THURSDAY, MARCH 19, 1874

THE CHEMICAL SOCIETY'S JOURNAL

THE man who jokingly said that he had to give up the study of chemistry when the science became so bulky that its Handbook required a wheelbarrow for its conveyance, expressed a truth which has been painfully felt by many scientific workers. With continual fresh additions to our knowledge, anything like a comprehensive grasp of a large science must become daily more and more difficult; but while this difficulty is generally felt, it occurs with special force in the science of chemistry. Chemistry, of all sciences, has perhaps the most unlimited capacity for development. Its subject is enormous, including the whole of nature, animate as well as inanimate. Nor is the chemist satisfied with studying the properties of matter as they are exhibited in the natural operations of the world around us, even this wide and attractive field of observation does not content him; he has made the grand discovery that the elements are his servants; that he can at will take to pieces in his laboratory the compounds found in nature, and construct therefrom a multitude of new bodies. Chemistry may thus be said to produce the matter upon which it feeds; the extent to which the production of new compounds can be carried seems practically unlimited, and these become, in most cases, the starting points of fresh investigations. We have here the principal cause of the wonderful development of modern chemistry; armed with such power, it cannot but abound in valuable discoveries, and furnish, at all times, copious results. As a consequence of this rapid development of the science, it has become a matter of the greatest difficulty for the investigator, the teacher, or the manufacturer, to keep pace with the daily progress of discovery; and improvement, and ignorance of the results already obtained in any department, naturally necessitates a loss of valuable time and labour to those engaged on the subject. The bulk and variety of chemical literature are not, however, the only obstacles to the student; the difficulty is greatly increased to an Englishman by the fact that the greater part of this literature is published on the Continent, and appears in a variety of languages with which the average Englishman has but little acquaintance.

With such difficulties to encounter, the individual student has certainly little prospect of successfully keeping abreast with modern chemistry. We are therefore exceedingly glad to find that the matter has been taken up by the Chemical Society of London, and that they now publish in their monthly journal * carefully prepared abstracts of all the original papers which appear in foreign and English periodicals. The abstracts are classified for facility of reference, and are divided into Physical, Inorganic, Mineralogical, Organic, Physiological, Agricultural, Analytical, and Technical Chemistry; it is, therefore, quite easy to ascertain what has been recently done in any department of the science. When we mention that the volume for last year consists of 1,300 pages, and contains, besides the papers and lectures read before the Society, about 1,500 abstracts of chemical papers published in other journals, we shall give some idea of the magnitude of the work which the Society has undertaken.

Looking carefully through the journal we find that nearly 40 periodicals are regularly abstracted; and as many of these periodicals reprint papers from other less known publications, the extent of literature brought under contribution is very considerable. The periodicals abstracted are German, French, Italian, American, and English, the first two preponderating. The preparation of the abstracts is of course laborious, and demands considerable care. It is accomplished by a body of twenty-six abstractors, chiefly Fellows of the Society, whose initials are appended to their respective work. We are bound to say that the abstracting so far as we have had an opportunity of judging, is exceedingly well done.

A work of this kind is far too expensive to be permanently carried on by a Society destitute of endowment, unless the scientific public in our own and other countries cordially support the enterprise. We understand that the sale of the journal outside the circle of the Society is at present very small, and that the expenses of publication are largely borne by a guaranteed fund raised to give the journal a fair start, and also by a grant from the British Association. We feel sure that the enterprise needs only to be widely known to obtain the support of all lovers of Science. What the Chemical Society is now doing is indeed exactly what we most need in the present day to assist the multitude of workers who are employing scientific facts and methods. It is a kind of work which must sooner or later be carried further, and extended to all the principal sciences, if ourselves and successors are to cope with the ever-increasing accumulation of facts. While such abstracts are, from their early intelligence and their widely gathered and condensed information, an unspeakable boon even to the independent and educated philosopher, they are of still greater value to the ordinary worker, who has not the advantages of a large and costly library, or of an education embracing many languages; to him these abstracts, obtainable at moderate cost in his own language, supply as far as possible the absence of fuller means of information. The work which the Chemical Society has taken up receives, therefore, on many grounds our warmest sympathy. It would indeed be a disgrace to the intellect of our country if such a genuine effort were allowed to drop for lack of support. We would especially invite the attention of our American readers to this monthly journal; supplying, as it does, in their own language a summary of the chemical literature of Europe, we should think it would exactly meet their wants. The Germans have long had a yearly volume of abstracts treating of chemistry and its allied sciences; up to the commencement of the present publication the German *Jahresbericht für Chemie* was indeed the only available work giving a summary of recent investigations. This annual periodical has lately fallen so behind in date (the volumes for 1870 were only obtainable in the middle of last year), that it has really become a chronicle of the past, rather than of the present state of science, and can hardly compare with the new English work. The subscribers to the "Journal of the Chemical Society" possess indeed at the

* "The Journal of the Chemical Society," containing the papers read before the Society, and Abstracts of Chemical Papers published in other Journals. Edited by H. Watts, F.R.S. (J. Van Voorst, 1873.)

present time abstracts of about 4,000 papers, all of later date than those noticed in the last German *Jahresbericht*. Our German fellow-workers may therefore, with advantage to themselves, give their support to this English work.

We trust that the appreciation of all interested in chemical science for this most useful work will be so decidedly shown that the Chemical Society will soon have no further anxiety as to the success of their undertaking. The circle of readers appealed to is a very wide one; not only is it an absolute necessity for those who work at Science and those who profess it, but the medical man, the agriculturist, the manufacturer, and the geologist will all find an abundance of matter interesting to their special pursuits.

TODHUNTER'S "MATHEMATICAL THEORIES OF ATTRACTION"

A History of the Mathematical Theories of Attraction and the Figure of the Earth from the time of Newton to that of Laplace. By I. Todhunter, M.A., F.R.S. Two vols. (London: Macmillan, 1874.)

I.

THE late Prof. de Morgan, in his "References for the History of the Mathematical Sciences," divides the written histories into two classes, those which are written on the plan of Montucla, Bossut, &c., in which a general account is framed out of the writer's notes or remembrances of miscellaneous reading; or in that of Delambre, Woodhouse, &c., in which the successive writings of eminent men are examined and described one after the other, so that each chapter or section is a description of the progress of Science in the hands of some one person, and is complete in itself. This latter plan is the one he considers the most favourable to accuracy and the most interesting to students who are desirous of being the critics of the historians, and of amending their works, if need be. The admirable two volumes before us would certainly be placed under this head. As to the utility of such works, our author remarks: "A familiarity with what has been already accomplished or attempted in any subject is conducive to a wise economy of labour; for it may often prevent a writer from investigating afresh what has been already settled; or it may warn him, by the failure of his predecessors, that he should not too lightly undertake a labour of well-recognised difficulty." Mr. Todhunter is no novice in this style of writing; his "History of the Calculus of Variations" appeared in 1861, and at once placed him in the foremost rank of mathematical historians; this work was followed, in 1865, by the "History of the Theory of Probability." The principles upon which these earlier works were written have been adopted in the work under consideration. Experience has improved his already first-rate powers of analysis and of graphic representation of the contents of the works he considers; all that he wants is leisure; possibly a time may come when the University of Cambridge will appoint an historian (or historians) to fill up the painfully patent void which now exists in this department of literature. The acknowledged high merits of his published histories would suggest Mr. Todhunter as a most fitting first occupant of such a chair; the liberality of the syndics of the University Press in defraying the expenses of the printing of this last work affords evidence that the work is appreciated. In his recent volume of "Essays" (p. 151), our author mentions his taste for the history of Mathematics; we heartily hope that the union of such taste and mathematical powers will result in the begetting a numerous progeny all equally comely with, and of as good disposition as, the elder members of the family.

There is one feature in these histories that especially commends them to our own mind, and that is the writer's *candour*. We cannot better express our own views upon this point than by citing the following passage from the late Sydney Smith's writings: "There is nothing more beautiful in science than to hear any man candidly owning his ignorance. It is *so* little the habit of men who cultivate knowledge to do so—they so often have recourse to subterfuge, nonsense, or hypothesis, rather than to a plain manly declaration, either that they themselves do not understand the subject, or that the subject is not understood—that it is really quite refreshing to witness such instances of philosophical candour, and it creates an *immediate* prepossession in favour of the person in whom it is observed." * It is the absence of this candour which has been productive of so much confusion in this subject of mathematical history: the straining after completeness leads to the insertion of second- and third-hand descriptions; the right rule seems to be that of De Morgan and our author, "to give no opinion or account of any book whatever unless such as is derived from personal acquaintance with its contents." Extreme care and painstakingness are manifest throughout without any sign of flagging. Interesting as Mr. Todhunter's histories are, even to the general student, from the many "sidelights' they contain, and which are especially numerous in the present work, they are exceedingly valuable to the special student, on account of the investigations with which they abound. These are not mere reproductions, but they translate, as it were, the old and now almost obsolete language of the earlier writers into the language of modern analysis: thus in § 443 it is remarked of D'Alembert's notation, "It is not very inviting, and he leaves it to explain itself." Some idea of the extent of these investigations may be got from the fact that 475 out of the 1,632 articles are devoted to them.

The author's design is to write the history of the Mathematical Theories of Attraction and of the Figure of the Earth; for this purpose, he says, he has endeavoured to include all the memoirs and works which relate to these subjects. Such has been his diligence in his seven years' research, that we should suppose few books have escaped his notice: certainly none that would materially affect the conclusions he has arrived at. That he would have added a few to his list had he consulted the British Museum library, or had access to that bequeathed by the late Mr. Graves † to University College, we shall probably show in the course of this notice.

Mr. Todhunter shows that the subjects treated of are of no common importance and influence. Researches into both theories have been fertile in yielding new resources for mathematicians: it will suffice to instance

* "Conduct of the Understanding."

† We are informed that the liberality of a gentleman who has already been a great benefactor to the College will shortly enable students to get an accurate idea of the treasures contained in the above library.

the Transformation of Multiple Integrals, the theory of the Potential, and the functions of Laplace. A knowledge of the figure and dimensions of the earth forms the basis of all the numerical results of Astronomy. In § 25 he carefully defines the several terms made use of in the two subjects, equating in a useful way the varying terms employed by different writers.

The foundation of our subject, as all our readers know, is "great Newton's own ethereal self," Newton, "the crown and glory of his race." "The propositions on Attraction are numerous, exact, and beautiful; they reveal his ample mathematical power. The treatment of the figure of the earth is, however, still more striking, inasmuch as the successful solution of a difficult problem in natural philosophy is much rarer than profound researches in abstract mathematics. Newton's solution was not perfect; but it was a bold outline, in the main correct, which succeeding investigators have filled up but have not cancelled. Newton did not demonstrate that an *oblatum* is a possible form of relative equilibrium; but, assuming it to be such, he calculated the ratio of the axes. This assumption may be called Newton's *postulate* with respect to the figure of the earth; the defect thus existing in his process was supplied about 50 years later by Stirling and Clairaut" (§ 44). Newton appears to have arrived at his theorems in attraction in 1685; the first edition of the "Principia" made its appearance in 1687. (De Morgan, in his "Budget of Paradoxes," p. 81, discusses some of the sources of the apple story.)

Mr. Todhunter nowhere takes account of theories maintained before the time of Sir Isaac Newton; these were, for the most part, if not entirely, non-mathematical. A sketch is given in Book III. of Maclaurin's "Account of Sir Isaac Newton's Philosophical Discoveries, in Four Books."* We draw attention to this work because no reference is made to it in the *History*, whereas great part of Books III. and IV. is devoted to the subject of gravitation.

The same reason (for we cannot suppose Mr. Todhunter not to have consulted the work) has possibly induced him to pass over in silence the "Theoricæ Mediceorum Planetarum ex causis Physicis Deductæ"† of Borelli, though Libri, in his Catalogue, states that this writer "uses the principles of the law of Attraction as afterwards promulgated by Sir Isaac Newton."

Hardly a subsequent chapter but contains from one or another writer an acknowledgment of Newton's high powers; we shall here content ourselves with citing only Laplace's warm eulogy:—"Cet admirable ouvrage contient les germes de toutes les grandes découvertes qui ont été faites depuis sur le système du monde;" and further, he says, that the first step thus made by Newton in the theory must appear immense.

Huygens next appears on the scene. Our author (§§ 64, 65) clears up one or two points, more especially the rightful claim to priority of Newton over Huygens; an error which crops up in Barlow's "Mathematical Dictionary" and Svanberg's work on the Lapland Operations.

From § 48 we gather that Mr. Todhunter has not seen the *Opera reliqua*. S'Gravesande, in the preface to vol. ii. (we quote from the edition Amstel, 1728, 4to.), says, "Tractatus de lumine et dissertatio de gravitate quæ ambo scripta gallice dedit auctor, quamvis primum ut ipse in hujus præfatione monet, in linguam latinam vertere sibi proposuerat:" the second is turned into Latin with title "De causâ gravitatis." The *præfatio* occupies pp. 95, 96, *dissertatio* pp. 97-116 with an *additamentum* down to p. 136, and there is a plate: the *De vi centrifugâ* occupies pp. 107-134.

In the *Opuscula postuma* (Lugd. Bat. 1703, 4to.) the treatise *De vi centrifugâ* (pp. 401-428) is founded, if we mistake not, on a different view of gravitation from that assigned to him by Mr. Todhunter in § 50.

Miscellaneous investigations, up to the year 1720, are then considered: Burnet's "Theory of the Earth" is glanced at, Keill's examination of the same, David Gregory's writings (which contribute nothing new), Hermann, Mairan ("Misapplied Mathematics and Misplaced Ingenuity") and the Cassinis, under whose powerful influence doubts arise as to the real shape of the earth, are more fully discussed.

We proceed to Maupertuis, a memoir* by whom is said (§ 128) to be the first example of the adoption of the principle of attraction by French mathematicians. We offer here a collation of the first editions of two of his works with the second editions which Mr. Todhunter discusses (§ 143).

The *avertissement* of 3 pp. in the 1738 edition of the *Examen désintéressé* is not reproduced in the 1741 edition: the 82 pp. of text, as also the 3 pp. of contents, at end, and the one page of errata, appear to be identical with the matter in the later edition. For the *Examen des trois*, &c., the bookseller's *avertissement* (4 pp.) is common to both: the 42 pp. of text appear to be the same; there is no list of errata; the foot-notes of the later edition appear as side-notes in the 1738 copy. The copy we consulted had the two essays bound up together, and is a duodecimo volume.

In the "Philosophical Dissertations on the Uncertainty of Human Knowledge" by the Marquis d'Argens, author of the "Jewish Spy," to which is added M. Maupertuis' "Dissertation upon Gravity,"&c., translated from the French edition, in 2 vols. 1753, with the running title "The Impartial Philosopher," we have the following:—"After M. Maupertuis had examined the Newtonian system and after he had undergone infinite dangers and difficulties in the frozen regions of the North, in verifying a particular part of it, he concludes that we may look upon gravity as a power diffused through all parts of matter by which all its particles attract each other. The concurrence of all the force of matter which composes the earth, attracts and causes bodies to fall towards its surface, keeps the moon in her orb, and produces with regard to the other planets, and with respect to the sun, the like phenomena, always in proportion to the quantity of their force, their direction, and their distance" (pp. 255-263).

Whilst treating of Maupertuis, we think we have seen in the Graves' library the English translation of his "La figure de la terre . . . au cercle polaire," 1738: possibly the extract cited above is taken from it. In § 149 Mr. Todhunter says "Childrey seems" &c.: Joshua Childrey, 1623-1670, was of Magdalen College, Oxford, Archdeacon of Salisbury, 1663. He wrote "Britannia

* London, 1775. 3rd edit. 8vo.
† Florentiæ, 1666. 4to.

* "Sur les loix de l'attraction:" cf. Bailly.

Baconica, or the Natural Rarieties of England, Scotland, and Wales, according as they are to be found in every Shire." (London, 1660. 8vo.) The passage referred to in the *Examen désintéressé* we presume was taken from the Paris edition (1667. 12mo.), under Carnarvonshire (pp. 244, 245 of the French, pp. 147, 148 of the English edition). In his dedication he writes:—"The calling I have entred into, and the capacity * wherein I have the honour to serve your Lordship, wil (I fear) offend the weake tendernesse of some, who think these deep searches into reason misbecoming a Preacher of Faith, and the contemplation of the works of Nature very impedimentall (if not destructive) to the work of Grace," &c.

Stirling (whom his rival Clairaut calls "one of the greatest geometricians I know in Europe") enunciated without demonstration approximate propositions respecting the magnitude and the direction of the attraction of an homogeneous *oblatum* at its surface and *implicitly* (§ 156) established Newton's *postulate*.

We now proceed to give an account of the original work (not seen by Mr. Todhunter) entitled "Dégré du méridien entre Paris et Amiens." (Paris, 1740.) It has 6 pp. of contents:—lvi. pp. of Part I. in nine chapters, with 3 pp. of plates. Part II. is "Mesure de la terre" par M. l'Abbé Picard, 106 pp., with 10 pp. on Aberration of fixed stars, and 5 pp. of fixed plates. On p. vi. we find, "le dégré comparé à celui que nous avons mesuré au cercle polaire, que nous avons trouvé de 57437, 9 toises donne la terre aplatie vers les pôles; et le rapport de l'Axe au diametre de l'equateur, comme 177 à 178." We had some difficulty in finding the book in the Museum from Mr. Todhunter's description; at last we found it catalogued under the heading Picard.

The Museum copy of the Essay by Celsius (§§ 198, 739, not seen by our author), entitled "De Observationibus," &c., Upsaliæ, 1738, is bound up with several other tractates on our subject, but all the rest partake of the character of the ante-Newtonian writers. Thus Nicolaus Winterberg (1596) heads his chapters—"Rotundam esse (terram) liquido apparet;" "Terram cum aquâ conjunctam σφαιροειδῆν asserimus;" and he further maintains the earth to be the centre of system of universe. We need not give an analysis of Celsius's work here; for this is undoubtedly the original from which the German translation, discussed by Mr. Todhunter, was taken. He styles Newton "vir immortalis," and, deciding against James Cassini, thus ends his ten-page tract—"Spero itaque me jam æquo et candido lectori satis superque ostendisse observationes Cassinianas, tam cœlestes quam terrestres in Gallia præcipue meridionali habitas, adeo incertas esse et inde figura telluris nullo modo deduci queat."

We have now arrived at the period when the question between the Newtonians and Cassinians was decisively settled, and the victory of the *oblates* over the *oblongs* acknowledged even by the Cassinis. This result was brought about by the expedition to Lapland in 1736-37, and won for its ruling spirit, Maupertuis, Voltaire's witty compliment of having "aplati les pôles et les Cassinis."

R. TUCKER

(*To be continued.*)

* He was chaplain to the "Rt. Hon. my most noble Lord and Master Henry Somerset Lord Herbert, &c."

OUR BOOK SHELF

Elements of Chemistry, Theoretical and Practical. By William Allen Miller, M.D., LL.D. Part II. Inorganic Chemistry. Revised by Herbert McLeod, F.C.S., Professor of Experimental Science, Indian Civil Engineering College, Cooper's Hill. 5th edit., with additions. (London: Longmans, 1874.)

IT will of course be superfluous to say anything in the way of criticism concerning this well-known manual. The death of its lamented author has necessitated the placing of the fifth edition in other hands, and it could not have fallen into better than those of its present editor. The principal changes so far have been a re-arrangement of the articles in accordance with the modern method of study and the removal of certain parts, such as those on gas analysis and the description of certain carbon compounds, to the appendix preparatory to their removal to the third part, to which they more strictly belong.

Some of the constitutional formulæ, now so much in use, have been introduced, and the kind selected have been those used by Frankland in his well-known "Lecture Notes." We are, however, glad to see that these have not been used to the exclusion of the notation adopted in former editions.

Great credit is due to Prof. McLeod for the thorough and conscientious way in which he has performed his task, and the only fault we have to find is that there is occasionally a certain amount of confusion caused by the use of different names for the same body, a fault for which, however, the science itself is largely responsible.

R. J. F.

Zones of Parallel Lines of Elevation in the Earth's Crust. By Angus Ross, sec. and mem. com. N. S. Inst. of Nat. Science. (Halifax, Nova Scotia, 1872.)

HE is a bold man who will predicate that no future discovered fact will disturb even the most widely accepted hypothesis. This being so, all hypotheses being in fact tentative only, and valuable in so far as they enable us to classify and deduce laws from such facts as we know, we ought to welcome every generalisation which groups known facts under some new aspect. In the above pamphlet we have such a generalisation. Whether it will prove to be supported by future discovery, or even whether it can be rigidly applied to explain actual facts will require much close criticism to determine. We can only say that it is ingenious and novel. The author claims to have discovered the method of distribution of the various mountain chains or lines of anticlinal elevation. These he asserts are arranged in parallel lines along certain belts or zones which girdle the earth, each zone following approximately the course of a great circle, and each having for its medial line or axis a line of volcanoes. Of these zones he describes seven, and we may extract one as a type of the rest. "Zone No. 1 on the Rocky Mountain system has its axial line in the volcanic belt extending from the middle Andes, inclusive, across Central America along the Rocky Mountains, Alaska, the Aleutian Islands, Kamtschatka, the Kurile Islands, Japan Islands, Loochoo Islands, Philippine Islands, Palawan, and Borneo. The Islands of Amsterdam and St. Paul, the Kerguelen Islands, the South Sandwich Islands, and South Georgia seem to indicate the completion of the more southerly part of the (approximately) great circle." The author, as we have said, describes seven such zones or belts which intersect one another, and argues that the points of intersection are foci of volcanic energy. He argues also that the great mountain-chains in their direction follow the course of one or other of these zones, and thus describes their arrangement:—"In each zone the proximity and elevation of the anticlinals diminish gradually from the axial line outwards, and if zone No. 1 be considered the most recent, and the others as successively less recent in the order in which I have named them, and comparing similar parts of any two zones, the height of the anticlinals is greater, the dip less, and the difference between their axes greater in the more recent." The pamphlet is ably written and very deserving of study. HENRY H. HOWORTH

LETTERS TO THE EDITOR

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

Animal Locomotion

IN NATURE, vol. ix. p. 301, there is a letter from Mr. Wallace on a very important point connected with the Theory of Flight. The question he discusses is "whether a bird's wing during onward flight moves *downwards and backwards* or *downwards and forwards;*" and Mr. Wallace supports Mr. Pettigrew in affirming that the movement is *downwards and forwards.*

As this is a subject to which I have paid long and close attention, I desire to express my conviction that neither of the two motions thus described by Mr. Wallace is the true motion of a bird's wing in forward flight.

The true motion is one strictly vertical to the axis of the bird's body; and as that axis is ordinarily horizontal in flight, the wing-stroke is a vertical stroke, that is simply downwards, and neither "downwards and forwards" nor "downwards and backwards."

This is not a question of theory, but a question of fact, to be determined by observation. The wing-stroke of most birds is indeed so rapid that the eye cannot distinctly follow the operation. But there are birds whose wing is so large and whose flight is so slow, that the wing-stroke can be followed with the greatest distinctness. Such is the common heron—common, alas, no longer in most parts of England, but numerous on the west coast of Scotland. When at home I am in the daily habit of watching their flight; and the truly vertical character of the wing-stroke is a fact which I have verified by the eye under every possible condition which could supply the evidence.

There are indeed two slight modifications of the perfect perpendicularity of the stroke which result (1) from the attachment of the wing to the body of the bird, and (2) from the structure of the wing-feathers. The first of these two modifications consists in this—that as the wing moves upon a hinge, its extremity must move downwards, not absolutely vertically, but describing an arc. The segment of a circle, however, through which the wing thus moves, is generally a very short one: and in so far as the movement of the extremity departs from the vertical, it departs therefrom neither "backwards" nor "forwards," but (as it were) "inwards,"—that is, in the direction of a circle encompassing the axis of the bird's body as with a hoop. Pigeons, as an amusement and in play, often complete this circle—making their primary quills clash against each other over their backs, and downwards again under their breasts. But in ordinary forward flight, when birds are intent only on progression, the wings move through a very small arc indeed of the complete circle referred to.

The second modification of the perpendicularity of the stroke arises from the "set" of the wing-feathers—which curve backwards and downwards from the wing-bones. In some birds, and notably in the heron, and all the storks, the concavity thus formed is very deep, and of course a surface which is thus not a plane surface, but a concave one, however truly it may be struck downwards, cannot have a purely vertical reaction on the air.

When we observe, however, that in the case of many birds, and some of these the most powerful fliers in the world, this concavity of the wing-feathers is very slight indeed, and that the whole vane is very narrow, flat, and "taut," it is obvious that a purely vertical stroke, or one as near it as possible, is the really essential stroke for flight.

The great secret of flight is the exquisite and complicated adaptation of structure in the feathers of a bird's wing which derives from this one simple action the resultant of a force which is both sustaining and propelling. It is an adaptation which, when thoroughly grasped and understood, at once dispenses with as needless, and condemns as mechanically erroneous, all the explanations which assume either a "downward and forward" or a "downward and backward" movement.

I venture to think that Mr. Wallace is certainly in error when he ascribes to Mr. Pettigrew the merit of having been the first to show that "horizontal forward motion is a general resultant of the upward and downward action of the wings under the influence of gravitation."

In February 1865 I published in *Good Words* a paper on the mechanism of flight, in which this effect of the wing-stroke was fully explained, and elaborately illustrated. This paper subsequently appeared as chap. iii. in the "Reign of Law" published in the end of 1866. Mr. Pettigrew's lecture before the Royal Institution (in which I believe his views were first promulgated) was delivered on March 22, 1867. I had the pleasure of hearing that lecture, and the amusement of recognising parts of it (including even a poetical quotation) as taken directly from my chapter on flight. The pleasure, however, was somewhat abated by the strange mixture of much that was quite correct, with a great deal more which I believed then, and believe now, to be wholly erroneous. ARGYLL

March 11

MR. WALLACE has well said that the question, How a bird's wing moves in flight, "is a very important question." In these days, when scientific attention is being directed to the problem of aërial navigation, it is especially important. I have the less hesitation, therefore, in troubling you with some further remarks in reply to the strictures of this very accurate observer.

At the outset I must deny that I assumed either that a bird's wing is inflexible or that it is a plane. Of its flexibility I had no cause for speaking at all; but so far from regarding it as a plane, I expressly objected to Dr. Pettigrew so representing it in his supposed refutation of the orthodox view. The point in dispute is entirely concerning the down stroke; against Mr. Wallace's account of the up stroke I make no objection.

First, what may we infer *à priori* concerning the down stroke? (1) Its efficiency is independent of the velocity of the bird: this is simply a consequence of the second law of motion. We have to suppose a bird fixed in still air, and to ascertain the effect which ensues on a downward blow of the wing. The subsequent forward velocity of the bird, so far as that depends on the down stroke, is but a consequence or an accumulation of these effects. It is thus only needful to analyse the single effect itself. To this end the shape and varying flexibility of the wing must be noted. Along the exterior margin we have a rigid area, comparable to the blade of an oar, and formed for the most part of bone, in the *top side* of which the rigid tubes of the primary and secondary feathers are inserted. On the under side of this, which we may term the oar part of the wing, there is thus a considerable concavity, the direction of which when the wing is extended is decidedly backward. The area towards the middle line of the wing is flat and horizontal, approximately so at all events, when the bird is freely suspended in the air. Of the posterior, the larger, half of the wing it is true, as Dr. Pettigrew says, that the aspect is forward, more especially in heavy birds with broad and rounded wings. The flexible extremities of the feathers readily turn upwards like vanes in the manner so well shown in Fig. 80 of Dr. Pettigrew's work. We may thus roughly distinguish four areas, beginning from the front: (*a*) the oar area; (*b*) the plane or flapping area; (*c*) the kite area; (*d*) the vane area. (2) Now we may inquire what will be the effect of each when the wing is struck downward. The reaction from the oar area will be (*a*) a force directed upwards and forwards; that from the plane area (*b*) a force directed upwards simply. Against the kite area will impinge the air sent backwards and rebounding from the blow of the oar area; the effect of this (*c*) is all that corresponds to what Dr. Pettigrew calls the kite action of the wing. Lastly, the same air in escaping through the feathers, and especially in raising the tips in the vane area, will produce the forward motion (*d*) to which Mr. Wallace refers, besides contributing something (*e*) to support the bird's weight. The horizontal component of (*a*) together with (*d*) will carry the bird forward. The slighter horizontal component of (*c*)—slighter because proceeding only from the rebounding air and from a yielding surface—will tend to hinder the forward motion: hence the absence, more or less complete, of this area in quick fliers. The forces (*b*), greater part of (*c*), and (*e*) will sustain the bird against gravity.

Neither Dr. Pettigrew, nor apparently Mr. Wallace, distinguishes the motion consequent on a surface striking against the air from that of a surface gliding through it. If I incline a sheet of paper to the horizon and let it slip from my hand it will descend with a similar incline towards the ground; but if, having stiffened it, I strike it against the air at the same inclination it will tend to rise in a direction at right angles to that inclination. The *blow* directed downwards and backwards must give an impetus upwards and forwards; the *still surface* so directed will glide downwards and forwards. I do not deny that if the down stroke of the bird's wing be directed backwards, *beyond a certain angle*, the resultant motion will be, as Mr. Wallace says, "obliquely downwards." But why? Because all the sustaining forces above enumerated are so seriously diminished—the horizontal and forward forces, with the exception of (*d*), being increased—that, to use Mr. Wallace's words, "the surplus vertical reaction of the down stroke over the up stroke is no longer able to overcome gravity," which converts the bird's wings for the nonce into kites as it comes sailing downwards, making but an occasional strike, now that the horizontal effect of the wings is so great, to increase the obliquity of its descent.

But within the limits of this angle, whatever they be, the effect of a downward and backward blow must, on mechanical grounds, be in general such as I have said. For clearness' sake it may not be superfluous to note an ambiguity in the expressions "downwards and backwards," "downwards and forwards;" they may apply either to the direction of the surface of the wing or to the direction of the anterior margin. I maintain only that the direction of the surface—in some wings, merely that of the anterior portion of the surface—is downwards and backwards. The anterior margin, by the contraction of the great pectoral muscle, is drawn downwards and forwards, in which, by the way, there is the further advantage that less air will escape from under the wing in front.

But, secondly, what can we *observe* as to the down stroke? (1) A fact, pointed out to me by an anatomical friend—that the great pectoral muscle which depresses the wing is inserted into a crest situated on the upper and forward side of the head of the humerus, so as to tilt the under surface of the wing slightly upwards, *i.e.* give it a backward direction. (2) If the flight of rooks, or still better of pigeons, be watched from a window towards sunset, the position of the shadows on the under side of the wings will be found pretty conclusive as to their direction. (3) The forward inclination of the wings of a bird about to alight, which shows that the motion of the wings in such a position retards flight. (4) The action of heavy land or water birds, that have to attain some momentum by the use of both feet and wings before they can rise; here surely a forward blow against the air is manifestly absurd. (5) "The highly-inclined position of a hovering bird," noticed by Mr. Wallace, and not of the bird only, but of his wings.

Mr. Wallace's closing remark is both true and sound:—"A bird's wing is a highly complex apparatus, subject to a variety of flexures and motions in every feather." Still it is possible, even probable, that all this variety is referrible to a few simple principles. It is with these alone that I have ventured to concern myself. JAMES WARD

Trin. Coll., Camb., March 3

WITH reference to Dr. Pettigrew's letter in NATURE, vol. ix. p. 362, I cannot do better than ask him to read the two papers that I refer to in my former reply, which he has evidently not done.

March 16 A. H. GARROD

The Moon's Want of Atmosphere

YOUR very suggestive review of Messrs. Nasmyth and Carpenter's work on "The Moon" leads me to propose an explanation of the absence of a lunar atmosphere, which I do not remember to have seen anywhere. The many arguments in favour of the temperature of the lunar surface being near or at the *absolute* zero, when added to the equally probable supposition that at the *absolute* zero all matter assumes the solid form, makes nothing more probable in my mind than that it is the consolidation from cold of all the previously existing gases and vapours of the Moon which has caused its atmosphere to disappear. Prof. Frankland's theory of the frozen condition of the lunar surface is evidently different from the above, and Lord Rosse's observations on lunar radiation apply only to the direct reflection of the solar rays. A. H. GARROD

On Volcanic Eruptions

A PASSAGE in Nasmyth's work on the "Moon" suggests, as a consequence, an explanation of volcanic eruptions that I have often given in lectures. The point to be explained is, why they are sudden and intermittent. Processes of cooling and expansion are gradual.

I postulate (1) that a solid crust is shrinking as it cools; (2) that the liquid interior expands on solidifying; (3) that the melting-point of lava is lowered by pressure.

Let us start with a volcanic vent in which the aperture has become partially stopped by cooled or solidified lava. In the region below, pressure sets in from the cooling and ultimate solidification of part of the liquid mass. Hence the melting-point of the rest is lowered by (3). The process continues until the pressure becomes sufficient to relieve itself through some vent, old or new; a lava rises in the vent. But this relieves the pressure, and it follows from (3) that more rock will solidify, *suddenly*, and in so doing force liquid rock *rapidly* up the vent.

A volcano is, in fact, a geyser of lava.

I do not remember to have seen this in any book; and it perhaps would have been hazardous to assert postulate (2) as certainly true previous to Nasmyth's experiments; but I have thought it probable: and if it is true, postulate (3) follows, I believe, from the laws of heat, and the explanation will be sound. I shall be glad to hear what is thought of it by authorities.

Rugby, March 13 J. M. WILSON

Remarks on Ozone

HAVING perused Dr. Moffatt's interesting communication on Ozone in the *Scottish Meteorological Journal* of October last, and also noticed the paragraph on the subject in the *Medical Times* drawing attention to it, I beg to send the following remarks respecting some points in it open to criticism from outsiders:—

1. The numbers, and special *years of records*, are not stated in the statistical tables, which might be of importance for comparison with other persons' records.

2. The occurrence of ozone with *hail*, and not with snow, may be explained by its happening in warmer weather, and not in winter, and in the warmer stratum of air through which the hail falls from the cold stratum above.

3. The larger quantity of ozone in Table II. in *winter over that in summer* is anomalous, and inconsistent apparently with the records in Tables VI. and VII., where it is stated to increase with the temperature.

4. If ozone be thought to increase in quantity with *increase of elevation* above the level of the sea, it may be asked how that is to be reconciled with the greater prevalence of it at the sea-shore than inland.

5. If there be only an apparent connection between *electrical storms* and ozone, explanation may be required to account for the production of artificial ozone by electrical action, and whether the two be identical in constitution if not in origin.

6. The paragraph—"The *air is drier* near the tropics than about the equator," might be more clearly defined by adding the "tropical circles of cancer and capricorn," as within those lines it certainly gradually gets more and more humid.

7. In one paragraph there is stated to be an intimate connection between *humidity* of the atmosphere and the manifestation of ozone, and in another this is stated to be purely accidental, which is ambiguous, while the testimony in support of it is not in accord with that in Tables IX. and X., where the adverse record is apparent.

8. That the absolute *humidity of the air* diminishes with increase of elevation may be true in the case of lofty balloon ascents, away from any terrestrial influences of mountains, but, as pointed out in a note, the *relative humidity* increases, as we may see in Westmoreland or Dartmoor, where the heights are always misty and damp.

9. The tropical or *trade winds* only chance to be land winds in some such regions as the North Indian Ocean, whereas they are generally said to be sea-breezes, as in the South Atlantic Ocean, in the ordinary acceptation of the term.

10. The connection of the production of ozone by the means of *turpentine* will bring to the mind of the tourist the freshness of the air of hills planted with pine forests.

11. Accepting the theory that ozone is connected with the equatorial winds, it may be asked how the increase of ozone in the *calm belts* is to be accounted for, where there are only Polar winds, converging to ascend into the upper regions of the air from north and south.

12. The table of *observations at sea* on board ship would require to be supplemented by a note of the period of the year and number of days on record, as the quantity of ozone is already stated to vary with the *seasons of the year* (Table II.), and

the winds, temperatures, and barometrical indications might have been added for the like reason.

13. Table XI. also requires a note of the season of the year and the number of days of observations. It may here be asked, how is the discrepancy to be reconciled between the lessening of ozone as you sail *to the Polar Regions*, and the increase of ozone as you ascend in the air, when the temperature as regularly falls in the one case as in the other.

14 The records showing the connection between *phosphorescence* and manifestation of ozone are very satisfactorily drawn out, and may probably become of much value in a new investigation.

15. The less prevalence of ozone in the higher *extratropical latitudes* may be due, as suggested in another case, to the dryness of the atmosphere impairing the *sensitiveness of the test papers*, so that for the present such deductions are under suspicion.

16. The idea that the prevalence of ozone is coincident generally with a *low barometer* seems well supported by the observations recorded, but some explanation will be required to account for its maximum occurrence with *south-east winds* in Tables IX. and XI., if one should accept the theory of its connection only with equatorial winds.

17. That its presence may be connected with *warm temperatures* of the air seems better established at sea than on land, as also its coincidence with *humidity of the air*, though this is somewhat vitiated by the conscientious suggestion that its manifestation may be due to the increased susceptibility of the test-papers when moist.

18. In the statement that ozone increases as you ascend *mountainous elevations*, it is not stated what winds were blowing at the time, which would appear to be necessary, if the idea of its prevalence with any particular wind were considered essential.

19. The key to the origin and prevalence of ozone in the atmosphere seems still undiscovered, and we do not yet appear to have determined if it belongs to aqueous vapour or a special wind, or whether it be an additional constituent of the air, like carbonic acid, or a floating entity, like a cloud.

NUBIBUS

The Limits of the Gulf Stream

As one of those engaged in the compilation of the Atlantic pilot-charts published by the Admiralty, on which are given the limits, velocity, and general features of the Gulf Stream, as well as the boundaries of the regions in which ice and icebergs may be fallen in with in the North Atlantic, I cannot allow the letter in NATURE (vol. ix. p. 343), by W. W. Kiddle, of the White Star Mail steamship *Oceanic*, to remain unchallenged.

The Gulf Stream and ice boundaries, delineated on the North Atlantic chart, referred to in that letter, are in their details transcripts from the Atlantic pilot-charts.

These details were the result of much patient investigation, and obtained from many sources probably unknown to Captain Kiddle; among the most valuable were the painstaking and sound observations made by members of the United States Coast Survey, and to be found embodied in the annual reports between 1843 and 1859; and especially from the exhaustive and learned work on currents, so well known to cultivators of nautical science, by the late Major Rennell.

If the average boundaries of the Gulf Stream cannot be laid down within reasonable limits from the authorities I have quoted, aided, too, by the many observations of ships of war, extending over the present century, I fear that Captain Kiddle's results will not assist us in a more accurate delineation.

It is, however, to be hoped that Captain Kiddle's information on the currents may be more reliable than that he has ventured upon giving with regard to the limits of iceberg-drift; here recorded facts are irresistibly against him. He has only to consult any North Atlantic memoir on the subject, and he will find that icebergs have been fallen in with so far south as 36° 10′ N., or 7° south of the high authority he quotes. I would refer him on this interesting subject, as well as how icebergs are found on the southern edge of the Gulf Stream, and why it is possible "that bergs could drift square across the heated waters of the Gulf Stream to lat. 39° N.," to a paper by the well-known W. C. Redfield, of the United States (reprinted in the *Nautical Magazine* for 1845), who gathered the facts that have simply been utilised in the Admiralty charts.

London, March 11 THOMAS A. HULL

The Great Ice-Age

MR. GREEN, reviewing Mr. J. Geikie's work on the "Great Ice-Age" (NATURE, vol. ix. p. 318), expresses the opinion that a glacial period must have been one of intense cold. This is the general opinion, and yet I think it can be shown to rest on a misconception. If the climate at any given elevation is cold enough to form glaciers, no decrease of the winter temperature will increase their magnitude; while on the other hand a low summer temperature is shown by the facts of physical geography to be eminently favourable to glaciation. This last may almost be called an identical proposition, for permanent snow means snow which lasts through the summer.

As Mr. Croll has pointed out, there have been periods where the sun's greatest and least distances were respectively greater and less than now. He thinks that a glacial period occurred when, in the course of the precession of the equinoxes, the sun's greatest distance occurred in the winter, so as to cause a *cold winter*. I think the true theory of the glacial climate is exactly the reverse of this: that is to say, it was caused by the *cold summer* which occurred when the sun's greatest distance was in the summer.

I have stated these views at greater length in the *Journal of the Geological Society of London*, 1869, p. 350.

Old Forge, Dunmurry, Co. Antrim, March 8 J. J. MURPHY

Mars

IN a most interesting article on the planet Mars, in your issue of NATURE for Feb. 19, which has just been shown to me, the Rev. T. W. Webb directs attention to the question of the colours of Mars being due to effects of contrast or not, and says—"Nor does it seem to have been noticed that no effect of contrast has been traced in the Polar snows."

Kindly permit me to inform Mr. Webb that, in a paper on Mars in the last volume of the "Monthly Notices of the Royal Astronomical Society," I expressly state that, "on May 14, 1873, the south Polar ice appeared (in an 8½-inch silvered glass reflector, by Browning) of quite a pale sky-blue colour, evidently by contrast," and I may add that this effect I noticed also on two or three subsequent occasions.

Burton-on-Trent, March 12 EDWARD B. KNOBEL

POLARISATION OF LIGHT*

VI.

MENTION was made in the previous article of the bands produced in the spectra of polarised light. Beside the fact of the existence of these bands it has been found upon examination that the state of polarisation at different parts of the interval between two successive bands varies; and such an examination may be made by means of a quarter-undulation plate or a Fresnel's rhomb.

If we carefully examine the spectrum of light which has passed through a selenite, or other ordinary crystal, we shall find on turning the analyser that, commencing with two consecutive bands in position, the parts occupied by the bands and those midway between them are plane-polarised, for they become alternately dark and bright; while the intermediate parts, *i.e.* the parts at one-fourth of the distance from one band to the next, remain permanently bright. These are, in fact, circularly polarised. But it would be incorrect to conclude from this experiment alone that such is really the case, because the same appearance would be seen if those parts were unpolarised, *i.e.* in the condition of ordinary light. And on such a supposition we should conclude, with equal justice, that the parts on either side of the parts last mentioned (*i.e.* the parts separated by one-eighth of the interval between two bands) were partially polarised. But if we introduce a quarter-undulation plate between the selenite and analyser, with its axis inclined at 45° to that of the selenite, circular polarisation will be converted into plane and plane into circular. This being so, the parts which

* Continued from p. 326.

were originally banded ought to become bright and to remain bright, while those that were originally bright ought to become banded during the rotation of the analyser. The effect to the eye will consequently be a general shifting of the bands through one-fourth of the space which separates each pair. Further, as on the one hand plane polarisation is converted into circular right-handed or left-handed by two positions of the plate at right angles to one another; so on the other right-handed circular polarisation will be converted by the plate in a given position into plane polarisation having the vibrations in one direction, and left-handed into plane polarisation having the vibrations in a direction at right angles to the former. Hence, if the plate be turned through a right angle from the position first described, the band will be shifted in a direction opposite to that in which they were moved at first. In this we have evidence not only that the polarisation on either band is circular, but also that on the one side it is right-handed, while on the other it is left-handed.

All the phenomena hitherto described manifestly depend upon the internal structure of the crystal plate, in virtue of which it affects the vibratory movement of the ether within it differently in different directions. And seeing that most crystals, when broken, divide themselves naturally into smaller crystals having the same form, *i.e.* having their planes and edges similarly inclined, we are naturally led to conclude that the structure of these bodies may differ not so much in different parts, as along different lines or planes connected with the forms into which they break, or (as it is also described) with their planes of natural cleavage. And this suggests the question whether an uncrystalline body might not, by pressure, or strain, or other mechanical distortion, be caused to affect the motions of the ether within it in a manner dependent upon their direction, and in that way to exhibit chromatic effects with polarised light analogous to those described above. Experiment answers this question in the affirmative.

The simplest experiment in this branch of inquiry consists in taking a rectangular bar of ordinary glass; and having crossed the polariser and analyser so as to give a dark field, to strain the bar with both hands as if we were trying to bend it or to break it across. The side towards which it may be supposed to be bent is of course compressed, while the opposite is stretched out. Between these two there must be an intermediate band, more or less midway between the two, which is neither compressed nor stretched. The moment the strain is put upon the bar light will be seen to pass through the parts of the bar nearest to both sides, while a band remains dark midway between the two.

This shows that the mechanical strain has imparted to portions of the glass a structural character analogous, at all events optically, to that of a crystal. The effects may be increased and rendered more striking by placing the glass in a frame furnished with a screw, by which the rod may be firmly held and considerable pressure applied at particular points. When this is done the structural character becomes more completely developed, and the dark band is fringed with colours which appear to flow inwards or outwards according as the pressure is increased or diminished. A slightly different, but more effective, exhibition of chromatic polarisation is produced by squeezing a thick square plate of glass in a vice. In this case the pressure may be carried further without fear of fracture, and the chromatic effects heightened.

It is, however, well known that molecular forces, such as those due to heat and cooling, in many cases far transcend in intensity those which we can exert by mechanical arrangements. And, in fact, if a block of glass be unequally heated to a very moderate degree, the internal structural effects immediately reveal themselves by dark bands, which indicate the border land between strain and pressure. As the block cools, these landmarks gradually disappear, and the field becomes again uniformly dark. But by far the most splendid effects (and these are permanent) are produced by unannealed glass; that is, by glass which has been rapidly and therefore unequally cooled. When a mass of glass has been cast in a mould in the form of a thick plate, then whatever be the contour line, the outside will cool first and become a rigid framework to which the interior of the mass must accommodate itself. The nature and direction of the pressure at each point of the interior will be primarily dependent upon the form of the contour; and by adopting various forms of contour the most beautiful and varied figures with coloured compartments may be produced. The forms and colours of the figures produced by transparent bodies when submitted to polarised light have been conversely used as a means of measuring, with almost unparalleled accuracy, the mechanical pressures which such a body is undergoing.

Besides glass many other substances may be used as reflectors so as to produce polarisation; for example, leaves of trees, particularly ivy, mahogany furniture, windows, shutters, and often roofs of houses, oil paintings, &c., and last but not least the surface of water. In each of these cases when the reflected beam is examined with a Nicol the alternations of light and darkness are most strongly marked, and the colours (if a crystal plate be used) are most vivid, or in technical language the polarisation is most complete, when the light is reflected at a particular angle. In proportion as the inclination of the incident light deviates from this angle the colours become fainter, until when it deviates very greatly all trace of polarisation disappears.

It will be found very interesting to examine the polarisation of sunshine reflected from ripples on the surface of a lake, or better still from the waves of the sea, and its different degrees of completeness produced at the variously inclined portions of the waves. But without having recourse to nature on so large a scale, an artificial piece of water may be placed in our room. A tea tray will serve as well as anything else to form our little sea; and a periodic tap at one corner will cause ripple enough for the present purpose. The waves appear bright, and although brighter in some parts than others they are nowhere entirely dark. But on turning the Nicol round the contrast of light and darkness becomes much stronger than before. In parts the light is absolutely extinguished, or the polarisation is complete; in others it is incomplete in various degrees. And if a selenite or other crystal plate be introduced we have the beautiful phenomena of iris-coloured rings playing over the surface of our miniature sea.

Suppose that we now turn our attention to the sky, and on a clear bright day we sweep the heavens with a polariscope, or even with a mere Nicol's prism, we shall find traces of polarisation in many directions. But if we observe more closely we shall find that the most marked effects are produced in directions at right angles to a line drawn from our eye to the sun, when in fact we are looking across the direction of the solar beams. Thus, if the sun were just rising in the east or setting in the west, the line of most vivid effect would lie on a circle traced over the heavens from north to south. If the sun were in the zenith, or immediately overhead, the most vivid effects would be found on the horizon; while at intermediate hours the circle of strongest polarisation would shift round at the same rate as the shadow on a sun-dial, so as always to retain its direction at right angles to that of a line joining ourselves and the sun.

Now, what is it that can produce this effect, or indeed, what produces the effect of light from all parts of a clear sky? The sky is pure space with no contents, save a few miles of atmosphere of the earth, and beyond that the impalpable ether, supposed to pervade all space, and to transmit light from the furthest limits of the stellar universe. The ether is however certainly inoperative in the diffusion of light now under consideration. But a very simple experiment will suffice to show that such a diffusion, or, as it has been better called, a scattering of light, is due to the presence of small particles in the air. If a beam from an electric lamp or from the sun be allowed to pass through a room its track becomes visible by its reflection from the motes of floating bodies, in fact by the dust in the air. But if the air be cleared of dust by burning it with a spirit lamp placed underneath, the beam disappears from the parts so cleared, and the space becomes dark. If, therefore, the air were absolutely pure and devoid of matter foreign to it, the azure of the sky would no longer be seen and the heavens would appear black; the illumination of objects would be strong and glaring on one side, and on the other their shadows would be deep and unrelieved by the diffused light to which we are accustomed. Now, setting aside the dust, there are always minute particles of water floating in the atmosphere. These vary in size from the great raindrops which fall to earth on a sultry day, through intermediate forms of mist and of fine fleecy cloud, to the absolutely invisible minuteness of pure aqueous vapour which is present in the brightest of skies. It is these particles which scatter the solar rays and suffuse the heavens with light. And it is a remarkable fact, established by Prof. Tyndall, while operating with minute traces of gaseous vapours, that while coarser particles scatter rays of every colour, in other words scatter white light, finer particles scatter fewer rays from the red end of the spectrum, while the finest scatter only those from the blue end. And in accordance with this law clouds are white, clear sky is blue.

But the point which most concerns us here is the fact, also discovered by Prof. Tyndall, that light scattered laterally from fine particles is polarised. The experiment by which this is most readily shown is as follows: Allow a beam of solar or other strong light to pass through a tube about thirty inches long filled with water, with which a few drops of mastic dissolved in alcohol have been mixed. The fluid so formed holds fine particles of mastic in a state of suspension, which scatter the light laterally; and if the scattered light be examined with a Nicol traces of polarisation will be detected. But better still, instead of using the scattering particles as a polariser and the Nicol as an analyser, we may polarise the light before it enters the tube and use the particles as an analyser, and thus produce the same effect as before, not only upon the particular point of the beam to which the eye is directed, but upon the whole body of scattered light. As the Nicol is turned the light seen laterally begins to fade; and when the instrument has been turned so as to cut off all vertical vibrations, the only parts remaining visible in a horizontal direction will be those reflected from the larger impurities floating in the water independently of the mastic. The direction of vibration of the light polarised by lateral scattering is easily remembered by the fact that the vibrations must be perpendicular both to the original and to the scattered beam; if, therefore, the latter be viewed horizontally, they must be perpendicular to two horizontal straight lines at right angles to one another, *i.e.* they must be vertical.

An effect still more beautiful, and at the same time perhaps more instructive, may be produced by interposing a plate of quartz between the Nicol and the tube. The whole beam then becomes suffused with colour, the tint of which changes for a given position of the spectator with the angle through which the Nicol is turned.

And not only so, but while the Nicol remains at rest the tints are to be seen scattered in a regular and definite order in different directions about the size of the beam. But this radial distribution of colours may also be shown in a more striking manner, by using a bi-quartz, which as explained before distributes the colours in opposite directions. The beam should in every case be viewed at right angles; the more obliquely it is viewed the less decided is the polarisation.

The colours here seen are those which would be observed upon examining a clear sky in a position 90° from that of the sun; and the exact tint visible will depend upon the position in which the Nicol is held, as well as upon that of the sun. Suppose, therefore, that a Nicol and quartz plate be directed to that part of the sky which is all day long at right angles to the sun, that is, to the region about the north pole of the heavens (accurately to the north pole at the vernal and autumnal equinox), then if on the one hand the Nicol be turned round, say, in a direction opposite to that of the sun's motion, the colours will change in a definite order; if, on the other, the Nicol remain stationary while the sun moves round, the colours will change in a similar manner. And thus, in the latter case we might conclude the position of the sun, or in other words the time of the day, by the colours so shown. This is the principle of Sir Charles Wheatstone's Polar clock, which is one of the few practical applications which this branch of polarisation has yet found.

Figs. 18 and 19 represent general forms of this instrument described in the following passage by the inventor.

"At the extremity of a vertical pillar is fixed, within a brass ring, a glass disc, so inclined that its plane is perpendicular to the polar axis of the earth. On the lower half of this disc is a graduated semicircle divided into twelve parts (each of which is again sub-divided into five or ten parts), and against the divisions the hours of the day are marked, commencing and terminating with VI. Within the fixed brass ring, containing the glass dial plate, the broad end of a conical tube is so fitted that it freely moves round its own axis; this broad end is closed by another glass disc, in the centre of which is a small star or other figure, formed of thin films of selenite, exhibiting when examined with polarised light strongly contrasted colours; and a hand is painted in such a position as to be a prolongation of one of the principal sections of the crystalline films. At the smaller end of the conical tube a Nicol's prism is fixed so that either of its diagonals shall be 45° from the principal section of the selenite films. The instrument being so fixed that the axis of the conical tube shall coincide with the polar axis of the earth, and the eye of the observer being placed to the Nicol's prism, it will be remarked that the selenite star will in general be richly coloured, but as the tube is turned on its axis the colours will vary in intensity, and in two positions will entirely disappear. In one of these positions a smaller circular disc in the centre of the star will be a certain colour (red, for instance), while in the other position it will exhibit the complementary colour. This effect is obtained by placing the principal section of the small central disc 22½° from that of the other films of selenite which form the star. The rule to ascertain the time by this instrument is as follows:—the tube must be turned round by the hand of the observer until the colour star entirely disappears while the disc in the centre remains red; the hand will then point accurately to the hour. The accuracy with which the solar time may be indicated by this means will depend on the exactness with which the plane of polarisation can be determined; one degree of change in the plane corresponds with four minutes of solar time.

"The instrument may be furnished with a graduated quadrant for the purpose of adapting it to any latitude; but if it be intended to be fixed in any locality, it may be permanently adjusted to the proper polar elevation and the expense of the graduated quadrant be saved; a spirit-level will be useful to adjust it accurately. The instrument might be set to its proper azimuth by the sun's shadow at noon, or by means of a declination needle; but an observation with the instrument itself may be more readily employed for this purpose. Ascertain the true solar time by means of a good watch and a time equation table, set the hand of the polar clock to correspond thereto, and turn the vertical pillar on its axis until the colours of the selenite star entirely disappear. The instrument then will be properly adjusted.

"The advantages a polar clock possesses over a sun-dial are :—1st. The polar clock being constantly directed to the same point of the sky, there is no locality in which

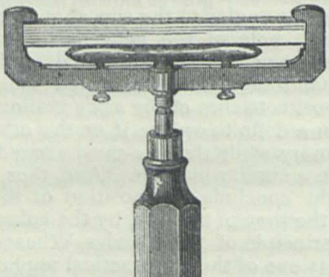


FIG. 16.

it cannot be employed, whereas, in order that the indications of a sun-dial should be observed during the whole day, no obstacle must exist at any time between the dial and the places of the sun, and it therefore cannot be applied in any confined situation. The polar clock is consequently applicable in places where a sun-dial would be of no avail ; on the north side of a mountain or of a

FIG. 17.

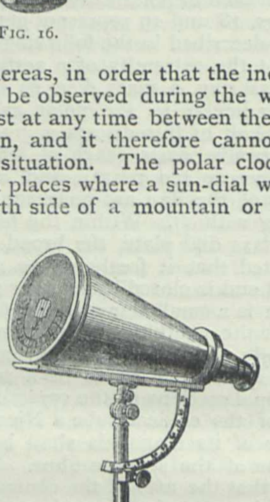


FIG. 18.—Wheatstone's Polar Clock.

lofty building for instance. 2ndly. It will continue to indicate the time after sunset and before sunrise ; in fact, so long as any portion of the rays of the sun are reflected from the atmosphere. 3rdly. It will also indicate the time, but with less accuracy, when the sky is overcast, if the clouds do not exceed a certain density.

"The plane of polarisation of the north pole of the sky moves in the opposite direction to that of the hand of a watch ; it is more convenient therefore to have the hours graduated on the lower semicircle, for the figures will then be read in their direct order, whereas they would be read backwards on an upper semicircle. In the southern hemisphere the upper semicircle should be employed, for the plane of polarisation of the south pole of the sky changes in the *same* direction as the hand of a watch. If both the upper and lower semicircles be gra-

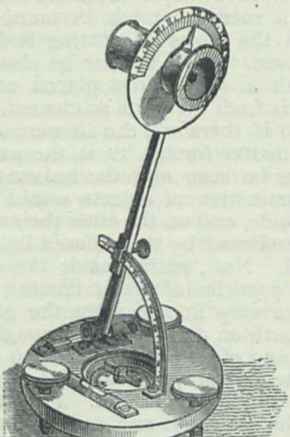


FIG. 19.—Wheatstone's Polar Clock.

duated, the same instrument will serve equally for both hemispheres."

The following is a description of one among several other forms of the polar clock which have been devised. This (Fig. 20) though much less accurate in its indications than the preceding, beautifully illustrates the principle.

"On a plate of glass twenty-five films of selenite of equal thickness are arranged at equal distances radially in a semicircle ; they are so placed that the line bisecting the principal sections of the films shall correspond with the radii respectively, and figures corresponding to the hours are painted above each film in regular order. This plate of glass is fixed in a frame so that its plane is inclined to the horizon 38° 32′, the complement of the polar elevation ; the light passing perpendicularly through this plate falls at the polarising angle 56° 45′ on a reflector of black glass, which is inclined 18° 13′ to the horizon. This ap-

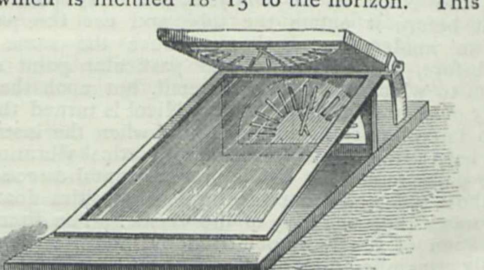


FIG. 20.—Polar Clock.

paratus being properly adjusted, that is so that the glass dial-plate shall be perpendicular to the polar axis of the earth, the following will be the effects when presented towards an unclouded sky. At all times of the day the radii will appear of various shades of two complementary colours, which we will assume to be red and green, and the hour is indicated by the figure placed opposite the radius which contains the most red ; the half-hour is indicated by the equality of two adjacent tints."

W. SPOTTISWOODE

(*To be continued.*)

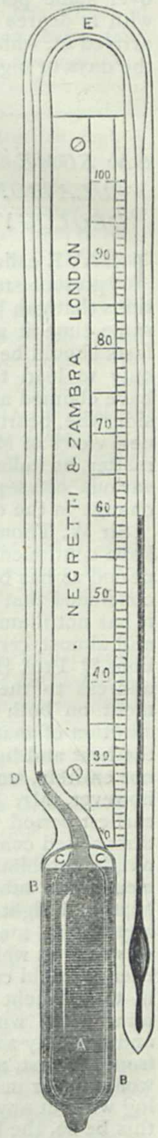
A NEW THERMOMETER

OUR readers will doubtless recollect a recent discussion in our pages relative to the priority of the invention of protected bulbs for deep-sea thermometers. The discussion has done something more than establish priority of invention, it has been the means of producing what, we believe, will prove to be a new and valuable meteorological instrument, for we have before us a paper by Messrs. Negretti and Zambra, communicated to the Royal Society by Dr. Carpenter at their last meeting, describing a new thermometer of such novel construction that it cannot fail to interest all scientific persons, meteorologists especially. We regret our inability, owing to want of space, to reproduce the paper in its entirety. The following are the main points of this communication.

In Prof. Wyville Thomson's "Depths of the Sea," p. 299, occurs the following passage :—"I ought to mention that in taking the bottom temperature with the Six's thermometer the instrument simply indicates the lowest temperature to which it has been subjected; so that if the bottom water were warmer than any other stratum through which the thermometer had passed, the observations would be erroneous."

Undoubtedly no other result could be obtained with the thermometers now in use, for unfortunately the only thermometer available for the purpose of registering temperature and bringing those indications to the surface, is that which is commonly known as the Six's thermometer—an instrument acting by means of alcohol and mercury, and having movable indices with delicate springs of human hair tied to them. This form of instrument registers both maximum and minimum temperatures, and as an ordinary out-door thermometer it is very useful ; but it is unsatisfactory for scientific purposes, and for the object for which it is now used (viz. the determination of deep-sea temperatures) it leaves much to be desired. Thus the alcohol and mercury are liable to get mixed in travelling, or even by merely holding the instrument in a horizontal position ; the indices also are liable either to slip if too free, or to stick if too tight. A sudden jerk or concussion will also cause the instrument to give erroneous readings by lowering the indices if the blow be downwards, or by raising them if the blow be upwards. It was on reading the passage in the book above referred to that it became a matter of serious consideration with Messrs. Negretti and Zambra, whether a thermometer could be constructed which could not possibly be put out of order in travelling, or by incautious handling, and which should be above suspicion and perfectly trustworthy in its indications. This was no very easy task. But the instrument submitted to the Fellows of the Royal Society seems to fulfil the above onerous conditions, being constructed on a plan different from that of any other self-registering thermometer ; and containing, as it does, nothing but mercury, neither alcohol, air, nor indices. Its construction is most novel, and may be said to overthrow our previous ideas of handling delicate instruments, inasmuch as its indications are only given by upsetting the instrument. Having said this much, it will not be very difficult to guess the action of the thermometer ; for it is by upsetting or throwing out the mercury from the indicating column into a reservoir at a particular moment and in a particular spot, that we obtain a correct reading of the temperature at that moment and in that spot.

The thermometer in shape is like a syphon with parallel legs, all in one piece, and having a continuous communication, as in the annexed figure. The scale of the thermometer is pivoted on a centre, and being attached in a perpendicular position to a simple apparatus (which will be presently described), is lowered to any depth that may be desired. In its descent the thermometer acts as an ordinary instrument, the mercury rising or falling according to the temperature of the stratum through which it passes ; but so soon as the descent ceases, and a reverse motion is given to the line, so as to pull the thermometer to the surface, the instrument turns once on its centre, first bulb uppermost, and afterwards bulb downwards. This causes the mercury, which was in the left-hand column, first to pass into the dilated siphon bend at the top, and thence into the right-hand tube, where it remains, indicating on a graduated scale the exact temperature at the time it was turned over. The woodcut shows the position of the mercury *after* the instrument has been thus turned on its centre. A is the bulb ; B the outer coating or protecting cylinder ; C is the space of rarefied air, which is reduced if the outer casing be compressed ; D is a small glass plug on the principle of Negretti and Zambra's Patent Maximum Thermometer, which cuts off, in the moment of turning, the mercury in the column from that of the bulb in the tube, thereby ensuring that none but the mercury in the tube can be transferred into the indicating column ; E is an enlargement made in the bend so as to enable the mercury to pass quickly from one tube to another in revolving ; and F is the indicating tube, or thermometer proper. In its action, as soon as the thermometer is put in motion, and immediately the tube has acquired a slightly oblique position, the mercury breaks off at the point D, runs into the curved and enlarged portion E, and eventually falls into the tube F, when this tube resumes its original perpendicular position.



The contrivance for turning the thermometer over may be described as a short length of wood or metal having attached to it a small rudder or fan ; this fan is placed on a pivot in connection with a second ; on the centre of this is fixed the thermometer. The fan or rudder points upwards in its descent through the water, and necessarily reverses its position in ascending. This simple motion or half turn of the rudder gives a whole turn to the thermometer, and has been found very effective.

Various other methods may be used for turning the thermometer, such as a simple pulley with a weight which might be released on touching the bottom, or a small vertical propeller which would revolve in passing through the water.

Messrs. Negretti and Zambra in their paper merely mention the new thermometer as being available for deep-sea temperatures ; but we believe it will prove to be of great value on land ; for with this thermometer we are at once provided with the means of making observations which will solve some of the most interesting questions connected with atmospheric temperature. At present we do not possess a *simple* instrument, in fact none at all which will automatically record *out of doors* the exact temperature at fixed periods ; we read of the temperature being so many degrees of heat or cold yesterday or last night, but we have no means of recording how cold it was (say) at midnight, or how warm at midday, except by actually watching the instrument at those hours. With the new thermometer in connection with an inexpensive time-piece, we can ascertain and record the exact temperature at any hour it may be deemed desirable, and by its means, and with experiments carried over some period. of time, we may be able to determine with a degree of accuracy hitherto only approximately arrived at, which are the coldest or warmest periods of the days or nights.

*ON SOME RECENT ASTRONOMICAL SPECULATIONS IN THEIR RELATION TO GEOLOGY**

I HAVE called my subject *speculations*, because in the present state of the inquiry there are so many questions that can be looked upon in no other light. At the same time it appears to me very desirable that certain facts should be examined from this new point of view, if only to lead to researches which otherwise would not have claimed attention. What I then propose is to consider the bearing on certain geological questions of the new views of Mr. Lockyer respecting the constitution of matter, as indicated by a comparison of the spectra of the various classes of stars, and the probable effects of a change in the constitution of our sun.†

Sir W. Thomson has contended that the sun cannot have continued to give out heat and light for so long a period as has been assumed by many geologists, and has concluded that it was "on the whole most probable that it has not illuminated the earth for 100 millions of years, and almost certain that it has not done so for 500 millions."‡ Prof. Huxley made this question the subject of his address to the Geological Society in 1869, but the argument on both sides was on the supposition that the constitution of matter is such that from the earliest epoch the heat and light given off had been derived mainly, if not entirely, from the simple cooling of a heated body. If, however, Mr. Lockyer's views be true, the sun at the earliest period must have consisted of matter in a more dissociated condition than at present, and, as he points out, in combining so as to give rise to other so-called elementary substances, probably a large extra amount of heat and light would be set free. The result of this appears to me to be that when the general temperature was that at which such a dissociation occurs, the sun's energy would continue nearly the same for a period which in the present state of our knowledge cannot be determined, but which would probably be of vast duration; and not only so, but the cooling would be more uniform from the first, and not subject to so great a variation as would occur in the case of an intensely-heated body cooling without any physical change in its constituents. If this be so, the length of time during which our globe may have been receiving such an amount of heat and light as would be compatible with the existence of animals and plants may well have been as great as that demanded by any of the supporters of evolutionary theories.

Though there would be such an approximate uniformity for a vast period, yet still at the earliest epoch, the physical state of the sun would not have been the same as now, and it becomes important to consider what effect this may have produced on life on the globe. According to Mr. Lockyer's views the sun at an early period had much the same physical constitution as the stars of the type of Sirius, giving off light of a whiter or bluer character, *i.e.* the rays at the blue end of the spectrum were relatively stronger than at present, whilst in future ages they would become more feeble, and the sun pass into the condition of stars of the red type. What then would be the effect of the greater intensity of the rays at the blue end of the spectrum on animals and plants at early geological epochs? This question clearly indicates the importance of future experimental inquiries, directed to this particular subject, but at the same time it may be well to consider the bearing of what is already known. In the present state of our knowledge no facts seem more likely to help towards a conclusion than those connected with the distribution in plants of the more important of the coloured substances which absorb different rays of light. I have found that there is an intimate relation between their optical and chemical characters, and that these are also related to the development of the individual plants, and to the structural development of mature plants of different classes. Taken as a whole, in advancing from a more rudimentary condition, there is in each case a farther and farther departure from such colouring-matters as can be formed artificially, and a relatively greater and greater production of those which are more and more easily decomposed by light, when not protected by the constructive energy of the living plants. This destructive action is due relatively more to the rays at the blue end of the spectrum, whilst, at all events in the case of chlorophyll, the production depends more upon the yellow rays. Hence, by relatively increasing the intensity of the blue rays the destructive force would be relatively increased, and the constructive force relatively diminished. We may, perhaps, therefore conclude that bluer light would be relatively more favourable to the higher classes of plants when in the early stage of their growth, and to the lower than to the higher classes when in the mature condition requisite to insure permanent reproduction. The former conclusion is borne out by Mr. Robert Hunt's experiments, which showed that whilst the rays at the blue end of the spectrum quicken the germination of the higher classes of plants, it is the rays at the extreme red end which facilitate their flowering and the perfecting of the reproductive organs.* The effect of differently coloured light on the growth of the cryptogamia has not, I believe, been examined; but, if the principles involved in the above arguments be correct, they would lead us to conclude that at an early epoch in the history of our globe the bluer light of the sun would be relatively more favourable to the growth of larger cryptogams than to that of phænogams. The arguments I have used do, however, involve so many new and imperfectly-tried general principles, that it would be very premature to say that the characteristic peculiarities of the vegetation of the earlier geological periods depended on this cause, and all that I contend is that the question deserves to be examined from this new point of view, since it may at all events assist in arriving at a true explanation.

* An abstract of a paper read before the Sheffield Literary and Philosophical Society, Feb. 3, 1874, by H. C. Sorby, F.R.S.

† *Comptes Rendus*, Dec. 8, 1873.

‡ Brit. Ass. Report, 1861, p. 28.

* Brit. Ass. Report, 1843, p. 35.

THE "CHALLENGER" EXPEDITION†

II.

FERNANDO NORONHA

THIS group of islands was visited by the *Challenger* on September 1 and 2, 1873. They consist of a principal island, about four miles long, and three-and-a-half broad, stretching about N.E. and S.W., and several smaller ones at the eastern extremity, known as Platform Island, Booby Island, St. Michael's Mount, Egg Island, and Rat Island. They are situated in the Atlantic, in lat. 30° 50′, about 200 miles from the nearest point of the South American coast, their entire length being about seven miles. The principal island is generally of a volcanic character, and hilly, the highest hill being about 600 feet. On its northern coast rises to a height of 1,000 feet what is known as the Peak. It is a peculiar-looking

† These Notes are founded on letters sent home by Mr. H. N. Mosely.

mass of bare rock, the summit of which is entirely devoid of vegetation, and quite inaccessible. The cliffs are composed of columnar basalt. At the eastern end of the island are some sand rocks, like that of Bermuda, and dunes of calcareous sand also occur. St. Michael's Mount is a cone 300 feet high, composed of a mass of phonolith. The remaining islands are flat, composed of sandstone, with volcanic particles.

It was in the dry season, which extends from July to December, that the *Challenger* visited these islands ; this season, however, is not one of absolute drought, parching up everything, but there are occasional heavy rains. Trees abound on the higher parts of the island, where the land has not been cleared for cultivation, or where the convicts have not felled them for making their fishing-boats or rafts, the largest trees, it is said, having all disappeared for this purpose. Numerous creepers cluster together in the branches of the trees. At the western extremity of the island the vegetation is thickest and richest, and apparently of a virgin character. *Jatropha gossypifolia* L., a large shrubby plant, common in the West Indies, and also growing in Bahia, Mexico, and New Granada, was very abundant; it was in full flower, but its only foliage were tufts of young leaves just beneath the inflorescence, so that its bare stems were conspicuous among the green creepers. The plant was also found on St. Michael's Mount and Rat Island. Another euphorbiaceous-looking plant, with stout thorns, was found on the principal island, but not on any of the others. A thorny acacia also grew on the shore; and climbing round almost every tree was *Abrus precatorius* L., one of the commonest of tropical plants, and so well known for its pretty brilliant scarlet and black seeds, which are used everywhere for necklaces, and other ornamental purposes, and in India as a standard weight. This plant, however, grew only on the main island. *Ipomœa pes-capræ* Sw. is abundant on the sand hills, and upon it and most of the other low-growing plants, *Cuscuta americana* L., spreads amazingly. A species of *Cereus* was abundant on the cliffs, but only one grass (*Oplismenus colonus* H.B.) was found on the main island.

Trees, bushes, and creepers cover the upper part of St. Michael's Mount, which is, to a certain extent, inaccessible, and, moreover, being so small, offers no room for cultivation; therefore there is no reason to suspect that the plants found upon it are attributable to any other than a natural origin. *Capparis cynophallophora* L. grows in abundance on the summit of the mount. It is a tree with a stem 8 or 9 inches in diameter, and dark green oval lanceolate leaves. A species of Ficus with aerial roots grows in favourable spots, and there forms a tree of considerable size; one is mentioned as having a trunk 30 feet high, and 18 inches in diameter. On Rat Island the same species of Ficus was also found down near the sea level, where, instead of forming a tree, it becomes a low spreading bush, not more than 5 or 6 feet high. From the natural exposure of this island to the full force of the wind, all the plants growing here, which are mostly leguminous and euphorbiaceous, mingled with cucurbitaceous creepers, are stunted in their growth. Although shady moist places occur about St. Michael's Mount, neither on this nor on the main island were any ferns, mosses, or hepaticæ found. Lichens also are very scarce.

Among the principal cultivated fruits are bananas and melons, the latter being very plentiful, and of a peculiarly fine flavour. Grapes grow well, but are not cultivated at the present time. Sugar-cane, cassava, maize, sweet potatoes, &c. are also grown in large quantities.

Animal life is singularly scarce, two lizards being the only animals recorded from Fernando Noronha, one of which is peculiar to the island, the other being found also in North America.

NOTES

THE following intelligence with regard to the late Dr. Livingstone, sent by Dr. Kirk, appears in the *Times* of Tuesday:—"Lieut. Murphy, in a note addressed to me from M'pwapwa, a place about ten days' journey from the coast, and dated the 20th of January last, says that he was then accompanying the body, and expected to reach Bagamoio, a seaport, on or about the 14th ult. Capt. Sheffe, of the Austrian ship-of-war *Heligoland*, had proceeded to the coast, and would at once convey the body and Lieut. Murphy's party to Zanzibar on their arrival. Lieut. Cameron had set out for Ujiji to recover papers left there by Dr. Livingstone. Lieut. Murphy had been in communication with him subsequent to the death of Dr. Dillon, and was sorry to find that great difficulties impeded his onward progress, owing to the antagonism of native chiefs and the desertion of many of his followers on the road from Unyanyembe to Ujiji. Chuma, who for eight years accompanied the Doctor in his wanderings, I learn had been into Zanzibar. He seems to place the position of Dr. Livingstone's death at the north of Lake Bangweolo, on or about the 4th of May, 1873. He was probably on his way westward. A reply to the official telegram, regarding the disposal of the body on arrival, was anxiously expected."

WE learn that Mrs. Arnott, the widow of the late Dr. Neil Arnott, has written to Dr. Lyon Playfair, the member for the University of Edinburgh, offering 1,000*l*. for the promotion of Natural Philosophy in that University.

THE trustees of the late Dr. Andrew Bell, the founder of the Madras School, St. Andrews, have placed at the disposal of the Senatus of the University of St. Andrews, his native city, a considerable sum towards the endowment of an Education Chair during the present session. The Senatus has had under consideration the subject of a teacher's degree, and a programme relating to its institution has been laid before the Education Department of the Government.

AT a meeting of the Sedgwick Memorial Committee held at Cambridge on the 11th inst., Prof. Humphry in the chair, it was stated by the Treasurer that the subscriptions exceeded 10,000*l*., and that more than 7,000*l*. had been paid into the account of the fund at the several banks. The question of the site of the new Geological Museum, which is to constitute the memorial, was discussed, and the feeling of the committee was in favour of the space in front of the New Museum and Pembroke Street.

THE Italian Government has determined to send out four expeditions for the observation of the Transit of Venus, the main instrument of inquiry depended upon being the spectroscope. On the other hand, for reasons not far to seek, no spectroscopes are to be employed by the English parties. Truly "they manage these things better in France," and not only in France, but in America and Italy.

TO-NIGHT (Thursday) Mr. Dewar's lecture On Dissociation will be given before the Chemical Society.

AT the annual meeting of the trustees of the Museum of Comparative Zoology, Cambridge, U.S., held in January, a committee reported that to carry out the plan inaugurated by Prof. Agassiz, a considerably larger endowment will be necessary, and that the funds now on hand are not sufficient to conduct operations on the present scale later than April 1, after which, unless an additional income of 15,000 dols. can be secured, it will be necessary to greatly reduce the scale of work. 30,000 dols. per annum is estimated as being the least sum on which the establishment can be maintained on a satisfactory scale. Efforts are now being made to secure an endowment of 300,000 dols., of which about 65,000 had been contributed at a recent date.

THE Duke and Duchess of Edinburgh, on their visit to the Zoological Gardens on Sunday last, desired to have their attention specially directed to a deer sent from Manilla by his Royal Highness as a present. When it arrived in this country, in May 1870, Mr. Sclater, F.R.S., the Secretary to the Society, immediately recognised that it belonged to a species hitherto quite unknown, and he accordingly named it *Cervus alfredi* or Prince Alfred's Deer. It is a very interesting fact that this specimen, and one of the opposite sex subsequently purchased by the Zoological Society, together with a young one born in the Gardens, are the only examples that have at any time been obtained of this particularly well-marked species of deer.

THERE will be an election at Worcester College, Oxford, in June, to three Scholarships, one of which will be in Natural Science. Particulars can be had on application to the senior tutor. At the same time there will be an election at Magdalen College to not less than four Demyships and one Exhibition. Of the Demyships, one at least will be Mathematical, and one at least in Natural Science; the Exhibition will be in Mathematics. The stipend of the Demyships is 95*l.* per annum, and of the Exhibition 75*l.*, tenable for five years. For particulars apply to the senior tutor.

THE Board of Trade have been informed by the Meteorological Committee that they are now prepared to re-introduce the use of Admiral Fitzroy's signals (cones and drum) with slightly modified significations, and that the change will take effect on and after March 15, 1874. The signals to be used will consist of:—1°. Cone, point downwards for southerly gales: S.E. round by S. to N.W. 2°. Cone, point upwards for northerly gales: N.W. round by N. to S.E. 3°. Drum *with cone*, to indicate the probable approach of a *very heavy gale* from the direction indicated by the cone. The drum will not be used without the cone. The signals are to be kept hoisted *during the daylight only*, until 48 hours have elapsed from the time *the telegram was despatched*, unless countermanded. At night lanterns may be used wherever the local authorities deem it advisable to do so, as pointed out in an explanatory pamphlet, copies of which are supplied for gratuitous distribution. It will be seen from the pamphlet in question, that the meaning of the signals is that an atmospherical disturbance exists (which will be explained in the telegram) and will probably, but not *necessarily*, cause a gale at the place warned *from the direction* indicated by the signal. The Meteorological Office will supply the canvas shapes and lanterns to such places as require them, on loan; but in all cases the local authorities must undertake the charges incidental to the hoisting of the signal, such as flagstaff and gear, oil, &c., and also to the keeping of the apparatus in repair, painting, &c.

M. G. TISSANDIER, the editor of *La Nature*, is completing a series of observations for calculating the amount of atmospheric dust falling each day. The mean found is said to be several pounds in twelve hours for a surface not larger than the Champ de Mars, rather less than a half a square mile.

THIRTY-SEVEN small planets have been discovered in the years 1872 and 1873, or 18½ for each year, making 1,850 per century. From the days of Hipparchus to the present time we may reckon 2,000 years; had astronomers worked with the same zeal and success during these 2,000 years, the number of small planets known would have amounted to 37,000, only three times the number given by Arago of stars up to the 7th magnitude, and a very small proportion of the stars of the 10th magnitude. Although very minute, the latter are generally much brighter than small planets as seen at the time of opposition.

PROF. O. C. MARSH has made out some interesting points in connection with the remains of equine forms in the North American tertiaries. Following up the genealogy of the horse, as traced by Prof. Huxley in the European remains, he has been able to show that the American deposits present even a more complete series of intermediate forms. Between *Orohippus agilis* of the Eocene, which was about the size of a fox, and had four toes on the fore foot, with three behind, and *Equus fraternus* of the Pleistocene, which is not osteologically distinguishable from the existing *Equus caballus*, the following genera form the connecting links in form, size, and antiquity, viz. *Miohippus* and *Anchitherium* of the Miocene, and *Anchippus*, *Hipparion*, *Protohippus* and *Pliohippus* of the Pliocene. "Considering the remarkable development of the group through the entire Tertiary period, and its existence even later, it seems very strange that none of the species should have survived, and that we are indebted for our present horse to the old world."

IN continuation of his exquisite researches on the phenomenon of flight (*Comptes Rendus*, January 12, 1874), M. Marey has made a series of observations which prove how important a part the onward movement of a bird plays in increasing the efficiency of each wing stroke. For supposing that in its descent the wing did not continually come in contact with a fresh volume of air, it would act at a disadvantage, because the downward impulse which, at the commencement of each stroke, it gives to the air below it, would make that air so much less efficient a resisting medium; whilst, by continually coming in contact with a fresh body of air, the wing is always acting on it to the best advantage. For this reason, when a bird commences its flight, it turns towards the wind if possible, to make up for its lack of motion on starting.

THE extension of the Cinchona cultivation in Darjeeling continues. Every year additional land is brought under Cinchona culture, and it is calculated that 2,000 acres more will be cleared and planted within the next four years. With regard to Ipecacuanha, upwards of 20,000 plants and cuttings are now in hand, all of which promise well. Another interesting fact relating to the introduction of useful plants into India, is that of the success in the Terai of the Cacao (*Theobroma cacao* L.). The plants that were planted out about a year ago, were sent from Kew at the suggestion of Dr. Hooker, and they are now in a most healthy and satisfactory condition.

THE French Academy has at last published the list of candidates for the seat rendered vacant by the death of the late Dr. Nelaton. The issue is quite uncertain. M. Broca, the celebrated anatomist, has obtained only a second rank, and M. Marey is placed in the third.

FATHER SECCHI is preparing, at Gauthier Villars, a second edition of his work on the Sun, on an enlarged scale. He has quoted so largely from Mr. Lockyer's "Solar Physics" that an intended translation of this work is abandoned for the present.

QUITE a sensation was produced in the last sitting of the Academie des Sciences, by the exhibition of photographs of Spitzbergen scenery, sent by Prof. Nordenskiöld. One of these represented a meteorite nearly 18 tons in weight.

A BELGIAN paper describes an immense petrified trunk of a conifer discovered in the province of Lineburg in perfect preservation. Its length is about 33 feet, and its diameter about 20 inches.

THE *Annuaire* of the Bureau des longitudes for 1874 has been recently published. Although sold at 1½ fr., it contains chromolithographs showing solar protuberances, and an essay by M. Faye on questions relating to the sun.

THE widow of General Poncelet, one of the most distinguished French military engineers, has written to the French Institute, announcing that the whole of the works of her late husband will soon be in the hands of the public. The last volume, the sixth of the series, containing the lectures delivered at the Metz School of Artillery and the École Polytechnique, will be issued shortly. It is edited by M. Krœtz, Chief Engineer of the National Manufactures, one of the General's pupils. General Poncelet was a member of the French Institute for more than twenty years. He died in 1864.

A MEMOIR upon the embryology of *Terebratulina*, by Prof. Morse, has just been published by the Boston (U.S.) Society of Natural History, this being the result of a thorough investigation in regard to the development of this genus of the brachiopod shells found so abundantly on the coast of Maine. Prof. Morse's labours were mainly prosecuted at Eastport, and extended through a period of several years. He found that the species spawns throughout the entire summer season (at least from April to August), but that investigations in the earlier part of the season were preferable, since, with the increasing warmth, the development is more rapid than is convenient to the observer.

THE "Fenland Meteorological Circular and Weather Report" is the name of a monthly periodical, two numbers of which, for February and March, have just reached us. It is edited by Mr. S. H. Miller, F.R.A.S. The circular is intended to be "a medium for local meteorologists, an abiding record of the climate of the Fenland, and a register of the changes of the weather, to which the agriculturist, horticulturist, and naturalist may easily refer." It is intended besides, we believe, that the "Circular" should fill to some extent an educational function, and induce agriculturists to take an interest in the sciences with which their art is so intimately connected. This purpose is to be served by the publication of readable articles on scientific subjects, the first of which is by Mr. S. B. J. Skertchly, F.G.S., on the "Practical Bearings of Meteorology." The two numbers sent us contain well-constructed tabular and graphic reports of the meteorology of Wisbech for January and February, besides district reports, and a number of notes and short articles on subjects more or less intimately connected with the department to which the "Circular" belongs; the paper and typography are all that can be desired, and the price is only 2*d*. per month. The enterprise is highly creditable to its originators, and we sincerely hope it will be a great success and attain a wide circulation in the important district for whose benefit it has been started; and we hope the example thus set will be followed by other districts in the kingdom. The publishers of the "Circular" are Leach and Son, Wisbech.

A BILL has been introduced into the U.S. Congress proposing an appropriation of 7,000 dols. to enable the Department of Agriculture to make a collection of all the species of trees growing throughout the United States, and for their exhibition in suitable cases. The collection itself, when completed, is to be exhibited at the Philadelphia Centennial Exposition, but to belong to the Agricultural Department, and to be returned to it.

AT the meeting of the French Academy of March 2, M. Charles Sainte-Claire Deville, gave an account of his meteorological mission to Biskra and Tuggurt. At each of these stations an observatory has been established similar to that at Montsouris. Captain Roget superintends the observatory of Biskra, and Dr. Audet that of Tuggurt; the latter, according to M. Deville, is in an almost perfect situation, surrounded by a sufficient quantity of vegetation, and preserved from the effects of radiation. The results comprehend already observations continued during the month of January, and M. Deville sees in them the promise of important discoveries.

WE have received a useful work lately published by the Smithsonian Institution, prepared by Prof. F. W. Clark, of Howard University, Washington. This is the first part of a series entitled the "Constants of Nature," and gives in tabular form the specific gravities, the boiling and melting points, and the chemical formulæ of a large number of substances, with indications of the authorities whence the facts are derived. The volume contains about 250 pages, and is well provided with the necessary indices.

LIEUTENANT G. M. WHEELER (United States Engineers) and party are in Washington, and are engaged in elaborating the results of their explorations in 1873. Already about 76,000 square miles of territory have been carefully gone over and topographed with a view to their representation in the new series f index maps now nearly ready for publication. Much valuable information in regard to the geology of the country, its mining facilities, and the probabilities of successful irrigation, has been obtained, and will be duly published. In the line of natural history the collections have been very large, embracing at least 1,200 skins of birds, besides many hundreds of reptiles, fish, insects, &c. The botanical collection is said to be the finest and largest ever procured by a Government expedition. The results of the woik of the expedition for the years 1871, 1872, and 1873 are, we understand, shortly to be published, comprising seven large quarto volumes, which will prove a valuable addition to the scientific history of the great West.

WE are glad to see, from Part IV. of the Transactions of the Clifton College Scientific Society, that that Society is exceedingly prosperous, so far as number and attendance of members is concerned, though we very much regret to learn that, like not a few other similar Societies, it contains but a small number of real workers. We hope that the patrons and office-bearers of the Society will do their best to devise some means to remedy this very serious defect, for serious the defect is, seeing that one of the main objects of such a Society is to train its members to methodical work, careful observation, and independent thinking. The only paper in this part, by a student member of the Society, is a long and elaborate one on Potteries, by C. C. Stevenson.

A THICK supplementary number of Petermann's *Mittheilungen* has been published containing a vast number of statistics relating to the population of the globe, with two excellent illustrative maps.

AT the meeting of the Adelaide Philosophical Society held on Nov. 25, 1873, Dr. Schomburgk read the second part of a long paper on poisonous plants. The plants noticed were those of the genus *Strychnos nux vomica*, *Datura stramonium* (the thorn-apple), *Solanum nigrum*, and *Taxus baccata* (the yew tree).

WE learn, from a Reuter's telegram, that the bank, sixty miles in length, formed for a long time past in the White Nile south of the province of Secionda, and mentioned by Sir Samuel Baker as an obstacle to navigation, has been partially removed by the works ordered by the Soudan Government. The river is now navigable up to Kondokaro over a distance of ten and a half degrees. The works for the complete removal of the bank continue.

THE additions to the Zoological Society's Gardens during the past week include a Finsch's Amazon (*Chrysotis finschi*) from W. Mexico, presented by Mr. C. Chivers; a Virginian Deer (*Cervus virginianus*) from N. America, presented by Mr. N. M. Bateson; a Cornish Chough (*Fregilus graculus*), British, presented by Mr. J. T. Hewes; a Common Otter (*Lutra vulgaris*), British, presented by Dr. Stafford; a Blue and Yellow Macaw (*Ara ararauna*) from S. America, presented by Miss J. Staines.

*THE GASEOUS, LIQUID, AND SOLID STATES OF WATER-SUBSTANCE**

IN two communications made by me to the British Association at its meetings at Edinburgh in 1871, and at Brighton in 1872, and printed as abstracts in the Transactions of the Sections for those years, considerations were adduced on relations between the gaseous, the liquid, and the solid states of matter. The new subject of the present paper constitutes a further development of some of those previous considerations, and a brief sketch of these is necessary here as an introduction for rendering intelligible what is to follow.

Taking into consideration any substance which we can have in the three states, gaseous, liquid, and solid, we may observe that when any two of these states are present in contact together, the pressure and temperature are dependent each on the other, so that when one is given the other is fixed. Then if we denote geometrically all possible points of temperature and pressure jointly by points spread continuously in a plane surface, each point in the plane being referred to two axes of rectangular co-ordinates, so that one of its ordinates shall represent the temperature and the other the pressure denoted by that point, we may notice that there will be three curves, one expressing the relation between temperature and pressure for gas with liquid, another expressing that for gas with solid, and another expressing that for liquid with solid. These three curves, it appears, must all meet or cross each other in one point of pressure and temperature jointly, which may be called the triple-point. The triple-point, considered in respect to its temperature, is in fact what would often be called the freezing point *in vacuo*; that is, the freezing temperature of water in contact with no gas except its own aqueous vapour or steam; and the pressure at the triple point is just the pressure of that aqueous gas or steam.

The curve between gas and liquid, which may be called the *boiling-line*, will be a separating boundary between the regions of the plane corresponding to the ordinary liquid and those corresponding to the ordinary gaseous state. But by consideration of Dr. Andrews's experimental results ("Phil. Trans.," 1869) we may see that this separating boundary comes to an end at a point of temperature and pressure which, in conformity with his language, may be called the *critical point* of pressure and temperature jointly; and we may see that, from any liquid state to any gaseous state, the transition may be gradually effected by an infinite variety of courses passing round the extreme end of the boiling-line.

The accompanying figure serves to illustrate these considerations in reference to transitions between the three states, the gaseous, the liquid, and the solid. The figure is intended only as a sketch to illustrate principles, and is not drawn according to measurements for any particular substance, though the main features of the curves shown in it are meant to relate in a general way to the substance of water, steam, and ice. A X and A Y are the axes of coordinates for the temperatures and pressures respectively; A, the origin, being taken as the zero for pressures and as the zero for temperatures on the Centigrade scale. The curve L represents the *boiling-line* terminating in the critical point E. The line T M represents the line between liquid and solid. It is drawn showing in an exaggerated degree the lowering of the freezing temperature of water by pressure; the exaggeration being necessary to allow small changes of temperature to be perceptible in the diagram. The line T N represents the line between the gaseous and the solid states of water-substance. The line L T N appears to have been generally (in the discussion of experimental results on the pressure of aqueous vapour above and below the freezing-point) regarded as one continuous curve; but it was part of my object in the two British-Association papers referred to, to show that it ought to be considered as two distinct curves (L T P and N T Q) crossing each other in the triple-point T.

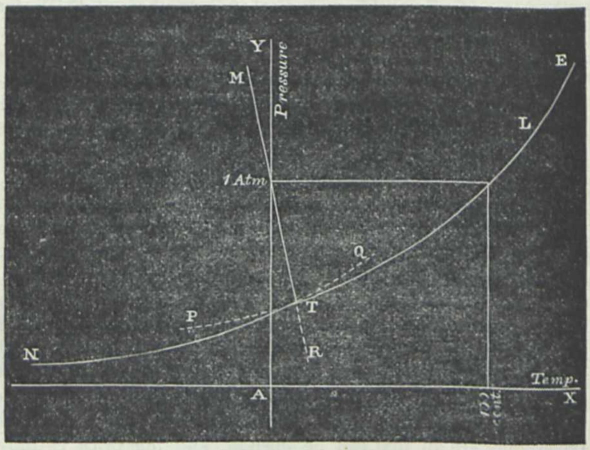
In the second of the two British-Association papers already referred to (the one read at the Brighton meeting, 1872), I gave demonstrations showing that these two curves L T and N T should meet, as shown in the accompanying figure, with a re-entrant angle at T, not with a salient angle such as is exemplified in the vertex of a pointed arch; and offered in conclusion the suggestion that the reasoning which had been adduced might be followed up by a quantitative calculation founded on experimental data, by which calculation the difference of the pressures of steam with water, and steam with ice for any given temperature very near the triple point, may be found with a very close approximation to the truth.

In the month of October 1872 I explained to my brother, Sir William Thomson, the nature of that contemplated quantitative calculation: I mentioned to him the method which I had prepared for carrying out the intended investigation, and inquired of him for some of the experimental data, or data already deduced by theory from experiments, which I was seeking to obtain. On his attention being thus turned to the matter, he noticed that the desired quantitative relation could be obtained very directly and easily from a simple formula which he had given in his paper on the Dynamical Theory of Heat, "Transactions of the Royal Society of Edinburgh," March 17, 1851, § 21 (3), to express the second law of thermodynamics for a body of uniform temperature throughout, exposed to pressure equal in all directions.

That formula is

$$\frac{dp}{dt} = \mathrm{C\,M}$$

in which p denotes the amount of the pressure, and $\frac{dp}{dt}$ its rate of increase per unit increase of temperature, the volume being kept constant; C denotes Carnot's function; and M denotes the rate of absorption at which heat must be supplied to



the substance per unit augmentation of volume, to let it expand without varying in temperature. The body may be either homogeneous throughout, as a continuous solid, or liquid, or gas; or it may be heterogeneous, as a mass of water and aqueous vapour (*i.e.* steam), or ice and water, or ice and aqueous vapour (*i.e.* steam).

Now apply that formula, 1st, to steam with water, and 2nd, to steam with ice, the temperature of the heterogeneous body in each case being that of the triple-point, or we may for the present purpose say 0° C., which is almost exactly the same. It is to be observed that while in the general application of the formula the rate of increase of the pressure with increase of temperature, *when the volume is kept constant*, has been denoted by $\frac{dp}{dt}$, yet in each of the two particular cases now brought under consideration it is a matter of indifference whether the volume be kept constant or not; because the pressure of steam in contact either with water or with ice, for any given temperature, is independent of the volume of the whole heterogeneous body; so that the change of pressure for change of temperature is independent of whether there be change of volume or not. As C is a function of the temperature which has the same value for all substances at the same temperature, it has the same value for the two cases now under consideration. Hence, retaining for the first case (that, namely, of steam with water) the same notation as before, but modifying it by the use of an accent where distinction is necessary in the second case (that of steam with ice), and thus using $\frac{dp}{dt}$ to denote the rate of increase of the pressure per unit increase of temperature for steam with wate

* "A Quantitative Investigation of certain Relations between the Gaseous, the Liquid, and the Solid States of Water-Substance." By Prof. James Thomson, LL.D., lately of Queen's College, Belfast, now of the University of Glasgow. Communicated to the Royal Society by Sir William Thomson, LL.D., F.R.S. Abridged for NATURE by the Author.

at the triple-point (0° C. nearly), and M to denote the rate of absorption at which heat must be supplied to a body consisting of steam and water at the triple point, per unit augmentation of volume of that whole heterogeneous body, to let it expand without varying in temperature, and using $\frac{dp'}{dt}$ and M′ to denote the corresponding rates for steam with ice at the triple point, we have

$$\frac{\frac{dp}{dt}}{\frac{dp'}{dt}} = \frac{M}{M'}$$

The latent heat of evaporation of one pound of water at the freezing-point (or at the triple point) into steam at the same temperature, as determined by Regnault, is 606·5 thermic units, the thermic unit being here taken as the heat which would raise the temperature of 1 lb. of water 1° C., and the latent heat of fusion of ice is about 78 or 79 of the same thermic units. Hence, though M and M′ belong each to a cubic foot of steam at the triple point, not to a pound mass of it, still the ratio $\frac{M}{M'}$, is = $\frac{606}{79 + 606}$

Hence

$$\frac{\frac{dp}{dt}}{\frac{dp'}{dt}} = \frac{606}{79 + 606} = \frac{1}{1 \cdot 13}$$

This shows that for any small descent in temperature from the triple point (where the pressure of steam with ice is the same as that of steam with water), the pressure of steam with ice falls off 1·13 times as much as does the pressure of steam with water.

In submitting the quantitative calculation now given, I have preferred to adopt the method proposed and developed by my brother rather than that which I had myself previously devised, because his method is simpler, and brings out the results more briefly by established principles from existing experimental data. I may say, however, that the method devised by myself was also a true method, and that I have since worked it out to its numerical results, and have found that these are quite in accordance with those brought out by my brother. The two indeed may be regarded as being essentially of the same nature; and I think it unnecessary to occupy space by giving any details of the method I planned and have carried out. Its general character may be sufficiently gathered from the concluding passages of the British Association 1872 paper, as printed in the Transactions of the Sections, Brighton Meeting.

In order to discover whether the feature now developed by theoretical considerations is to be found showing itself in any degree in the experimental results of Regnault on the pressures of steam at different temperatures *, I have made careful examinations of his engraved curve (Plate viii. of his Memoir), and of his empirical formulæ adapted to fit very closely to the results exhibited in that curve, and of his final tables of results at the close of his Memoir; and by every mode of scrutiny which I have brought to bear on the subject—in fact by each of some seven or eight varied modes—I have met with clear indication of the existence of the expected feature; and by some of them I have found that it can readily be brought prominently into notice. The engraved curve drawn on the copperplate by Regnault himself is offered by him as the definitive expression of his experiments, as being an expression which satisfies as well as possible the aggregate of his observations; subject, however, to a very slight alteration, which he has pointed out as a requisite amendment in the part of the curve immediately below the freezing-point, a part with which the investigations in the present paper are specially concerned.

After telling (p. 581 of his Memoir) of the great care with which he had marked the curve on the copperplate and got it engraved, he says:—"Je n'ai pas pu éviter cependant quelques petites irrégularités dans les courbes; mais une seule de ces irrégularités me paraît assez importante pour devoir être signalée. Elle se présente pour les basses températures comprises entre 0° et − 16°, la courbe creuse trop vers l'axe des températures, elle laisse, notablement au-dessus d'elle, toutes les déterminations expérimentales qui ont été faites entre 0° et − 10°. Ainsi les valeurs, que cette petite portion de la courbe donne pour les forces élastiques, sont un peu trop faibles, et j'ai eu soin de les augmenter, de la quantité convenable, dans les nombres que je donnerai plus loin." Whether we are now to think that this bend downwards* of the curve towards the axis of temperatures involving what Regnault regarded as a small faulty departure of his drawn curve from his actual experiments, was introduced merely by a casual want of accuracy in drawing, or whether we may suppose that possibly there may have been some experimental observations which attracted the curve downwards, but were afterwards rejected on a supposition of their being untrustworthy, it appears that such a bend is a feature which the curve really ought to possess, and is one which, even after being partially smoothed off by way of correction, is not obliterated, but still remains clearly discoverable in the final numerical tables of results.

This is best brought to light by means of the empirical formulæ devised and employed by Regnault for the collating of his results. He proceeded evidently under the idea of the curve being continuous in its nature, so that a single formula might represent the pressures of aqueous vapour throughout the whole of his experiments; but before seeking for such a formula he proceeded to calculate several local formulæ of which each should represent very exactly his experiments between limits of temperature not wide apart; and afterwards he worked out several general formulæ, each adapted singly for the whole range of his experiments.

Now in the paper communicated to the Royal Society, and printed in the Proceedings for December 11, 1873, from which the present paper is an abridgment, the details of a scrutiny of the chief of these formulæ are given (the formulæ, especially, which were adopted by Regnault for deducing his final general table extending from − 32° C. to + 230° C.), from which it appears that they present clear indications that at and very close to the freezing-point (or rather the triple-point) the rate of increase of pressure for increase of temperature is decidedly less in the case of steam with water than in that of steam with ice; or, in other words, that at and very close to the triple-point the steepness of the curve for steam with water is decidedly less than that of the curve for steam with ice; or, to state the same a little more fully, that while the steepness is increasing as we ascend from temperatures below the triple-point up to the triple-point, with ice in contact with steam, there is a sudden abatement of the steepness in passing the triple-point, where the change occurs from steam with ice to steam with water, after which, with continued rise of temperature, the steepness goes on again increasing. In fact the result comes out that these formulæ, expressing an aggregate of experimental results of Regnault, would indicate for $\frac{\frac{dp}{dt}}{\frac{dp'}{dt}}$ at the freezing-point, or the triple-point, not the value 1 (as would be the case if a curve continuous past the triple-point would express the pressure of steam or aqueous vapour, for different temperatures, in contact with ice below the triple-point and with water above it), but 1·09 or 1·10, which makes a near approach to the result 1·13 found by my brother's quantitative calculation already here cited. The decimal fractions in excess of unity, here represent the quantitative relation between the greatness of the feature under consideration as brought out by the theoretical investigation on the one hand, and as deduced from Regnault's results on the other hand: and thus we may say that this feature can be brought to view as existing in Regnault's principal results in a degree about $\frac{9}{13}$ or $\frac{10}{13}$ of that in which the theoretical investigation shows that it must really exist, and ought to be found experimentally, if the experiments had a sufficiently minute exactitude to detect it and to measure it.

Regnault also gives, in the same Memoir, another statement of results deduced from his experiments, and put in the form of a table intended chiefly for meteorological purposes; which table shows the pressures of aqueous vapour for temperatures ranging below and above the freezing-point at very small intervals of temperature, $\frac{1}{10}$th of a degree centigrade each. In this table, the numbers inserted as representing the pressures below the freezing-point are slightly different from the corresponding ones in his general table, which, with the formulæ used in making it, has just now been referred to; and he mentions that this slight

* Regnault, "Des forces elastiques de la vapeur d'eau aux différentes températures," Mémoires de l'Académie des Sciences, 1847.

* In M. Regnault's curve the temperatures are measured horizontally across the sheet and pressures are measured upwards.

discrepance has resulted from the fact that the two tables were formed at different periods, and were not calculated by the same formula; but he remarks that the differences are insignificant, as they scarcely amount to ·02 millimetre of mercury in the pressures which the two tables respectively show. Here, too, as in the general table, the feature expected shows itself, though in a diminished degree. By careful examination of the minute changes of pressure for small changes of temperature, close to the freezing-point, both above and below it, as they are shown in this table, I find that the experimental results as here offered would indicate the existence of the feature in a degree about $\frac{5}{13}$ or $\frac{6}{13}$ of that in which the theoretical investigation now shows that it ought to be met with, if experiments could be made on pressures of aqueous vapour, above and below the freezing-point, with sufficiently minute exactitude.

It is indeed a great credit to the accuracy of Regnault's experiments, and to the exactitude of his results, that the results contain the clear indications they do of this feature, which only comes to view through comparison of differences of pressure represented by very minute fractions of a millimetre of mercury; and which, unless a very high order of accuracy were maintained, might have given no perceptible indication of its existence, or might readily have been made to disappear totally from the final results, through the application of the ordinary methods for clearing off small errors of observation.

SCIENTIFIC SERIALS

American Journal of Science and Arts, February 1874.—This number commences with a paper by Mr. Langley (accompanied with plate), describing studies on the minute structure of the solar photosphere, made at the Alleghany Observatory. The equatorial used had an aperture of 13 inches. The author finds that the ultimate visible constituents of the photosphere are not the "rice grains," but small *granules* composing them, and not more than 0″·3 in size. Comparing the total area covered by them with that of the whole sun, he estimates that the greater part of the solar light comes from an area of not over one-fifth of its visible surface, and which may be indefinitely less. Hence the received estimates of the intensity of the action to which solar light is due must, he thinks, be greatly increased. In the penumbra there are not only numerous small cyclones, and even right- and left-handed whirls in the same spot, but probably currents ascending nearly vertically. The action of superposed approximately horizontal currents is a prominent feature. The outer penumbral edge seems to be formed by rupture. Mr. Langley accepts M. Faye's theory as the most probable.—Prof. Pickering communicates some measurements of the polarisation of light reflected by the sky, and by one or more plates of glass. One remarkable result arrived at was, that the polarisation (from the sky) is the same, for a given solar distance, for any meridian distance; in other words, that the polarisation is the same for all points equally distant from the sun.—In a translated paper on the dissipation of electricity in gases, by a Russian physicist, M. Boboulieff, the author concludes from his experiments, that the dissipation in air (and other gases) diminishes with diminution of the pressure; and that the dissipation in hydrogen is less than in air (at the same pressure).—Mr. Verrill continues his notes on results of recent dredging expeditions on the coast of New England.—In the "Scientific Intelligence" we find a summary of a recent important memoir by Prof. Morse on the systematic position of the Brachiopoda. His avowed object is to show that in every point of their structure the Brachiopoda are true worms, with possibly some affinities to the Crustacea, and that they have no relations to the Mollusca, save what many other worms may possess in common with them.—The organisation of an American Metrological Society is announced; the design being to originate and promote measures for improving the system of weights, measures, and moneys, and bring these into relations of simple commensurability with each other.

IN the number of the *Bulletin Mensuel de la Société d'Acclimatation de Paris* issued in January one of the principal papers is an article by M. Mérice on Agriculture in Brazil. This country is by nature fitted to be one of the most prolific agricultural tracts of earth in the world, so varied and abundant are its productions, and so fruitful its soil. Improved implements are necessary, and increased skill would give greatly increased results. Great strides are being taken in the "education" of various kinds of silkworms, and under the directions given in the *Bulletin* the different varieties may be properly reared on the most approved principles.—M. Carbonnier, to whom the honour is due of introducing the "Paradise fish" from China, has been successful in importing some live specimens of another species of *Macropodus*, the "rainbow-fish" of India: specimens of the *Pesca-re*, or "king-fish" of South America have also been brought to Paris from Buenos Ayres. M. E. Perris' paper on birds and insects is continued.—The recent transmission of salmon ova to New Zealand, the cultivation of lobsters in the United States, the introduction of the *Eucalyptus* in various parts of Europe, are all, amid a mass of other matter, referred to at length. The number for January, just received, gives an interesting account of the year's work at the *Jardin d'Acclimatation*. In the Bois de Boulogne experiments have been made in the cultivation of various vegetables and other useful plants, on the results of which the future utilisation of new importations greatly depends.—The notes on the cultivation of the vine and on the use of mineral manures will be found very valuable.—The question of fish culture, which has assumed such large proportions in England and America, is being taken up by the Society, and an interesting paper on the subject is contributed by M. de la Blancheu.

Reale Istituto Lombardo di Scienze e Lettere. Rendiconti: Dec. 18, 173. In this number Prof. Lombroso furnishes exact measurements of the crania of 66 Italian criminals (from various museum collections) along with an interesting analysis of those data. With reference to capacity as measured by apparent circumference, while there were a few of pretty large circumference (1 of 580, 1 of 550, 2 of 560, 2 of 540, out of 65), and a moderate number of ordinary size (8 of 530, 13 of 520), a large proportion were quasi-microcephalic (39 out of 65); and precisely 19 of 510, 8 of 490, and 12 of 500. Of 40 crania examined the mean capacity, in cubic centimetres, was 1,389; two were of more than ordinary capacity (1,610 and 1,633), 3 were of ordinary (1,500 and more), 12 had a capacity of 1,400 and more, 19 were of much inferior capacity (1,300 and more), while 4 were truly microcephalic, with a capacity of 1,100 to 1,200. Of the last, 2 were crania of assassins, 1 that of a thief, and 1 that of an incendiary (with intent to rob). Prof. Cantoni gives the concluding portion of a valuable paper in experimental physics, on the polarisation of non-conductors. He here takes up several objections urged by Prof. Righi to his opinion as to the possibility of polarising a non-conductor durably.—M. Corradi continues an historical sketch of the study and teaching of anatomy in Italy in the Middle Ages.—We also note short papers on primitive tumours of the dura mater (by M. Bizzozero), and on a remarkable appearance of the zodiacal light, and a shower of falling stars observed in some parts of Italy on December 12 last.

Bulletin de l'Academie Royale des Sciences (*de Belgique*). No. 12. In this number are given a series of papers or lectures of a somewhat popular nature, read at the public seance in December. The first is by M. Gluge, who advocates the teaching of biology in the Belgian schools.—M. d'Omalius d'Halloy follows with an argument for the hypothesis of transformation by generations of forms from a first creation, as against that of successive creations on the one hand, and that of evolution from matter on the other.—The next lecture, by M. van Beneden, is entitled "A Word on the Social Life of the Inferior Animals," and gives some curious facts in natural history, relating especially to parasites.—And lastly, we have an able lecture by M. Schwann, on the commencement and the end of the world, according to the mechanical theory of heat. The author gives a lucid exposition of the two fundamental principles of conservation and dissipation of energy, explaining, with special fulness, the doctrine enunciated by Clausius and the considerations leading to it.—The *Bulletin* further contains reports by members of the Academy on various prize-competitions. One subject proposed was the relation of heat to development of phanerogamic plants, with special reference to the periodic phenomena of vegetation. The Committee give lengthy analyses of the memoir received, to which, while of some scientific merit, the prize is not awarded. In a lecture by M. Morreu on the subject, the law is elucidated, that, other things being equal, the quantity of carbon fixed by a vegetation is greater in proportion as its height is less, inasmuch as this supposes a less expenditure of movement.—Another question called for a description of the coal system of the Liége Valley. From a memoir on which the committee report favourably, it appears that instead of having twenty-five beds of coal, as had been thought since the labours of Dumont, the district referred to contains scarcely half that number.—Several mathematical notes are given, and there is a description of three additions to the synopsis of Calopterygines, by M. Longchamps.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, March 12.—"Contributions to the Developmental History of the Mollusca, Sections I., II., III., IV." By E. Ray Lankester, M.A., Fellow of Exeter College, Oxford. Communicated by G. Rolleston, F.R.S., Linacre Professor of Anatomy and Physiology in the University of Oxford.

The points of greatest interest to which the author draws attention in the present memoir are :—

1. The explanation of the basket-work structure of the surface of the ovarian egg, by the plication of the inner egg-capsule.
2. The increase of the yelk by the inception of cells proliferated from the inner egg-capsule.
3. The homogeneous condition of the egg at fertilisation.
4. The limitation of yelk-cleavage to the cleavage-patch.
5. The occurrence of independently-formed corpuscles (the autoplasts) which take part in the formation of the blastoderm.
6. The primitive eye-chamber formed by the rising up of an oval wall, and its growing together so as to form a roof to the chamber.
7. The origin of the otocysts by invagination.
8. The rythmic contractility of a part of the wall of the yelk-sac.
9. The disappearance of the primitive mouth, and the development of a secondary mouth.
10. The development of a pair of large nerve-ganglia by invagination of the epiblast immediately below the primitive eye-chambers.

"The Localisation of Function in the Brain," by David Ferrier, M.A., M.R.C.P., Professor of Forensic Medicine, King's College, London. Communicated by J. Burdon Sanderson, F.R.S., Professor of Practical Physiology in University College.

The chief contents of this paper are the results of an experimental investigation tending to prove that there is a localisation of function in special regions of the cerebral hemispheres.

Anthropological Institute, March 10.—Prof. George Busk, F.R.S., president, in the chair. A paper, by Dr. A. P. Reid, was read, "On the mixed or half-breed Races of North-Western Canada." The mixed races were nine in number, viz. the progeny of (1) the Anglo-Saxon father and Indian mother, (2) the French and French-Canadian father and Indian mother, (3) the Anglo-Saxon father and mixed Anglo-Saxon and Indian mother, (4) the French father and mixed French and Indian mother, (5) the "half-breed" Anglo-Saxon and Indian as father and mother, (6) the "half-breed" French and Indian as father and mother, (7) the descendants proceeding from intermarriage of 5th class, (8) the descendants proceeding from intermarriage of 6th class, (9) the mixed or "half-breed" father and Indian mother. Those nine divisions included the principal mass of the mixed peoples of Manitoba. The French and Anglo-Saxons and their descendants rarely intermarried. The author pointed out the marked change in physique, which was common to all the classes he had enumerated, that quickly followed the removal of Europeans to American soil. The complexion becomes swarthier and more nearly resembling the type of native Americans than one would suppose. That change was due to climatic influences, to different food, and to altered customs. On the whole, there was a tendency, in all the mixed races, to the Indian rather than to the European type. They could not be said to possess any objectionable peculiarities ; they were not more inclined to the abuse of alcohol or to other irregularities than the pure whites ; and it would be difficult to find a people who have fewer faults. Some of the families of the pure white and pure Indian were often very numerous, sometimes reaching the number of fifteen ; but four to six was the average.—A paper, by the Rev. George Taplin was read, "On the mixed races of Australia and their migrations." The author's deductions were made chiefly from linguistic data. He however recorded the fact of having met with some individuals of the Narrinyeri tribe who had light complexions and straight hair. He found also that among the Narrinyeri there were superstitions and customs identical, even in name, with those obtaining among the Samoans.—Commander Telfer, R.N., communicated notes on the discovery of burial grounds near Tiflis in Georgia. In one of the graves were found parts of a body that had undoubtedly been interred in a sitting posture. The skull of an adult was remarkably distorted, and bore a striking resemblance to the longest form of the Titicacan skulls of South America.—A paper by Miss A. W. Buckland "On the Serpent, in connection with primitive metallurgy," was also read.

Royal Horticultural Society, March 4.—General Meeting.—Lieut.-Gen. Hon. Sir A. H. Gordon, K.C.B., in the chair.—The Rev. M. J. Berkeley commented on some of the plants shown. They included *Encholirion corallinum*, a curious Bromeliaceous plant from Brazil, a fine species of Medinilla probably new, and the beautiful *Iris reticulata*, which, though a native of Persia, proved quite hardy in this country.

Scientific Committee.—Dr. Hooker, C.B., P.R.S., in the chair.—The Rev. M. J. Berkeley called attention to the following communication made by Prof. Panceri to the Institut Egyptien at its meeting on December 13, on Cryptogamic vegetation found within the egg of an ostrich, which was interesting in connection with what he had himself brought before the committee on March 5 and 19, 1873. The egg had been given Prof. Panceri at Cairo, and was still fresh, the air space having not even been formed. He soon, however, noticed the appearance of dark blotches within the shell, and having broken it open to ascertain the cause, he found that they were produced by the growth of minute fungi. Instances of a similar kind had already been studied by him, and he had communicated the results to the Botanical Congress held at Lugano in 1859. The Rev. M. J. Berkeley had found *Cladosporium herbarum* in the interior of a fowl's egg.—Dr. Masters brought shoots of *Picea nobilis*, in which the primary shoot was dead and swollen beneath the apex. In many instances he had found similar excrescences to contain the larva of an insect. In other cases the primary cause of injury appeared to be frost or cutting east winds.—Dr. Masters exhibited some peas which had been attacked by a beetle (*Bruchus pisi*) which fed on the cotyledonary portion, but left the plumule, so that the seeds still germinated.—Dr. Masters reported on the monstrous Cyclamen which had been referred to him at the last meeting. The apparent corolline whorl in the Primulaceæ is now regarded as an outgrowth from the andrœcium. In the present case there appeared to be a supplementary staminal whorl alternating with the normal one, and therefore with its members opposite to the sepals. These members, however, had become partially petaloid, and were rolled up, so that the whole flower had a superficial resemblance to a case of lateral prolification.—Mr. Grote stated that Mr. F. Moore, of the Indian Museum, agrees with Prof. Westwood, and refers the Assam Tea-bug to the genus *Helopeltis* of the Capsidæ. A Ceylon species of this bug is figured by Signoret in the *Ann. Soc. Entom. de France*, 3rd series, pl. 12, fig. 2. Two other species are known from the Indian Archipelago. The Indian species described by Mr. Peal, differs from the Ceylon species in its habit of feeding on the juice of the tea-plant ; and Mr. Moore proposes to call it *H. theivora*.—Prof. Thiselton Dyer read the following extract from a letter from Mr. James Caldwell, Port Louis, Mauritius :—"I would especially call your attention to a case in which the ribbon cane has sported into a green cane and a red cane from the same head. I saw at least 200 instances of it in the same plantation, and the fact has completely upset all our preconceived ideas of the difference of colour being permanent. The conversion of a striped cane into a green cane was not uncommon, but the change into a red cane universally disbelieved, and that both events should occur in the same plant incredible. I find, however, in Fleischman's 'Report on Sugar Cultivation in Louisiana for 1848,' published by the American Patent Office, the circumstance mentioned, but he says he never saw it himself."—In the report of the meeting for Feb. 11 (NATURE, vol. ix. p. 354) the word "gabsy" in line 12 should be omitted.

Mathematical Society, March 12.—Dr. Hirst, F.R.S., president, in the chair.—The following papers were read :—On certain constructions for bicircular quantics, and On a geometrical interpretation of the equations obtained by equating to zero the resultant and the discriminant of two binary quantics, by Prof. Cayley.—On the Cartesian equation of the circle which cuts three given circles at given angles, by J. Griffiths.—On another system of poristic equations, by Prof. Wolstenholme.

EDINBURGH

Royal Physical Society, Feb. 25.—Dr. John Alex. Smith, president, in the chair.—On a New Mode of Estimating the Amount of Colour in Water, by Mr. J. Falconer King. Mr. King's process consists in adding to chemically pure water a standard coloured solution contained in an accurately graduated instrument until the pure water equals in colour the specimen of water under examination. The result of an estimation of colour in a water can thus be accurately recorded and preserved for future reference.—Occurrence of the Deal Fish (*Trachypterus arcticus*), near Montrose, by James C. Howden, M.D., Sunnyside.—Notes of the American Bittern (*Botaurus lentiginosus*), and some other of our rarer birds recently shot in the South of Scotland, by John Alex. Smith, M.D.—Note on a New Fossil from the Silurian Rocks in the Pentland Hills, by Mr. D. J. Brown. Mr. Brown described a section through the rocks in which the fossil occurred. The fossil he believed to be a seaweed, of which it appeared to be a frond. This is the third specimen of seaweed that has been found in these rocks.—Note on Bryozoa, from the carboniferous limestone at Longniddry Station, by Mr. D. J. Brown. Mr. Brown exhibited a fine series of Bryozoa from the lower carboniferous limestone group of Longniddry quarry, Haddingtonshire. This series of Bryozoa consisted principally of fragments of the genera *Fenestrella* and *Polypora*, the whole facies being eminently that of the carboniferous limestone, although amongst them was one fragment apparently referable to the Permian genus *Thamniscus*.—On some Peculiarities in the Geographical Distribution of the Mammalia of Greenland, as explanatory of the origin of the flora and fauna of that country, by Dr. Robert Brown, secretary. Dr. Brown considered that a great portion of the Greenland fauna and the bulk of its flora had been derived from Europe when Greenland was united, probably during or shortly after the time the Miocene beds were laid down to the European continent, by continuous land or a chain of islands, of which it is possible that Iceland, Bear Island, and perhaps even the Orkneys and Shetlands, are only fragments.

MANCHESTER

Literary and Philosophical Society, Feb. 24.—Rev. William Gaskell, vice-president, in the chair.—On the Effect of Acid on the Interior of Iron Wire, by Prof. Osborne Reynolds. It will be remembered that at a previous meeting of this Society Mr. Johnson exhibited some iron and steel wire in which he had observed some very singular effects produced by the action of sulphuric acid. In the first place the nature of the wire was changed in a marked manner, for although it was soft charcoal wire it had become short and brittle; the weight of the wire was increased; and what was the most remarkable effect of all was that when the wire was broken and the face of the fracture wetted with the mouth it frothed up as if the water had acted as a powerful acid. These effects, however, all passed off if the wire were allowed to remain exposed to the air for some days, and if it were warmed before the fire they passed off in a few hours. Prof. Reynolds subjected one of the pieces of wire to a farther examination, and from the result of that examination was led to what appears to be a complete explanation of the phenomena. He was led, from certain observations, to conclude that the effect was due to hydrogen, and not to acid, as Mr. Johnson appeared to think, having entered into combination with the iron during its immersion in the acid, which hydrogen gradually passed off when the iron was exposed. This conclusion he tested and proved to be correct by further experiments. The question appears to the author one of very considerable importance, both philosophically and in connection with the use of iron in the construction of ships and boilers. If, as is probable, the saturation of iron with hydrogen takes place whenever oxidation goes on in water, then the iron of boilers and ships may at times be changed in character and rendered brittle in the same manner as Mr. Johnson's wire, and this, whether it can be prevented or not, is at least an important point to know, and would repay a further investigation of the subject.—Dr. Ransome demonstrated the movements of the chest in respiration, showing the remarkable mobility of its several parts, and the consequent facility with which its cavity can be inflated.

BALTIMORE

Maryland Academy of Sciences, Feb. 2.—A paper was read from Prof. D. S. Martin upon the economical resources of Cornwall, embracing one of the most remarkble deposits of iron ore in America, and forming three large banks. Reference was made in the paper to the immense development of the magnetic oxide of iron in these mines, and to the fact that some of the ore exhibits marked magnetic polarity (native lode-stone). The yield is 175,000 tons annually, but the capacity is double. The minerals of unusual interest found here, with the magnetite, are lodes and coatings of cuprite, chrysocolla, azurite, malachite, and brochantite, the last named being found in no other locality in Eastern North America.

PARIS

Academy of Sciences, March 9.—M. Bertrand in the chair. —M. H. Resal communicated a paper on the theory of the "ground swell" (*houle*).—On a new spiral regulating chronometers and watches, by M. Phillips. The results of experiments made to test the isochronism of chronometers provided with the new spiral.—Researches on crystalline dissociation; estimation and division of the work done in saline solutions, by MM. P. A. Favre and C. A. Valson. A continuation of former communications on this subject.—On a particular arrangement of micrometer with movable wires proposed for the telescopes to be used for the observation of the Transit of Venus, by M. Ph. Hatt. Communicated by M. Fizeau.—New note on waves of variable height and velocity, by M. L. E. Bertin.—On the dispersion of gases, by M. Mascart. The author has determined the dispersion of air, nitrogen, hydrogen, N_2O, CO, CO_2, and CN. The dispersion of these gases bears no direct relation to their refractive power nor to their density.—On the wave-lengths and characters of the violet and ultra-violet rays of the solar spectrum, given by a photograph taken by means of a diffraction grating, by M. H. Draper. An abstract of this paper and the accompanying Alberttype print have already appeared in this journal.—Note on hydrogenised palladium, by MM. L. Troost and P. Hautefeuille. By studying the tension of the gas in the metal at various temperatures the authors come to the conclusion that hydrogenised palladium is a definite compound of hydrogen and palladium with an excess of hydrogen dissolved in it. The tension of the hydrogen is constant from the time the amount of contained gas is equal to 600 volumes; this amount corresponding to the formula Pa_2H. The authors announce a future communication on hydrides of potassium and sodium (K_2H and Na_2H). No allusion is made to the results recently obtained by Wright and Roberts by the determination of the specific heat of hydrogenised palladium.—New apparatus for determining the tannin contained in the different astringent materials employed in tanning, by M. A. Terreil. The process used depends upon the absorption of oxygen by tannin in presence of alkaline liquids.—Organogenesis compared with androgenesis in its relations with natural affinities (Classes *Selaginaceæ* and *Verbenaceæ*), by M. A. Chatin. —Reply to a reclammation of priority of M. Béchamp, by M. P. Schutzenberger.—Probable character of the first fortnight of March, by M. de Tastes. A weather prognostication.—Researches on the origin of the lithological elements of the tertiary and quaternary soils of the neighbourhood of Oran, by M. Daubrée. The author concludes from his researches that the Middle Tertiary epoch was especially an age of eruptions of a trachytic nature. M. A. Nordenskiöld presented to the Academy some photographs taken in Spitzbergen in 1872–73. Among the photographs was one of the largest known mass of meteoric iron. It was discovered in Greenland (Ovifak) in 1870. This mass weighs 21,000 kilogrs. and is about to be brought to Stockholm for the Royal Museum.

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