonic, and 63 per cent. of the anticyclonic cases of silver thaw on Ben Nevis were followed or preceded by gales on our northern and north-western coasts; and it would appear from the wind conditions that the barometric gradient at the height of Ben Nevis (4407 feet) must be totally different from what obtained at sea-level during the occurrence of silver thaw on the hill-top. Many of our readers will remember the remarkable case of silver thaw which occurred in London last Christmas Day.

THERE has been much talk in Germany about Dr. Peters's discovery of saltpetre in the Kilima Njaro district. This discovery accords with statements which were already well known. Dr. Fischer, after an examination of the Donjongai volcano, reported that in the neighbourhood of the crater there were a series of curiously shaped veins of a white substance which he took to be either saltpetre or soda. In 1879, Herr Jarler asserted that large quantities of sulphur would probably be found in the crater. The Berlin correspondent of the *Times*, by whom these facts are noted, adds that not far from the volcano there lie great swamps from which soda is obtained. It is expected that an expedition for the exploration of the district will soon be sent out by the German East Africa Company.

PROF. A. GIARD calls the attention of naturalists to a new case of mimicry between two very different insects (the one Hymenopterous, the other Dipterous)—*Athalia annulata* and *Beris vallata*. When both insects are quiet, the resemblance of colour and patterns is perfect, and as the *Athalia*, like most Tenthredinæ, is protected against birds and other foes by its unpleasant smell, it is probable that the resemblance is of considerable service to *Beris*. M. Giard also refers to the larva of *Allantus tricinctus* which is commonly found on the leaves of plants, vividly coloured, and quite conspicuous, but resembling in form and colour birds' droppings, as is the case with a spider described by H. O. Forbes.

THE number of persons who approve of cremation seems to be steadily increasing. From the Report of the Cremation Society of England for 1891, we learn that in 1885, the first year the crematorium at Woking was used, only three bodies were sent there; in 1886 the number was 10; in 1887, 13; in 1888, 28; in 1889, 46; in 1890, 54; and during the past year, 99. Crematoria are being built in various parts of the country. At Manchester a crematorium is in course of erection, and will, it is thought, be completed and opened for use during the coming spring. A company has also been formed, and is making rapid progress, with the same object at Liverpool; and the City of London Commission of Sewers is taking steps to obtain powers to erect a crematorium at their cemetery at Ilford. The Cremation Society at Darlington, and other associations, are moving in the same direction.

A SMALL axe of nephrite, found at Ohlau, in Silesia, and now in the Roman-German Museum at Mainz, has lately been carefully examined by Dr. O. Schoetensack, with a view to the discovery of the source from which the material must have been obtained. From a thorough determination of its specific gravity, microscopic structure, and chemical composition, Dr. Schoetensack concludes that the nephrite is exactly the same as a mineral which has been found by Dr. Traube near Jordansmühl, in Silesia. There is no reason, therefore, why the axe should not be pronounced to be of Silesian origin. This is the only prehistoric object of nephrite, found in Europe, the source of which has been definitely decided.

PROBABLY few authorities responsible for the making and maintenance of roads are aware that one of the things against which they should be on their guard is the use of rotten flints for macadamising purposes. Mr. C. Carus-Wilson has been giving some attention to this subject at Bournemouth, and, in a letter to a local journal, states the conclusions at which he has arrived. He was led to consider the matter by the abnormal quantity of slush on the surface of Poole Road. This he attributes to the fact that the road has again and again been mended with rotten flints, by which he means flints that have become decomposed to such an extent that there is very little true flint left. These flints are surrounded by a thick zone of soft white material. This is rapidly removed from the flinty nucleus by the grinding and pressure to which it is subjected; while the true flinty nucleus, being thus denuded of its outer covering, becomes too small to bear much crushing weight, so that it quickly breaks up, and forms a fine flint sand. Mr. Carus-Wilson has found by repeated microscopic analysis that the Poole Road mud is formed principally of these two substances.

THE Journal of the Society of Arts prints this week an interesting lecture on burning oils for lighthouses and lightships, by E. Price Edwards. It was delivered at a meeting of the Society on February 10. After the lecture Sir Lyon Playfair, who was in the chair, said it was quite clear that mineral oils must in time beat the vegetable oils, on account of their chemical composition, the ingredients in the latter not being all combustible, but consisting of fatty acids and glycerine. Mineral oils, on the other hand, were nearly of the same composition as olefiant gas, the illuminating constituent of coal gas, with the addition of a little more hydrogen. They were therefore sure to win in the end; it was merely a question of manufacturing them safely.

AT the meeting of the Field Naturalists' Club of Victoria on December 14 last, Mr. A. J. Campbell read a paper on a nest and egg of a bird of paradise (*Ptiloris victoriæ*). Eastern Australia possesses a genus (three species) of these beautiful birds, but they are very seldom seen. The nest was found on November 19, by Mr. Dudley Le Souëf and Mr. H. Barnard during a visit to the North Barnard Islands, about forty miles off the coast from Cardwell, Queensland. They watched the hen bird for some time, and saw her fly into the crown of a Pandanus tree growing close to the open beach. Although they could not distinguish the nest itself, they could see the head of the bird as she sat on it. The nest was about 10 feet from the ground, and the bird sat quietly, although they were camped about 5 feet away from the tree. There was a single egg, the incubation of which was probably about seven days old. The nest was somewhat loosely constructed of broad dead leaves and green branchlets of climbing plants and fibrous material. Inside were two large concave-shaped dead leaves underneath pieces of dry tendrils which formed a springy lining for the egg or young to rest upon. The following is the measurement in centimetres: over all, 19 broad by 9 deep; egg cavity, 9 across the mouth by 4 deep.

DOVE's observation that when a tuning-fork of proper pitch is held to each ear beats can be heard, where there is no possibility of interference of the sound-waves in the air, is confirmed by Dr. W. Scripture (Wundt's *Philosophische Studien*), who further gives experimental ground for rejecting the hypothesis of transference of the sound through the bones of the head of the Eustachian tubes. Difference-tones, on the other hand, seem to be only perceptible when the tones of both forks affect the same ear. The same number of the *Studien* contains the first instalment of an elaborate article by Dr. J. Merkel upon the psychophysical error-methods.

If we may judge from the progress of the Photographic Society of India, photography is rapidly becoming more popular among Anglo-Indians. In January 1891, there were 277 members on the Society's books; now there are over 310.

The *Oesterreichische Botanische Zeitschrift* for February contains a very full account of the results of Porta and Rigo's visit to Spain in 1891, and of the species gathered by them on their journey.

We have received Parts 10 and 11 of the "Illustrations of the Flora of Japan," published at Tokyo. The drawings continue excellent; the diagnoses are unfortunately in Japanese.

Messrs. Dulau and Co. have issued a catalogue of botanical works which they offer for sale. It contains the titles of about 3000 writings relating to geographical botany.

The "Electrical Trades Directory," issued by Mr. George Tucker on behalf of the *Electrician*, has made its tenth annual appearance; and no effort has been spared to bring it up to date. The biographical division of the work contains sketches of the careers of 260 men who are well known in the electrical world. These sketches have all been revised by the subjects of them. No fewer than 28 of these notices are accompanied by portraits, among which is a portrait of Prof. Ayrton from a specially engraved steel plate.

The Royal University of Ireland has issued its Calender for the year 1892.

An experiment, illustrating the remarkable power possessed by palladium of occluding hydrogen, is described by Prof. Wilm, of St. Petersburg, in the current number of the *Berichte* of the German Chemical Society. The experiment is so simple, and requires so short a time to exhibit, that it would appear to be eminently suitable for lecture demonstration. The metallic palladium is employed in the finely divided state obtained by heating the easily prepared yellow crystals of the compound $PdCl_2 . 2NH_3$, first in the open air, and subsequently for a short time in an atmosphere of hydrogen. A small quantity, about four grams in weight, of the palladium so obtained is placed in a bulb blown at the bend of a U-shaped tube. The extremity of one limb of the U-tube is bent round at right angles, and connected with a wash-bottle containing sulphuric acid, which in its turn is connected with a Kipp's apparatus generating hydrogen from zinc and dilute sulphuric acid. The wash-bottle serves not only to dry the hydrogen, but also to indicate the speed of the current of gas. The extremity of the other limb of the U-tube is narrowed to a capillary, and terminates with a tightly-fitting stopcock and jet. In commencing the experiment, the hydrogen current is started, and then, first the metal, and afterwards the whole U-tube, is carefully heated with a Bunsen flame in order to remove the moisture formed by the action of the hydrogen, under the influence of the palladium, upon the oxygen of the air contained in the apparatus. When all the air and moisture are thus driven out of the apparatus, an attempt may be made to ignite the issuing hydrogen at the jet above the open stopcock. It will be found, however, that even while the metal is hot and the stream of hydrogen very rapid, a constantly burning flame cannot be maintained at the jet with the stopcock fully open; instead, a series of somewhat explosive ignitions and sudden extinctions occurs. It is only when the stopcock is turned so as to reduce the exit of the gas to a minimum that a constantly burning jet can be obtained, the hydrogen in contact with the palladium being then subjected to a certain amount of compression. The palladium is now heated a little more strongly, just above bright redness, when it is no longer capable of occluding hydrogen, and then the lamp is withdrawn, and after a few seconds the stopcock closed. The occlusion is then demonstrated in a most striking manner, for the stream of hydrogen continues to bubble through the sulphuric acid bottle and into the U-tube for several minutes with its original rapidity, although all exit is prevented by the closing of the stopcock. At length, however, the occlusion diminishes, and the stream of hydrogen gradually becomes slower and slower, until it entirely ceases, the palladium having regained the temperature of the room, and become saturated with hydrogen at this temperature. If now the stopcock is opened, and the metal again heated, upon applying a flame to the jet, the issuing hydrogen evolved from the palladium takes fire, and burns with a tall flame which remains constant for some minutes, then, as the hydrogen stored in the palladium becomes exhausted, diminishes in size, and finally disappears. The moment the flame is removed occlusion instantly commences again, and the experiment may be repeated any number of times with undiminished effect.

The additions to the Zoological Society's Gardens during the past week include a Herring Gull (*Larus argentatus*), six Common Gulls (*Larus canus*), five Black-headed Gulls (*Larus ridibundus*), British, presented by Mr. T. A. Cotton, F.Z.S.; a Roseate Cockatoo (*Cacatua roseicapilla*) from Australia, a Grey Parrot (*Psittacus erithacus*) from West Africa, presented by Mrs. Hennah; a Red-winged Parrakeet (*Aprosmictus erythropterus*) from Australia, presented by Lieut.-Colonel R. J. H. Parker, R.E.; a Cape Dove (*Œna capensis*) from South Africa, presented by the Rev. George Smith.

OUR ASTRONOMICAL COLUMN.

Photographic Magnitudes of Stars.—The Astronomer-Royal communicated a paper "On the Relation between Diameter of Image, Duration of Exposure, and Brightness of Objects in Photographs of Stars taken at the Royal Observatory, Greenwich," to the Royal Astronomical Society in January. First, with regard to the relation between the diameter of the image of a star on the photographic plate and the time of exposure. Measures of a large number of stars have led to the development of the following empirical formula for the same star with different exposures—

$$\sqrt{d} = 1{\cdot}03 \log t + \text{const.},$$

where d = the diameter of a stellar image in seconds of arc, and t the exposure in seconds of time, the magnitude, m, being expressed on Pogson's scale. This relation represents the observations through a range of eight magnitudes, with a mean apparent error of less than one-twentieth of a magnitude. By using the measures from which the above formula was deduced, and photometric determinations of magnitude made at Bonn and Oxford, data were obtained for determining whether there was a constant relation between duration of exposure and brightness of star photographed, whether, in fact, for equal diameters of images—

$$\text{Exposure} \times \text{brightness} = \text{const.}$$

The relation found from the comparison was

$$0{\cdot}4 \times m = 0{\cdot}97 \log t + \text{const.}$$

This agrees very well with

$$0{\cdot}4 \times m = \log t + \text{const.},$$

in which 0·4 is the logarithm of the number expressing the magnitude-ratio. By combining the two relations developed, it follows that, for the same exposure—

$$\text{Magnitude of star} = \text{const.} - 2{\cdot}43 \times \sqrt{\text{diameter}}.$$

And finally the three formulæ are expressed by the following:—

$$m = 2{\cdot}5(\log t - 0{\cdot}97\sqrt{d}) + \text{const.},$$

connecting magnitude, diameter of image, and time of exposure.

THE ZODIACAL LIGHT.—"The Zodiacal Light as related to the Terrestrial Temperature Variations" is the subject of a reprint from the *American Meteorological Journal* for November 1891, recently received from Mr. O. T. Sherman. In it the author endeavours to show that, when the temperature of the whole earth is considered, "the principal cause of variation is the difference in the sum of the local densities of the zodiacal light-forming matter which lies between us and the sun." Curves have been constructed to indicate five-yearly means of temperature variation from 1790 to 1884, five-yearly means for the zodiacal light, and the yearly auroral numbers for Europe south of the polar circle; but there are not sufficient data to permit the deduction of any very definite conclusion from them.

THE ANCIENT TOMBS AND BURIAL MOUNDS OF JAPAN.

AT a recent meeting of the China branch of the Royal Asiatic Society at Shanghai, Prof. Hitchcock, of the Smithsonian Institute, read a paper on the ancient tombs and burial mounds of Japan, in the course of which he said that, while the form and structure of the Japanese mounds were now known, thanks to the as yet unpublished researches of his companion in many journeys in Japan, Mr. W. Gowland, their early origin was yet to be traced. It was surmised that a few at least of the Japanese burial customs were derived from China. In the course of his own travels in the north of China he had failed to discover any indications of the existence of mounds like those in Japan; but he still expected to hear of them from some experienced traveller in the interior of that vast empire. Referring to the origin of the tombs, the lecturer said the first Emperor, who lived in the seventh century B.C., is supposed to be buried in Yamato, and the tombs of his successors are pointed out by the Imperial Household Department. The identity of the sepulchres may be questioned, but it is a fact that we can distinguish consecutive modifications of form apparently corresponding to successive periods of time.

Several distinct methods of interment have prevailed at different periods in Japan. They may be conveniently distinguished as follows: (1) burial in artificial rock caves; (2) in simple earth mounds, with or without coffins; (3) in rock chambers, or dolmens; (4) in double or Imperial mounds. The lecturer then proceeded to illustrate the appearance of these different kinds of mounds by the aid of photograph slides thrown on to a screen. He showed that the double mounds were invariably protected by a wide and deep moat, sometimes by two, and consisted of two distinct mounds with a depression between them. One of these double mounds, near Sakai, according to Japanese reckoning dates from about the fourth century. The height is about 100 feet, and the circuit of the base 1526 yards. The Emperor Kei Tai, who is reported to have lived in the sixth century, was one of the last emperors known to have been buried in a double mound. Some mounds have terraced sides, and this form is said to date from about the seventh century. Large quantities of clay cylinders were used for the purpose of preserving the terraces against the effects of the weather. When the covering of earth is removed, it is found that the stone chamber beneath, which contained the coffin, opens through passages often 40 feet and sometimes 60 feet long. The earth has in many cases been washed away from the mounds, exposing the rocks which are piled over the central chamber. According to a Japanese authority, in all the sepulchres the first order of performing the burials was the piling up of the earthen mound, leaving an underground tunnel leading from the outside to the very centre of the mound. This mound completed, the coffin, usually carved and made of stone, in which the corpse was placed, and sealed, was then introduced through the tunnel and placed in the centre of the mound, and the tunnel was then filled up with stones. The lecturer, however, said the coffins were not always introduced through the galleries, and the tunnels were certainly not filled up with stones, although their ends were probably closed with stones. He inferred from his own observations that the chambers were frequently, if not usually, built round the coffins. Stone and clay coffins had been found together in one cave, showing them to have been contemporaneous.

After showing a number of photographs of the pottery discovered in the mounds, he drew attention to a number of small clay figures representing human beings. He said it was a very ancient custom in Japan to bury the retainers of a prince standing upright around his grave. Like many other customs, this also came from China. In the time of the Japanese Emperor Suinin (97–30 B.C.), his younger brother died, and they buried all who had been in his immediate service around his tomb alive. "For many days they died not, but wept and cried aloud. At last they died. Dogs and crows assembled and ate them. The Emperor's compassion was aroused, and he desired to change the custom. When the Empress Hibatsuhime-no-Mikoto died, the Mikado inquired of his officers, saying: 'We know that the practice of following the dead is not good. What shall be done?' Nomi-no-Sukune then said: 'It is not good to bury living men standing, at the sepulchre of a prince, and this cannot be handed down to posterity.' He then proposed to make clay figures of men and horses, and to bury them as substitutes. The Mikado was well pleased with the plan, and ordered that henceforth the old custom should not be followed, but that clay images should be set round the sepulchre instead." Even as late as the year 646 an edict was published, forbidding the burial of living persons, and also the burial of "gold, silver brocade, diaper, or any kind of variegated thing." From this it might be inferred that the old custom of living burial was kept up, to some extent, even to the seventh century. The edict reads: "Let there be complete cessation of all such ancient practices as strangling oneself to follow the dead, or strangling others to make them follow the dead, or killing the dead man's horse, or burying treasures in the tomb for the dead man's sake, or cutting the hair, or stabbing the thigh, or wailing for the dead man's sake." The figures of clay thus introduced as substitutes for human sacrifices, and also to take the place of horses, are known as *tsuchi ningio*. Specimens of them are now very rare, and this fact leads to the supposition that the figures were not buried, but left exposed on the surface of the ground.

In the discussion which followed, Dr. Edkins pointed out the resemblance which existed between the stone relics found in Japan and China and in Europe, as indicating the existence of communication between distant lands in those days. It was also very interesting to note that, in the very earliest ages, men had been possessed with the idea of a future life for the soul.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—E. W. Hobson, M.A., Fellow and Lecturer of Christ's College, late Deputy Lowndean Professor, has been approved for the degree of Doctor in Science.

A decision on the subject of appointing lecturers in agricultural science will be taken by the Senate on February 25.

A meeting for the purpose of considering the propriety of erecting in Westminster Abbey a memorial to Prof. Adams has been summoned by the Master of St. John's, and will be held in the Combination room of that College on Saturday, February 20, at 3.30 p.m.

ST. ANDREWS.—At a meeting on Saturday, the 13th inst., of the Senatus Academicus of St. Andrews University, consisting of the Principals and Professors of the United College and St. Mary's College, St. Andrews, and University College, Dundee, it was unanimously resolved to confer the honorary degree of LL.D. upon Prof. Michael Foster, F.R.S., and Dr. Hugo Müller, F.R.S. The conferring of the degrees will take place in April.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, January 28.—"On certain Ternary Alloys. Part V. Determination of various Critical Curves, and their Tie-lines and Limiting Points." By C. R. Alder Wright, D.Sc., F.R.S., Lecturer on Chemistry and Physics in St. Mary's Hospital Medical School.

The author describes a number of "critical curves" obtained in accordance with the triangular system of representation proposed by Sir G. G. Stokes, whereby the composition of a given ternary mixture is represented by the position of the centre of gravity of three weights placed at the respective corners of an equilateral triangle, and respectively in the proportions of the three components of the mixture. With certain pairs of metals, *e.g.* lead and zinc, each will only dissolve the other to a limited extent when molten; so that a mixture of the two separates into two binary alloys, one containing chiefly lead with a little zinc, the other mainly zinc with a little lead, the exact proportion depending on the temperature of the mass. If a third metal, *e.g.* tin, be added to the mixture, such that this third, or "solvent," metal will mix in all proportions with either of the other "immiscible metals" separately, the ternary mixture produced either forms a "real" ternary alloy, not separating into two different mixtures on standing (molten), or else an "ideal" alloy incapable of existing, and immediately separating into two different ternary mixtures; *e.g.* one chiefly containing lead with some of the tin and a little zinc, the other mainly consisting of zinc with the rest of the tin and a little lead. The two points representing these two mixtures lie on opposite sides of the point indicating the total mass of the three metals originally used, this third point lying somewhere on the straight line connecting the other two "conjugate points," or "tie-line." By employing a series of mixtures containing gradually increasing proportions of the third "solvent" metal, two branches of a curve are gradually traced out representing the respective loci of the pairs of conjugate points; these two branches of the "critical curve" tend to meet at some point, termed the "limiting point," where the tie-lines vanish.

A large number of experiments were made to determine the conditions necessary to obtain the nearest possible approximations to the positions of truly conjugate pairs of points; former experiments having indicated that the compositions of the two alloys formed might be influenced by the relative proportions of the two immiscible metals, in such fashion that points slightly varying in position along one branch might be obtained as conjugates to a given point on the other branch under different conditions. It was found that *such variations entirely disappeared with thorough admixture,* which is more readily effected when the proportions used are such as to give rise to approximately equal quantities of the two alloys than when they are such that one alloy is formed to a much larger extent than the other. Observations with non-metallic analogous fluids (chloroform, water, glacial acetic acid) always gave sharply concordant values, thorough intermixture (by shaking vigorously in a stoppered bottle) being much more easy than with metals melted in a crucible and simply stirred vigorously.

Plotting the curves with the heavier immiscible fluid (lead, chloroform, bismuth) at the left hand corner of the base of the triangle, the lighter one (zinc, water) at the right hand corner, and the solvent fluid (tin, acetic acid) at the apex of the triangle, it was found that the tie-lines with chloroform-water-acetic acid, always sloped downwards to the left, the angle of slope continually increasing. The right hand branch of the critical curve rises to a maximum elevation, and then descends again to the limiting point, whilst the left hand branch continually ascends to that point. At the limiting point the chloroform and water are in the proportions $2CHCl_3$, $5H_2O$; the acetic acid present being less the higher the temperature, *i.e.* the critical curve for a higher temperature lying *inside* that for a lower one. At positions near the limiting point the mixture is extremely sensitive to temperature-variation; a few tenths of a degree will often make all the difference as to whether a single homogeneous fluid results (a "real" mixture), or two different ones, the point indicating the mixture in the first case lying just outside the critical curve, and in the second case inside it.

Analogous results were obtained with the metallic mixtures; with lead-zinc-tin mixtures, the lower ties slope to the left, the upper ones to the right, the limiting points also lying on the right and below the highest point of the critical curve; at this point the lead and zinc are nearly in the proportion $PbZn_6$. Two points, one in the left hand branch, the other in the right, are indicated by the configurations of the tie-lines, corresponding approximately with the definite atomic compounds $SnPb_3$ and $SnZn_4$ (when aluminium is used instead of zinc, similar configurations are developed, corresponding with $SnPb_3$ and $SnAl_4$). When silver is employed instead of tin, all the ties slope to the left; irregularities of outline (bulges) are noticed, caused by the formation of definite atomic compounds, $AgZn_5$ and Ag_4Zn_5: these bulges are still more pronounced when bismuth is used instead of lead (bismuth-zinc-silver curve). With both silver-lead-zinc and silver-bismuth-zinc curves, the limiting points lie to the *left* of and below the highest point of the critical curve; with both tin-lead-zinc and tin-bismuth-zinc curves, to the *right* and below the highest points.

In any given curve, substitution of bismuth for zinc *depresses* the curve, so that the curve with lead uniformly lies *outside* that with bismuth. The curve for a higher temperature always lies *inside* that for a lower temperature, the effect of temperature-variation being very much less marked at the lower parts of the curve, than at points lying anywhere near the limiting point.

February 4.—"On the Thermal Conductivities of Crystals and other Bad Conductors." By Charles H. Lees, M.Sc., the Owens College, Manchester.

The author commences by referring to Kundt's discovery, that the metals stand in the same order as conductors and as to the velocity of propagation of light through them, and mentions that his experiments were originally intended to furnish data for a similar comparison for crystals, but that their object has been extended.

After some preliminary experiments, he adopted the "divided bar" method, which consists in placing a disk of the material the conductivity of which is required, between the ends of two bars of metal placed coaxially, heating one end of the combination, and observing, by means of thermo-junctions applied to the bars, the distribution of temperature along them—first, with the disk in position; second, with the bars in contact without the disk. When the conductivity of the bar is known, these observations suffice to determine that of the disk.

The ends of the bars which came in contact with the disks were amalgamated, as this was found to be the best method of securing good contacts These bars were suspended horizontally in a frame, and, by means of screws, set accurately in the required position.

The conductivity of the bar was determined by the method due to Forbes of determining the loss of heat from the surface by allowing the bar to cool and observing the rate of change of temperature, and then observing the steady distribution of temperature along the bar when heated at one end.

The author finds it to be 0·27 C.G.S. unit, and to increase slightly with temperature.

The disks used were of the same diameter as the bar, and were of various thicknesses, in order to make the distribution of temperature throughout the bars nearly the same in each case.

The following are the results obtained. No relation of the kind found by Kundt for metals seems to hold for the crystals experimented on:—

	C.G.S. units.
Crown glass	0·0024
Flint glass	0·0020
Rock salt	0·014
Quartz along axis	0·030
,, perpendicular to axis	0·016
Iceland spar along axis	0·010
,, ,, perpendicular to axis ...	0·0084
Mica perpendicular to cleavage	0·0016
White marble	0·0071
Slate	0·0047
Shellac	0·00060
Paraffin	0·00061
Pure rubber	0·00038
Sulphur	0·00045
Ebonite	0·00040
Gutta-percha	0·00046
Paper	0·00031
Asbestos paper	0·00057
Mahogany	0·00047
Walnut	0·00036
Cork	0·00013
Silk	0·0002
Cotton	0·0006
Flannel	0·0002

Chemical Society, January 21.—Prof. W. A. Tilden F.R.S., Vice-President, in the chair.—The following papers were read:—The estimation of oxygen dissolved in water, by M. A Adams. The author describes an apparatus in which the estimation of dissolved oxygen in water by Schützenberger's method may be carried on so as entirely to avoid loss of oxygen by diffusion. The results obtained by this method are liable to differ according to the rate at which the determinations are effected, higher results being obtained when the operations are quickly performed.—The luminosity of coal-gas flames, by V. B. Lewes. The author has quantitatively studied the actions which occur in luminous gas flames, and also those which lead to loss of luminosity in the flame of a Bunsen burner. He considers that the most accurate method of dividing a luminous hydrocarbon flame into zones, is to regard it as made up of three:—(1) The inner zone, in which the temperature rises from a comparatively low point at the mouth of the burner to about 1000° C. at the apex of the zone; in this portion of the flame various decompositions and interactions occur, which culminate in the conversion of the heavier hydrocarbons into acetylene, carbon monoxide being also produced. (2) The luminous zone, in which the temperature varies from 1000° to a little over 1300°. The acetylene is here decomposed with liberation of carbon, which imparts luminosity to the flame. (3) The extreme outer zone, in which the combustion is practically complete. The various actions which tend to cause the loss of luminosity in a Bunsen flame may be summarized as follows:—(1) The chemical action of the atmospheric oxygen, which causes loss of luminosity by burning up the hydrocarbons before they, in their diluted condition, can afford acetylene. (2) The diluting action of the atmospheric nitrogen, which, by increasing the temperature necessary to bring about the partial decomposition of the hydrocarbons present, prevents the formation of acetylene, and so causes non-luminosity. (3) The cooling influence of the air introduced, which is able to add to the general result, although the cooling is less than the increase in temperature brought about by the oxidation due to the oxygen in the air. (4) In a normal Bunsen flame the nitrogen and oxygen are of about equal importance in bringing about non-luminosity, but if the quantity of air be increased, oxidation becomes the principal factor, and the nitrogen practically ceases to exert any influence.—The origin of flame coloration, by A. Smithells. This paper was reported in NATURE of January 28, (p. 306).—Note on the action of dilute nitric acid on coal, by R. J. Friswell. If bituminous coal be treated with 49 per cent. nitric acid, a black substance is obtained which is almost completely soluble in sodium carbonate solution and behaves as a nitro-acid; on treating this substance further with nitric acid, a brown acid is obtained which has not yet been examined.—A pure fermentation of mannitol and dulcitol, by P. F. Frankland and W. Frew. The authors have obtained an organism which sets up a fermentative decomposition, not only of mannitol, but also of dulcitol, a substance which has hitherto resisted fermenting bacteria. The products of the activity of this organism are essentially the same in the case of both sugars, consisting of ethyl alcohol, acetic acid, succinic acid, carbon dioxide, hydrogen, and varying quantities of formic acid. The decomposition may be represented by two sets of changes, viz. (*a*) $C_6H_{14}O_6 = 2C_2H_6O + CO_2 + CH_2O_2$, and (*b*) $C_6H_{14}O_6 = C_4H_6O_4 + C_2H_4O_2 + 2H_2$. It appears that two molecules of the sugar are resolved in accordance with equation (*a*) for every one decomposed according to (*b*). The organism is termed *Bacillus ethaceto-succinicus*.—Synthesis of hexahydroterephthalic acid, by J. E. Mackenzie and W. H. Perkin, Jun. The sparingly soluble hexahydroterephthalic acid of Von Baeyer is obtained by eliminating two molecules of carbon dioxide from hexamethylene-tetracarboxylic acid.—The magnetic rotation of dissolved salts, by W. Ostwald. The author considers that the values obtained by Perkin for the magnetic rotatory power of the ammonium salts of fatty acids in solution are in accord with the electrolytic dissociation theory. This theory does not lead to any numerical value or sign of the difference between the observed and calculated values of the magnetic rotation of electrolytes. In the case of haloid acids and salts the variation is positive; in the case of the oxy-acids negative. It is, therefore, not surprising that molecules with nearly zero variations are capable of existence.—The dissociation of nitrogen peroxide, by W. Ostwald. Using the data given by Cundall, the author has compared the extent to which nitrogen peroxide undergoes dissociation when vaporized, with that which it suffers when dissolved in chloroform. From the results he concludes that it behaves in accordance with Van 't Hoff's generalization that dissolved substances obey the same laws as gases. It appears, however, that in the gaseous state dissociation is far more advanced than in a chloroform solution.—Corydaline, by J. J. Dobbie and A. Lauder. The authors have obtained corydaline in colourless crystals melting at 134° 5. The results obtained on analysis do not agree with Wicke's formula, $C_{18}H_{19}NO_4$, but rather with $C_{22}H_{28}NO_4$. Analyses of the hydriodide, platinochloride, and methiodide of the alkaloid also give numbers agreeing with this formula.—Silver compounds of thiourea, by J. E. Reynolds. The author has obained a series of crystalline compounds of thiourea with silver nitrate, bromide, chloride, iodide, and cyanide. These substances contain one, two, or three molecules of thiourea to one of the silver salt, and readily afford silver sulphide when heated a few degrees above their melting-points.

Zoological Society, February 2.—Mr. W. T. Blanford, F.R.S., in the chair.—The Secretary read a report on the additions that had been made to the Society's Menagerie during the month of January 1892.—Mr. W. Bateson, F.R.S., exhibited some Crabs' claws bearing supernumerary prongs. It was shown that these extra parts are really complementary (right and left) pairs of indices or pollices, according to their position of origin, and not repetitions of the two pincers of the claw, as was commonly believed.—Mr. Sclater made some remarks on the breeding of the Ground-Pigeons (*Geophapes*) in the Society's Gardens, and showed that the young of these Pigeons, when first hatched, were not materially different in point of development from those of the typical Pigeons, and that there was consequently no ground for separating the *Geophapes* from the order Columbæ on this accouut, as it had been recently proposed to do.—A letter was read from Prof. R. Ramsay Wright, inclosing some photographs of the heaps of skulls of the American Bison which are collected on the plains of the Saskatchewan, and piled up at the sidings on the Canadian Pacific Railway, awaiting transport, and which testify to the enormous number of these animals recently exterminated.—Mr. W. Bateson gave a summary of his recent observations on numerical variation in teeth. The facts given related chiefly to specimens of Quadrumana, Carnivora, Marsupials, and other orders of Mammals in the British and other Museums. The author pointed out that the ordinarily received view of homologies between teeth is based on the hypothesis that the series is composed of members each of which is either present or absent. In the light of the facts of variation, this hypothesis was shown to be untenable, and an attempt was made to arrive at a more just conception of the nature of the homology of multiple parts.—Mr. R. Lydekker described part of an upper jaw of a Sirenian Mammal from the Tertiaries of Northern Italy, containing milk-teeth. As these teeth showed a masked Selenodont structure, it was urged that the specimen indicated the descent of the Sirenia from Selenodont Artiodactyle Ungulates. It was incidentally shown that *Halitherium veronense*, Zigno, from the same deposits, belongs to *Prorastomus*, Owen.—A communication was read from the Rev. H. S. Gorham, containing descriptions of and notes on the Coleoptera collected by Mr. John Whitehead on Kina Balu, Borneo. The present communication related to the families Hispidæ, Erotylidæ, Endomychidæ, Lycidæ, Lampyridæ, and others.—Another communication from the Rev. H. S. Gorham and Mr. C. J. Gahan gave an account of some of the Coleoptera collected by Mr. W. Bonny in the Aruwimi Valley, Central Africa.—Mr. P. L. Sclater, F.R.S., read some notes on a small collection of Mammals brought by Mr. Alfred Sharpe from Nyassaland, amongst which was a flat skin of Angas's Bush-bok (*Tragelaphus angasi*), a species of Antelope not hitherto recorded to occur in this district.—Mr. Sclater also gave the description of a new Antelope from Somali-land, proposed to be called *Bubalis swaynii*, after Captain H. G. C. Swayne, R.E., who had furnished him with the specimens on which it was based. He likewise exhibited and remarked on some other examples of Antelopes from the same country contained in Captain Swayne's collection.

Linnean Society, February 4.—Prof. Stewart, President, in the chair.—Mr. J. E. Harting exhibited Gould's coloured plate of a humming-bird (*Phæthornis longuemareus*), of which species a pair had made their nest in the drawing-room of Mr. Hamilton, of Queen's Park, Trinidad. The nest was built in a palm about five feet high, standing in a tub within the room. The first egg was laid on December 27 last, the second egg on December 29, and a young bird was hatched on January 12. The circumstance was regarded as quite unprecedented, though Mr. D. Morris was able to quote a case which came under his own observation in Jamaica, wherein a humming-bird had built its nest on the extremity of a saddle-bar in a verandah.—Mr. Harting also exhibited some life-sized photographs of the egg-cases of two species of dog-fish (*Scyllium*), and made some remarks on the mode of deposition and period of incubation as observed in different aquaria.—Mr. F. N. Williams read a paper on the genus *Dianthus*. He pointed out that *Velezia*, *Dianthus*, and *Funica* form a natural group of genera distinguishable from the Silene group by their seeds, which have a facial hilum and straight embryo. *Velezia* may be distinguished from *Dianthus* and *Funica* by having half the number of stamens. There are, however, three characters to be relied on in distinguishing these two genera; (1) the presence of a spicalyx of bracts; (2) the number of nerves to the calyx; (3) the junction of the claw with the blade of the petal. This last character was regarded as distinguishing very clearly *Dianthus* from *Funica*. In *Dianthus* the blade of the petal is abruptly narrowed into the claw, so that the two are distinct; in *Funica* the transition is gradual. Mr. Williams was of opinion that the species of *Dianthus* might be arranged in three natural groups (sub-genera): (1) in which the flowers are numerous and clustered, as in "Sweet William"; (2) the largest group, in which the flowers are few and usually solitary on the branches of the stems, as in carnations; and (3) a small group intermediate between *Funica* and the true pinks, and corresponding with the genus *Kohlrauschia* of Kunth. The number of species recognized by Mr. Williams in this monograph amount in round numbers to 250.—A paper by Messrs. G. J. Hinde and W. M. Holmes was then read, on the Sponge remains in the Lower Tertiary strata near Oamaru, Otago, New Zealand. Near Oamaru there are beds of white, friable siliceous rock of Upper Eocene age, almost entirely composed of Sponge spicules, Diatoms, and Radiolaria, thus resembling in character the Diatom and Radiolarian ooze of the present deep seas. The Sponge remains are all detached; they belong largely to the *Monactinellidæ*, though *Tetractinellid*, *Lithistid*, and *Hexactinellid* spicules are also present. The smaller flesh spicules of these different groups are perfectly preserved, and thus enable a comparison to be made with existing Sponges, to which generically they mostly belong. In all 43 genera and 113 species have been recognized by their characteristic spicules. Many of the forms have not hitherto been known as fossil. The existing relatives of many of them now inhabit the Indian and Southern Oceans, but some are at present only known from the North Atlantic. The remains of deep-water Sponges are intermingled in the deposit with others hitherto supposed to belong to moderate depths only, but in recent dredgings by H.M.S. *Egeria* off the south-west coast of Australia, at a depth of 3000 fathoms, there is a corresponding admixture of similar spicules.

PARIS.

Academy of Sciences, February 8.—M. d'Abbadie in the chair.—Observations on a note by M. H. Le Chatelier, on the optical measurement of high temperatures, by M. Henri Becquerel.—Silica in plants, by MM. Berthelot and G. André. An examination into the occurrence, distribution, and state, of silica in the spring-wheat plant in various stages of its growth, and into the quantity and state of the silica in the soil in which the plants were grown.—A new chart of the currents of the North Atlantic, by Prince Albert I. of Monaco.—Determination of the freezing-point of very dilute solutions: application to cane-sugar, by M. Raoult. The author describes a new method of taking cryoscopic observations, giving readings to within 1/500 of a degree. The molecular lowering of the freezing-point of water by cane-sugar is represented by a curve, which demonstrates that the author was correct in asserting that sugar gave, like other bodies, a gradual increase in its molecular lowering beyond a certain stage of dilution.—New measurement of the Perpignan base, by General Derrécagaix. The results are as follows: (1) the measured line, reduced to Delambre's base, gives for the length of this base 11,706·69 m.; (2) this length is verified by the geodesic accordance between the segments and the entire base, to an approximation of 1/250,000. Hence (1) the modern measurement of the Perpignan base makes it longer by 0·29 m. than found by Delambre using Borda's measures; (2) this new length is less than that calculated from the Paris base by only 5 cm.—New researches on the solar atmosphere, by M. H. Deslandres.—A new interpretation of Abel's theorem, by M. Sophus Lie.—On the integrals of equations of the first order which admit of only a finite number of values, by M. Paul Painlevé.—On a new process for the transmission of electric undulations along metallic wires, and a new arrangement of the receiver, by M R. Blondlot.—Refraction of liquefied gases, by M. James Chappuis. The indices of refraction at zero and under their maximum vapour pressure have been found for sulphurous acid and methyl chloride. For D they are respectively 1·3518 and 1·3533.—The rotatory power of quartz for the infra-red rays, by M. E. Carvallo.—The action of chlorine upon ruthenium: ruthenium sesquichloride and oxychloride, by M. A. Joly.—On a nitro-silicate of silver, and on the existence of a nitro-silicic acid, by MM. G. Rousseau and G. Tite.—On the decomposition of sulphurous acid by carbon at very high temperatures, by M. Scheurer-Kestner. Sulphurous acid passed over charcoal at a white heat is decomposed in accordance with the equation—

$$2SO_2 + 3C = 2CO + CO_2 + 2S.$$

—Chlorosulphide and bromosulphide of lead, by M. F. Parmentier. The chlorosulphide, obtained by a method described, is found to be $PbS.PbCl_2$. Its colour, when suspended in water, is cinnabar-red; when collected, it appears darker. The properties of the bromosulphide, $PbS.PbBr_2$, are similar, but it is more stable than the chlorosulphide. The existence of an iodosulphide is indicated.—Researches on sodium isopropylate, by M. de Forcrand.—On a nitro-derivative of antipyrin, by M. Edm. Jandrier.—On the rotatory power of diacetyltartric derivatives; reply to a note by M. Colson, by M. J. A. le Bel.—On the minimum perceptible amount of some odours, by M. Jaques Passy.—The law of absorption of carbon monoxide by the blood of a living mammal, by M. N. Gréhant.—On the fauna of the fresh waters of Iceland, by MM. Jules de Guerne and Jules Richard.—On the structure of the ovule and the development of the embryonic sac of *Dompte-venin* (*Vincetoxicum*), by M. Gustave Chauveaud.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

BOOKS.—Stanford's Handy Atlas of Modern Geography (Stanford).—The London Matriculation Directory, No. xi. January 1892 (Clive).—Synopsis of Non-metallic Chemistry: W. Briggs (Clive).—Pocket-book of Electrical Rules and Tables, 8th edition: Munro and Jamieson (Griffin)—Philosophical Notes on Botanical Subjects: Dr. E. Bonavia (Eyre and Spottiswoode).—The First Book of Euclid's Elements: Rev. J. B. Lock (Macmillan).—Hourly Means, 1888 (Eyre and Spottiswoode).—Recollections of a Happy Life, being the Autobiography of Marianne North, 2 vols.: edited by Mrs. J. A. Symonds (Macmillan).—Observations made at the Magnetical and Meteorological Observatory at Batavia, vol. xiii., 1890 (Batavia).

PAMPHLETS.—Aboriginal Skin-dressing: O. T. Mason (Washington).—Ten Years' Sunshine in the British Isles, 1881–90 (Eyre and Spottiswoode).—Harmonic Analysis of Hourly Observations of Air Temperature and Pressure at British Observatories (Eyre and Spottiswoode).

SERIALS.—Proceedings of the Royal Society of Edinburgh, vol. xviii., pp. 261–374 (Edinburgh).—Transactions of the Burton-on-Trent Natural History and Archæological Society, vol. ii. (Bemrose).—The Transactions of the Yorkshire Naturalists' Union, Parts 10 to 16 (Leeds, Taylor).—The Engineering Magazine, February (New York).—The Geological Magazine, February (K. Paul). Quarterly Journal of Microscopical Science, new series, No. 130 (Churchill)

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