THURSDAY, JANUARY 3, 1889.

SCIENTIFIC WORTHIES.

XXV.—JAMES JOSEPH SYLVESTER.

JAMES JOSEPH SYLVESTER, born in London on September 3, 1814, is the sixth and youngest son of the late Abraham Joseph Sylvester, formerly of Liverpool.[1] He was educated at two private schools in London, and at the Royal Institution, Liverpool, whence he proceeded in due course of time to St. John's College, Cambridge. In these early days he manifested considerable aptitude for mathematics, and so it was not matter for surprise that he came out in the Tripos Examination of 1837 as Second Wrangler; being incapacitated, by the fact of his Jewish origin, from *taking* his degree, he was not able to compete for either of the Smith's Prizes. In more enlighted times (1872) he had the degrees of B.A. and M.A., by accumulation, conferred upon him, and received therewith the honour of a Latin speech from the Public Orator. He himself says: "I am perhaps the only man in England who am a full (voting) Master of Arts for the three Universities of Dublin, Cambridge, and Oxford, having received that degree from these Universities in the order above given: from Dublin, by *ad eundem;* from Cambridge, *ob merita;* from Oxford, by decree." He is now D.C.L. of Oxford, LL.D. of Dublin and Edinburgh, and Hon. Fellow of St. John's College, Cambridge. It is still open for him to receive yet higher recognition from his own *alma mater*.

Prof. Sylvester became a student of the Inner Temple, July 29, 1846, and was called to the Bar on November 22, 1850.[2] He has been Professor of Natural Philosophy at University College, London; of Mathematics at the University of Virginia, U.S.A.;[3] then ten years later Professor at the Royal Military Academy, Woolwich; and again, after a five years' interval, Professor of Mathematics at the Johns Hopkins University, Baltimore, U.S.A., from its foundation in 1877. Finally, in December 1883, he was elected Savilian Professor of Geometry at Oxford, in succession to Prof. Henry Smith.[4] His first printed paper was on Fresnel's optical theory (in the *Phil. Mag.*, 1837).

We can here only briefly allude to a communication which was accompanied by many important results: we refer to the Friday evening address (January 23, 1874) to the Royal Institution, "On Recent Discoveries in Mechanical Conversion of Motion." He says:—"It would be difficult to quote any other discovery which opens out such vast and varied horizons as this of Peaucellier's,—in one direction, descending to the wants of the workshop, the simplification of the steam-engine, the revolutionizing of the mill-wright's trade, the amelioration of garden-pumps, and other domestic conveniences (the sun of science glorifies all it shines upon) and in the other, soaring to the sublimest heights of the most advanced doctrines of modern analysis, lending aid to, and throwing light from a totally unexpected quarter on the researches of such men as Abel, Riemann, Clebsch, Grassmann, and Cayley. Its head towers above the clouds, while its feet plunge into the bowels of the earth."

[1] Foster's "Hand-book of Men at the Bar" [2] Foster, *l.c.*

[3] The late Prof. Key, of University College and School, was the first occupant of the Chair, founded by Mr. Jefferson, once President of the United States, in 1824.

[4] He commences his Oxford lecture (NATURE, vol. xxxiii. p. 222), of date December 12, 1885, with the words: "It is now two years and seven days since a message by the Atlantic cable containing the single word 'elected' reached me in Baltimore informing me that I had been appointed Savilian Professor of Geometry in Oxford, so that for three weeks I was in the unique position of filling the post and drawing the pay of Professor of Mathematics in each of two Universities."

The only works that Prof. Sylvester has published, we believe, are: (1) "A Probationary Lecture on Geometry, delivered before the Gresham Committee and the Members of the Common Council of the City of London, December 4, 1854," a slight thing which had to be written and delivered at a few hours' notice; (2) "Laws of Verse," 1870; (3) several short poems, sonnets, and translations, which have appeared in our columns and elsewhere.

Our notice would be incomplete without some record of the honours that have been conferred upon Dr. Sylvester. He was elected a Fellow of the Royal Society on April 25, 1839; has received a Royal Medal (1860) and the Copley Medal (1880), this latter rarely awarded, we believe, to a pure mathematician. On this last occasion, Mr. Spottiswoode accompanied the presentation with the words, "His extensive and profound researches in pure mathematics, especially his contributions to the theory of invariants and covariants, to the theory of numbers and to modern geometry, may be regarded as fully establishing Mr. Sylvester's claim to the award." He is a Fellow of New College, Oxford; Foreign Associate of the United States National Academy of Sciences; Foreign Member of the Royal Academy of Sciences, Göttingen, of the Royal Academy of Sciences of Naples, and of the Academy of Sciences of Boston; Corresponding Member of the Institute of France, of the Imperial Academy of Science of St. Petersburg, of the Royal Academy of Science of Berlin, of the Lyncei of Rome, of the Istituto Lombardo, and of the Société Philomathique. He has been long connected with the editorial staff of the *Quarterly Journal of Mathematics* (under one or another of its titles), and was the first editor of, and is a considerable contributor to, the *American Journal of Mathematics;* and he was at one time Examiner in Mathematics and Natural Philosophy in the University of London. He was not an original member of the London Mathematical Society (founded January 16, 1865), but was elected a member on June 19, 1865, Vice-President on January 15, 1866, and succeeded Prof. De Morgan as the second President on November 8, 1866. The Society showed its recognition of his great services to them and to mathematical science generally by awarding him its De Morgan Gold Medal in November 1887. Wherever Dr. Sylvester goes, there is sure to be mathematical activity; and the latest proof of this is the formation, during the last term at Oxford, of a Mathematical Society, which promises, we hear without surprise, to do much for the advancement of mathematical science there.

The writings of Sylvester date from the year 1837; the number of them in the Royal Society Index up to the year 1863 is 112, in the next ten years 38, and in the forthcoming volume 81, making 231 for the years 1837 to 1883: the number of more recent papers is also considerable. They relate chiefly to finite analysis, and cover by their subjects a large part of it: algebra, determinants, elimination, the theory of equations, partitions, tactic, the theory of forms, matrices, reciprocants, the Hamiltonian numbers, &c.; analytical and pure geometry occupy a less prominent position; and mechanics, optics, and astronomy are not absent. A leading feature is the power which is shown of originating a theory or of developing it from a small beginning; there is a breadth of treatment and determination to make the most of a subject, an appreciation of its capabilities, and real enjoyment of it. There is not unfrequently an adornment or enthusiasm of language which one admires, or is amused with: we have a motto from Milton, or Shakespeare; a memoir is a trilogy divided into three parts, each of which has its action complete within itself, but the same general cycle of ideas pervades all three, and weaves them into a sort of complex unity; the apology for an unsymmetrical solution is—symmetry, like the grace of an eastern robe, has not unfrequently to be purchased at the expense of some sacrifice of freedom and rapidity of action; and, he remarks, may not music be described as the mathematic of sense, mathematic as the music of the reason? the soul of each the same! &c. It is to be mentioned that there is always a generous and cordial recognition of the merit of others, his fellow-workers in the science.

It would be in the case of any first-rate mathematician—and certainly as much so in this as in any other case—extremely interesting to go carefully through the whole of a long list of memoirs, tracing out as well their connection with each other, and the several leading ideas on which they depend, as also their influence on the development of the theories to which they relate; but for doing this properly, or at all, space and time, and a great amount of labour, are required. Short of doing so, one can only notice particular theorems—and there are, in the case of Sylvester, many of these, "beautiful exceedingly," which, for their own sakes, one is tempted to refer to—or one can give titles, which, to those familiar with the memoirs themselves, will recall the rich stores of investigation and theory contained therein.

A considerable number of papers, including some of the earliest ones, relate to the question of the reality of the roots of a numerical equation: in the several connections thereof with Sturm's theorem, Newton's rule for the number of imaginary roots, and the theory of invariants. Sylvester obtained for the Sturmian functions, divested of square factors, or say for the reduced Sturmian functions, singularly elegant expressions in terms of the roots, viz. these were $f_2(x) = \Sigma(a-b)^2(x-c)(x-d)\ldots$, $f_3(x) = \Sigma(a-b)^2(a-c)^2(b-c)^2(x-d)\ldots$, &c.; but not only this: applying the Sturmian process of the greatest common measure (not to $f(x), f'(x)$, but instead) to two independent functions $f(x)$, $\phi(x)$, he obtained for the several resulting functions expressions involving products of differences between the roots of the one and the other equation, $f(x) = 0$, $\phi(x) = 0$; the question then arose, what is the meaning of these functions? The answer is given by his theory of *intercalations*: they are signaletic functions, indicating in what manner (when the real roots of the two equations are arranged in order of magnitude) the roots of the one equation are intercalated among those of the other. The investigations in regard to Newton's rule (not previously demonstrated) are very important and valuable: the principle of Sturm's demonstration is applied to this wholly different question: viz. x is made to vary continuously, and the consequent gain or loss of changes of sign is inquired into. The third question is that of the determination of the character of the roots of a quintic equation by means of invariants. In connection with it we have the noteworthy idea of *facultative* points; viz., treating as the coordinates of a point in n-dimensional space those functions of the coefficients which serve as criteria for the reality of the roots, a point is facultative or non-facultative according as there is, or is not, corresponding thereto any equation with real coefficients: the determination of the characters of the roots depends (and, it would seem, depends only) on the bounding surface or surfaces of the facultative regions, and on a surface depending on the discriminant. Relating to these theories there are two elaborate memoirs, "On the Syzygetic Relations &c.," and "Algebraical Researches &c.," in the Philosophical Transactions for the years 1853 and 1864 respectively; but as regards Newton's rule later papers must also be consulted.

In the years 1851–54, we have various papers on homogeneous functions, the calculus of forms, &c. (*Camb. and Dub. Math. Journal*, vols. vi. to ix.), and the separate work "On Canonical Forms" (London, 1851). These contain crowds of ideas, embodied in the new words, *cogredient*, *contragredient*, *concomitant*, *covariant*, *contravariant*, *invariant*, *emanant*, *combinant*, *commutant*, *canonical form*, *plexus*, &c., ranging over and vastly extending the then so-called theories of linear transformations and hyperdeterminants. In particular, we have the introduction into the theory of the very important idea of *continuous* or *infinitesimal* variation: say that a function, which (whatever are the values of the parameters on which it depends) is invariant for an infinitesimal change of the parameters, is absolutely invariant.

There is, in 1844, in the *Philosophical Magazine*, a valuable paper, "Elementary Researches in the Analysis of Combinatorial Aggregation," and the titles of two other papers, 1865 and 1866, may be mentioned: "Astronomical Prolusions; commencing with the instantaneous proof of Lambert's and Euler's theorems, and modulating through the construction of the orbit of a heavenly body from two heliocentric distances, the subtended chord, and the periodic time, and the focal theory of Cartesian ovals, into a discussion of motion in a circle and its relation to planetary motion"; and the sequel thereto, "Note on the periodic changes of orbit under certain circumstances of a particle acted upon by a central force, and on vectorial coordinates, &c., together with a new theory of the analogues of the Cartesian ovals in space."

Many of the later papers are published in the *American Mathematical Journal*, founded, in 1878, under the auspices of the Johns Hopkins University, and for the first six volumes of which Sylvester was editor-in-chief. We have, in vol. i., a somewhat speculative paper entitled "An application of the new atomic theory to the graphical representation of the invariants and covariants of binary quantics," followed by appendices and notes relating to various special points of the theory; and in the same and subsequent volumes various memoirs on binary and ternary quantics, including papers (by himself, with the aid of Franklin) containing tables of the numerical generating functions for binary quantics of the first ten orders, and for simultaneous binary quantics of the first four orders, &c. The memoir (vols. ii. and iii.) on "Ternary cubic-form equations" is connected with some early papers relating to the theory of numbers. We have in it the theory of residuation on a cubic curve, and the beautiful chain-rule of rational derivation; viz. from an arbitrary point 1 on the curve it is possible to derive the singly infinite series of points (1, 2, 4, 5, . . . $3p \pm 1$) such that the chord through any two points, m, n, again meets the curve in a point $m+n$, $m \sim n$ (whichever number is not divisible by 3) of the series; moreover, the coordinates of any point m are rational and integral functions of the degree m^2 of those of the point 1.

There is in vol. v. the memoir, "A Constructive Theory of Partitions arranged in three acts, an Interact in two parts, and an Exodion," and in vol. vi. we have "Lectures on the Principles of Universal Algebra" (referring to a course of lectures on multinomial quantity, in the year 1881). The memoir is incomplete, but the general theories of nullity and vacuity, and of the corpus formed by two independent matrices of the same order, are sketched out; and there are in the *Comptes rendus* of the French Academy later papers containing developments of various points of the theory,—the conception of "nivellators" may be referred to.

The last-mentioned paper in the *American Mathematical Journal* was published subsequently to Sylvester's return to England on his appointment as Savilian Professor of Mathematics at Oxford. In December 1886, he gave there a public lecture containing an outline of his new theory of reciprocants (reported in NATURE, January 7, 1887), and the lectures since delivered are published under the title, "Lectures on the Theory of Reciprocants" (reported by J. Hammond), same *Journal* vols. viii. to x.; thirty-three lectures actually delivered, entire or in abstract, in the course of three terms, to a class in the University, with a concluding so-called lecture 34, which is due to Hammond. The subject, as is well known, is that of the functions of a dependent variable, y, and its differential coefficients, y', y'', . . . , in regard to x (or, rather, the functions of y', y'', . . .), which remain unaltered by the interchange of the variables x and y: this is a less stringent condition than that imposed by Halphen ("Thèse," 1878) on his differential invariants, and the theory is accordingly a more extensive one. A passage may be quoted:—"One is surprised to reflect on the change which is come over Algebra in the last quarter of a century. It is now possible to enlarge to an almost unlimited extent on any branch of it. These thirty lectures, embracing only a fragment of the theory of reciprocants, might be compared to an unfinished epic in thirty cantos. Does it not seem as if Algebra had attained to the dignity of a fine art, in which the workman has a free hand to develop his conceptions, as in a musical theme or a subject for painting? Formerly, it consisted in detached theorems, but nowadays it has reached a point in which every properly-developed algebraical composition, like a skilful landscape, is expected to suggest the notion of an infinite distance lying beyond the limits of the canvas." And, indeed, the theory has already spread itself out far and wide, not only in these lectures by its founder, but in various papers by auditors of them, and others,—Elliott, Hammond, Leudesdorf, Rogers, Macmahon, Berry, Forsyth.

Sylvester's latest important investigations relate to the Hamiltonian numbers: there is a memoir, *Crelle*, t. c. (1887), and, by Sylvester and Hammond jointly, two memoirs in the Philosophical Transactions. The subject is that of the series of numbers 2, 3, 5, 11, 47, 923, calculated thus far by Sir W. R. Hamilton in his well-known Report to the British Association, on Jerrard's method. A formula for the independent calculation of any term of the series was obtained by Sylvester, but the remarkable law by means of a generating function was discovered by Hammond, viz. E_0, E_1, E_2, . . . , being the series 3, 4, 6, . . . of the foregoing numbers, each increased by unity; then these are calculated by the formula $(1-t)^{E_0} + t(1-t)^{E_1} + t^2(1-t)^{E_2} + \ldots = 1 - 2t$, equating the powers of t on the two sides respectively: observe the paradox, $t = \frac{1}{2}$, then the formula gives 0 = sum of a series of positive powers of $\frac{1}{2}$.

Enough has been said to call to mind some of Sylvester's achievements in mathematical science. Nothing further has been attempted in the foregoing very imperfect sketch.

A. CAYLEY.

THE CREMATION OF THE DEAD.

The Cremation of the Dead. By Hugo Erichsen, M.D. (Detroit: D. O. Haynes and Co., 1887.)

THIS book is an appeal to the general public on the propriety of introducing the practice of cremation, universally, into civilized communities; or, as the author puts it, "it is a plea for the burning of the dead." He considers, and we are inclined to think he is right, that the period of fanatic and fierce opposition to cremation has passed, and has made way for a calm consideration of the subject. In 1874, he tells us, a Persian gentleman then resident in one of the Eastern States of the free and great Republic of America, who wanted to have his wife cremated, was compelled by an ignorant mob to resort to interment; but now the feeling has changed.

In our own country the same sensible desire to discuss the question of cremation, fully and freely, is fairly established at the present time; and so greatly has prejudice disappeared, that now the act of cremation has been carried out over fifty times at the Woking Cemetery alone. As Sir Spencer Wells shows, in an introductory chapter which he has written for the work before us, the obstacle of law in England against cremation has been removed, and relatives may resort to the cremation of their dead without any unreasonable impediments.

In saying so much in favour of freedom in regard to cremation, we must, however, in this country confine the freedom to the voice of the living. The wishes of the dead, though they may have been delivered up to the last moment in favour of cremation, and may even have been ordered in the will of the deceased, have no legal weight with the survivors. The writer of this article was called, quite recently, to see a lady who had rather suddenly died, to determine that life was actually extinct. She had been haunted with the most terrible fears that she might be buried alive, fears much intensified by the existence in her family of a tradition that one of her relatives had actually been subjected to this awful ordeal. She had directed in her will that she was to be cremated, but her legal adviser, who had himself drawn out the document, discovered that it had no force in regard to the direction of cremation, and two of the nearest relatives having a determined and conscientious objection to the process, the body had to be interred. In this instance every precaution was taken that the body was absolutely dead, and even decomposed, before it was laid in the earth, and to this extent the wishes of the deceased were fulfilled; but the fact that the law does not respond to the wishes of the dead is a point to be remembered by all who would be cremated. The same failure of law seems to be operative in Italy, for we all remember that the final request of the great Garibaldi as to the disposal of his body by fire remains to the present moment disregarded.

Dr. Erichsen must at once receive the credit of having written the best book that has issued from the press on the subject of cremation. It is short and yet full, concise and yet complete. There are eight chapters: the first, a history of cremation; the second, the evils of burial, and the sanitary aspects of incineration; the third, cremation in times of war; the fourth, the processes of modern cremation; the fifth, the medico-legal aspect of incineration, and the objections to cremation; the sixth, burial alive, cremation from an æsthetic and religious point of view; the seventh, the present state of the cremation question.

The introductory letter by Sir Spencer Wells, to which reference has already been made, is an excellent prelude to the chapters above recorded. Sir Spencer Wells has for many years been a staunch and consistent advocate of cremation, and has put it on record that, when the time comes—may it be long delayed!—his body is to be destroyed by fire. He writes, therefore, with authority, as one who has well considered the subject in all its details, and has learned the best and most forcible answers to the many objections that have from time to time been raised against cremation. He quotes the late Dr. Parkes's statement "that neither affection nor religion can be outraged by any manner of disposal of the dead which is done with proper solemnity and respect to the earthly dwelling-places of our friends. The question should be placed entirely on sanitary grounds. Burying in the earth appears certainly to be the most insanitary plan." On the religious side of the question, Wells also adds a strong sentence from the late Lord Shaftesbury, who remarked to him that, if cremation were wrong, "what has become of the blessed martyrs who were burned at the stake in ancient and modern persecutions?"

We turn naturally, as scientific readers, to the section of Dr. Erichsen's work which treats on the sanitary aspect of the subject. This is not, in our view, the strongest part. In it the author has collated the widely reported instances of the spread of epidemics on the opening of burial-places where persons who died of contagious diseases, similar to maladies which have broken out, have been interred. But here three fallacies are suggested. In the first place, it is impossible to accept all the illustrations as illustrations strictly in point and entirely trustworthy—or, for the matter of that, any of the evidence as absolutely trustworthy—seeing that other causes which might have been at work to produce the effects named are not duly eliminated. In the second place, if the instances cited may be accepted as *primâ facie* evidence, they accord imperfectly with other instances, not of exceptional, but of every-day life, in which cemeteries and graveyards holding the remains of those who have died of contagious maladies have been partly or largely opened without any manifestation of the dangers referred to; or in which persons have lived for long series of years in close proximity to graveyards and cemeteries receiving the dead from infectious diseases, and yet have not suffered from those diseases more than others in other localities. There is at the present moment a cemetery near London, from which at times, emanations of the worst kind proceed, indicating that the cemetery is overcharged with dead, and ought at once to be closed; but no epidemic has broken out from it as a centre of contagion. In the third place, the evidence collected by the author, if it were accepted as mainly trustworthy, is not quite *ad rem*. It would be correct in so far as old burial-grounds and old modes of burial are concerned, but it would have no bearing whatever on the earth-to-earth system of burial which our countryman, Mr. Seymour Haden, has done so much to introduce and to perfect.

Dr. Erichsen's answer to these objections would be: Why linger at all over the bodies of the dead? they feel not, neither do they know. "It is of no consequence to the dead whether they rot in earth and originate miasmata, or are transformed by fire into pure white ashes. They feel as little of the process of decay as they do of the flame: their eye is surrounded by the same darkness, whether down in the deep grave or in the glowing light of the crematory furnace. But it is of greatest consequence to us, the living; and the only way to protect ourselves from poisonous infection by our dead is to burn them."

In this one sentence lies, in a scientific sense, the gist of the whole question. If it were true and demonstrable that the only way by which the living can be preserved from the dead is to burn the dead, every true man of science would support the principle of cremation out and out, and the practice would become universal in a very short space of time. Moreover, as Science, like Nature herself, has no morbid sentiments, but goes straight to and for the truth, she would not tarry long in making herself heard. It is just because the voice of Science cannot be so absolute that it demurs or hesitates. Her scholars inquire amongst the living of the day to see if they afford an answer to the important question. They ask: Are the persons whose duty it is to be nearest to the dead immediately after death—the upholsterers and the servants of the cemeteries and graveyards—more liable than others to the infectious diseases from their special occupations? and the answer which comes back is certainly negative. They ask other and similar questions:—How many times has it been known that a medical man in conducting the autopsy of a person who has died of a contagious affection has contracted the disease? How many women of the death-chamber have contracted disease from the dead? These questions also receive a negative answer; and as a matter of course the man of science is, therefore, unable to be dogmatic or to strain a necessity: he cannot clearly recognize, on a "not proven" verdict, the duty of wounding the extremely sensitive feelings of millions of his fellow-men, on a subject that is amongst the most tender of all that pertains to humanity.

Our idea is that in the current state of public opinion, and in the current state of scientific knowledge, it is best to let the public feeling towards cremation work its own way, and to let earth-to-earth burial also have its free course.

Cremation will come partly by necessity, partly by a gradual sentiment in its favour. To force it by conjuring up dangers which do not exist is the very means of arresting it in its progress. We do not say that the work we have had under review is open to too severe criticism on these grounds; on the contrary, it adduces such a number of sound arguments in support of its case, and, on the whole, shows such a just and good weight on its own side, that we commend it as an excellent treatise—we should not improperly say standard treatise—on cremation.

ASSAYING.

Practical Metallurgy and Assaying: a Text-book for the use of Teachers, Students, and Assayers. By Arthur H. Hiorns. Pp. 471, with 91 Illustrations, Appendix, and Index. (London: Macmillan and Co., 1888.)

ASSAYING was a term originally used to denote the estimation, by the agency of heat, of a particular metal in an ore, alloy, or other metallic compound. Since the publication of Agricola's work in 1556, numerous English translations of foreign treatises on the subject have been published. Amongst these may be mentioned the translations of the works of Erker (1629), Barba (1674), and Cramer (1774). Assaying by the dry way has changed so little that the methods and instruments described in these old books might still be successfully used. Since the introduction, however, of the rapid and accurate wet processes, improvements have quickly followed each other, and from a particular ore a larger yield is now obtained than was formerly the case, so that the dry methods are, with a few exceptions, rapidly falling into disuse, as in many cases they do not indicate with sufficient precision the amount of metal actually present in the ore. The modern English literature of assaying is confined to Mitchell's large treatise, and to the chapters given in Percy's works and in Phillips's "Elements of Metallurgy." No small text-book, in which full cognizance is taken of wet processes, has hitherto been published, and a gap in our metallurgical literature has now been well filled by Mr. Hiorns's useful book, which is based on the course of instruction organized at the Royal School of Mines by Prof. W. C. Roberts-Austen, to whom the author, as an old pupil, dedicates his work. In all the Continental Schools of Mines, the instruction is conducted in a most unsatisfactory manner. Large classes rapidly pass through the various assaying processes, all the students working together with military precision at the Professor's word of command. In London, on the other hand, each student works independently, and is not permitted to pass from one metal to another until he can prove that he is able to constantly produce trustworthy results. As a student of the Royal School of Mines, Mr. Hiorns has thus had an excellent training for the task he has undertaken. Besides this, as Principal of the School of Metallurgy at the Birmingham and Midland Institute, he has had ample opportunity of ascertaining the wants of the average student.

Like so many of the text-books of science now published, Mr. Hiorns's book has been arranged to meet the requirements of the Science and Art Department Syllabus. The first part contains a number of experiments for the student to perform in order to elucidate the principles upon which metallurgy is based; the second part contains an account of the methods of assaying by dry methods; whilst the third deals with assaying by wet methods, and includes volumetric analysis and the analysis of furnace gases. The course is very systematically arranged, and it is certain that any student who has performed the experiments enumerated would be thoroughly well grounded in practical metallurgy. And the fact that such a book is now required by a large number of students in evening classes shows what excellent service the Science and Art Department is doing for practical metallurgy throughout the country.

The author discusses several of the newer methods, such as Turner's method of estimating carbon in iron, and alludes to recent researches, such as those of Beringer on the accuracy of the volumetric estimation of copper. He appears, however, to be unacquainted with the newer methods in use on the Continent, and it is to be regretted that he has not consulted the standard works of Balling and of Bruno Kerl, or the careful abstracts of foreign papers published in the Journals of the Chemical Society and of the Iron and Steel Institute. It is to be regretted, too, that there is a want of uniformity in the weights and measures adopted. Grains and grammes, ounces and cubic centimetres, are used indiscriminately. For industrial purposes, it was perhaps necessary that the "grains" should be retained. But, with regard to the "ounces," many assayers, who are familiar with metric measures, have no idea how many ounces make a pint. In the nomenclature and notation, there is also an unfortunate want of uniformity, as is shown by the indiscriminate use of the terms, oil of vitriol and sulphuric acid, carbonate of soda and sodium carbonate, hæmatite and hematite, OH_2 and H_2O, SO_4H_2 and H_2SO_4. The book is remarkably free from typographical errors. The name Fresenius is, however, spelt wrong in places (pp. 174, 183) and "oxide of silica" (p. 312) is a compound unknown to the chemist.

On the whole, the work is an excellent one, and will, no doubt, prove of great service to the teachers and students of classes in practical metallurgy. Chemists generally, accustomed to ordinary laboratory manipulation, will be interested to see how many operations there are, which, while differing from those with which they deal, are capable of affording very trustworthy results. The illustrations are of a very effective character, and are well executed from drawings that have been prepared with an amount of care not usual in figures of this class. Mr. Hiorns's literary style is far from faultless, but his instructions are always perfectly clear, and, to use the words of an old metallurgist, "he writes like one who hath black'd his Fingers and sing'd his Beard in metallick Operations."

B. H. B.

THE ORCHIDS OF THE CAPE PENINSULA.

The Orchids of the Cape Peninsula. By Harry Bolus, F.L.S. With Thirty-six Plates, partly coloured. Off-print from the Transactions of the South African Philosophical Society, 1888, Vol. V., Part 1. (Cape Town, 1888.)

"THIS," as the author informs us, "is an attempt to describe the Orchids growing on the peninsula of the Cape of Good Hope; to give their names and synonyms; to arrange them as far as possible in groups; to adduce the stations where they have been found, and their further distribution so far as known. To this is added a list of collectors; and of books and papers already published upon the subject of South African Orchidology." Mr. Bolus's name as an authority on Cape Orchids is already well known through his papers in the Journal of the Linnean Society; and the thoroughness with which his work has been done is vouched for by the fact that it has occupied a great part of his leisure time for several years, and embodies the results of a comparison of the Orchids of Thunberg's Herbarium, by Mr. N. E. Brown, A.L.S.; also by the fact that Lindley's Type Herbarium, and the General Herbarium, at Kew, where Mr. Bolus has been staying for several months, have been fully consulted.

The Cape peninsula is a tract of land about forty miles long, varying in width from about three to eleven miles, and has a total area of 197½ square miles; and it is interesting to note that in an area about one-fourth larger than the Isle of Wight, no less than 102 species, belonging to ten genera are found, thirty-three of which, so far as at present known, are endemic. The order is considered to take a position the fourth in importance in the flora (after Compositæ, Leguminosæ, and Ericaceæ), and to constitute 5·8 of the whole. The altitudinal range of the species is very interesting. The greater part of the area in question is occupied by a central mountain range, of which Table Mountain, which attains an elevation of 3562 feet, is the highest part. From Mr. Bolus's tables, it appears that fifty-nine species never descend into the plains to a lower elevation than 500 feet, twenty others are always found below this elevation, while the twenty-three remaining ones are indifferent in this respect. He also remarks that fifteen species have a vertical range of from 2000 to 3000 feet, and six species a range of more than 3000 feet. This large vertical range, which is shared in common with many of the flowering plants, Mr. Bolus attributes to the equability of the temperature, and of the moisture of the atmosphere at different elevations, owing to the close proximity of the sea on nearly every side.

One of the species is of such great beauty that there has been some danger of its ultimate extinction; on which points the following will be read with interest. "The peerless *Disa uniflora* is in its glory on the rivulets of Table Mountain in February. . . . This beautiful flower is the object of universal admiration, and the name which has been given to it, the 'Pride of Table Mountain,' indicates the honour in which it is held. It is, indeed, the queen of terrestrial Orchids in the southern hemisphere, as *Cypripedium spectabile* may be said to reign, though with less magnificence, in the northern. . . . It is still abundant on Table Mountain, although of late years large quantities of the tubers have been annually exported to Europe, and much needless destruction, arising from wasteful gathering by unskilled hands, resulted. But the summit of the mountains being Crown land, the Government has recently intervened, and restricted the removal of tubers within reasonable limits, so that, if this supervision be continued, there will be little reason to fear the extinction of this truly noble species."

The thirty-six, partly coloured, plates, which represent the rarer or least known species, are drawn by the author, and the dissections and botanical details are admirably portrayed; though in some cases the outline only is given, and a little shading would have enhanced their effect.

Respecting the structure and homologies of Orchideæ, largely cited from Darwin, and with a plan of the flower from the same source, it may be pointed out that the so-called union of the two lateral stamens of the outer staminal whorl with the median petal, to form the lip, was disputed, and, I think, satisfactorily disproved, by Crueger; a view which has been since confirmed by Dr. Masters, in the case of *Cypripedium*. The papers in question appear to have been overlooked, but the oversight detracts little from the value of this admirably executed work. To those who wish to procure copies, the omission of the publisher's name is unfortunate. Messrs. West, Newman, and Co., of Hatton Garden, E.C., are the printers, and may be able to supply the work.

R. A. ROLFE.

OUR BOOK SHELF.

Carl von Linné's ungdomsskrifter. Samlade af Ewald Ährling, och efter hans död med statsunderstöd utgifvna, af K. Vetenskaps-Akademien. Första serien, första häftet. (Stockholm: P. A. Norstedt & Söner, 1888.)

THIS is the first part of the youthful writings of Linnæus, collected by the late Dr. Ewald Ährling, and published, under a State grant, by the Royal Swedish Academy of Science. The work as a whole is to be divided into two series of several parts each, the first series including a record of the life of Linnæus up to the year 1734, with botanical addenda. The second will contain the author's account of his journeys in Lapland (1732), hitherto printed only in English; in Dalabergslagen (1733); in Dalecarlia (1734); and notes on his sojourn abroad (1735). In the first series are the following purely botanical works:—"Hortus Uplandicus," after Tournefort's system, from the original in the possession of the Linnean Society; "Hortus Uplandicus," after Tournefort's system, with an addendum, and a new division of Umbellatæ (1730), from the original in the Leufsta Library; "Hortus Uplandicus," after the author's method the sexual system (1731), original in possession of the Rev. J. Johansson, at Ifvetofta; and "Adonis Uplandicus," after the sexual system (1731), original in the Leufsta Library.

The majority of readers will find that the most interesting of the papers in the part before us is the great botanist's diary. The original of this, wholly in the handwriting of Linnæus, is one of the two autographs in the possession of the Linnean Society. It contains thirteen closely-written pages, and we must conclude, from a remark in the diary, that it was written between 1730 and 1735. To judge from difference of writing and ink, additions were made at a later date.

The diary is followed by "Catalogus Plantarum

Rariorum Scaniæ item Catalogus Plantarum Rariorum Smolandiæ" (1728), in the possession of the De Geer family (Leufsta Library); and by "Spolia Botanica" (1729), the original of which is in the possession of the Linnean Society, and is considered to have been finished towards the end of 1729. This seems, however, improbable, the date of dedication (to Prof. Roberg, one of Linnæus's teachers at Upsala) being May 5, 1729. The work is accompanied by twelve facsimile drawings of the principal representatives of the Lapland flora.

This part of the first series contains copious and explanatory notes by the late Dr. Ährling, a work which must have entailed very great labour. After his death, his editorial duties were undertaken by Dr. M. B. Swederus. The second series will be edited by Prof. G. Lindström.

First Principles of Physiography. By John Douglas (London: Chapman and Hall, 1889.)

THE ever-increasing number of text-books on this subject is evidence that the study of physiography is gaining in popularity. The object of the book before us, as the author states in his "Prologue on the Beach," is to give a systematic statement of the nature of the forces at work in the world, and of the changes which the matter of the world undergoes. The book is obviously designed to cover the syllabus issued by the authorities at South Kensington, although no mention of this fact is made.

The first part of the book deals with force, but for some reason or other, force is not defined until p. 26, and there only in an obscure place. The author's notion of treating elementary chemical ideas is somewhat peculiar; to make statements about positive and negative elements without explaining the meanings of those terms, and to use formulæ like NH_3 and H_2SO_4 (p. 36) without naming the compounds they represent, is scarcely the way to inspire a student with confidence in his teacher.

No less than 23 pages are devoted to tables, all of more or less interest to students of physiography.

Perhaps the chief novelty of the book is the introduction of copious quotations from, and references to, standard works. Their introduction as footnotes, however, is rather objectionable, as it tends to discontinuity. A good deal of information is undoubtedly given, but the style is not such as to commend it to those who are just commencing the study of science, and these, it must be remembered, constitute the majority of those who take up the subject of physiography.

LETTERS TO THE EDITOR.

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

"Engineers" *versus* "Professors and College Men."

PROF. GREENHILL is, himself, one of many proofs that the distinction between "Engineers" and "Professors and College Men" is a Cross Division. Every "Engineer" ought to be a "highly-trained College man." If he were, he would know at once, from the very first sentence of the Principia (*Quantitas materiæ est mensura ejusdem* &c., &c.) that mass is the personal property of a body, one of the invariable things in nature:—and not an accidental property dependent, for its amount and even for its very existence, on the momentary surroundings. The letter M has hitherto been used by Newtonians in this sense. If anyone has since attached to it another and different sense, *he* is responsible for the consequent confusion. Would it not be well if Prof. Greenhill, and the School to which he has attached himself, would kindly leave to Newtonians their M, as defined for them by their Master; and (with severely logical consistency) turn it upside down (thus, W) when they wish to embody their own revolutionary definition? No Newtonian will refuse to recognize $Wv^2/2g$ as a correct expression for so much energy:—though he will probably think it both clumsy and complex, and will prefer to write as usual his $Mv^2/2$.

I am curious to know how Prof. Greenhill would deal with physical Astronomy. What is his measure of the earth's mass? According to the analogy of his "*units of* g *pounds*" the earth's mass is at present (near perihelion) to be spoken of as if it were some 6 or 7 per cent. greater than it was six months ago!

The whole of this attempt to improve on Newton is caused by unwillingness to face, once for all, the small amount of labour and thought requisite for learning or teaching how to pass from one system of units to another. A properly taught student learns, very early in his career, that this is no awful and mysterious process:—in fact that it is, throughout, quite as simple in principle as is the passing from miles per hour to feet per second.

And I venture to assert that such a student would attack with ease and confidence any *fair* question (*i.e.* one free from mere tricks or traps) connected with the subject. This one, for intance:—

"How many of the following quantities (taken in order) can, by selection of the requisite system of units, be simultaneously expressed by one and the same *number*. *First*, when that number is given? *second*, when it is not?

(*a*) The weight of a ton, at sea-level, at the equator.

(*b*) The speed of light *in vacuo*.

(*c*) The average kinetic energy of a particle of hydrogen at 0° C.

(*d*) The minimum compressibility of water at low pressures.

(*e*) The mean angular velocity of the earth about the sun.

Express the requisite units in C.G.S. measure, when the common numerical value, above mentioned, is $\log_\epsilon \pi$; and also when it is not assigned."

Of course it is understood, and this is my answer to Prof. Greenhill's first question, that *the student would be furnished with all the necessary data*, experimental or otherwise, expressed in definite *assigned* units.

In answer to Prof. Greenhill's second question I need only say that it is no part of my case to assert that all statements, made by "College men," are *necessarily* characterized by definiteness, by accuracy, or even by common-sense.

December 21, 1888. P. G. TAIT.

The Sun-spot Cycle.

IT may interest some of the readers of NATURE to learn that an expected change has just been observed upon the solar surface.

It is a well-established fact that in each new series of sunspots the first spots of the cycle are seen in high solar latitudes, and that as the number of spots increases there is a common drift towards the sun's equator, the spot area becoming most extensive as the sixteenth parallel of heliographic latitude is reached. During late years the spots have been diminishing in number and size, and approaching the solar equator; and in the past twelve months very few spots have been seen on the sun's surface, and all in low latitudes, that recorded on December 21 being 4° south of the solar equator. The close of the year has, however, witnessed a change, as a small spot is recorded on the Stonyhurst drawing of December 30 at 36° south latitude. Spots near the equator will probably continue to be observed for some time, but, whilst they are diminishing, those in higher latitudes will be on the increase. S. J. PERRY.

Stonyhurst Observatory, Lancashire, December 31, 1888.

"Renaissance of British Mineralogy."

MR. FLETCHER'S admirable address on a "Renaissance of British Mineralogy," of which a report was published in a recent issue of your paper, calls timely attention to the present condition of the science. Mineralogy as a popular study seems dead: the chemists have deserted it for a study of complex organic compounds, so that it has become a mere hanger-on of geology. The science is now not thoroughly taught in any institution in this country, and teachers therefore have no means of acquiring knowledge, in the only really useful way, by working under the acknowledged masters. This is especially the case with crystallography, which must at present, unfortunately, be regarded as a branch of mineralogy. And if they cannot acquire sound knowledge, how can they teach well? I feel quite sure that if the Science and Art Department would institute a summer course for teachers, where they would not have to waste their time over the merest rudiments, but could study practically the methods of crystal measurements, and the higher branches of mineralogical research, it would be largely taken advantage of by teachers and those who intend to become teachers. Failing this, I for one should be glad to know of any institution in this country or in Germany where such instruction could be obtained during the summer months.

A TEACHER.

Glasgow, December 31, 1888.

Ventilating Bees.

I do not know whether it is generally known that here, and I believe in other tropical countries, there are in every hive what one can only describe as "ventilating bees." I mean that during the hot season two or three bees post themselves, on their heads, at the entrance of the hive, and fan the interior with the incessant motion of their wings. They are relieved at intervals by fresh bees, who carry on the process. They are kept to their duty by a sort of patrol of bees to insure their incessant activity. This is a well-authenticated and known fact, and as such may interest your readers.

EVA M. A. BEWSHER.

Mauritius, November 21.

Sonorous Sand at Botany Bay.

REFERRING to the correspondence with regard to the so-called "musical sands," which has appeared in your columns, it may be of interest to record the fact that sand with similar properties is found in Botany Bay, New South Wales, not far from the spot where Captain Cook first landed. When displaced by pressure from above, or disturbed with the hand or stick, this sand emits a musical sound, which appears to vary in intensity according to the degree of moisture which it contains. Should any of your correspondents wish for specimens, I shall be happy to forward a small quantity to them.

A. SIDNEY OLLIFF.

Australian Museum, Sydney, November 16, 1888.

HOW RAIN IS FORMED.[1]

IN certain villages in the Indian Central Provinces, besides the village blacksmith, the village accountant, the village watchman, and the like, there is an official termed the *gápogári*, whose duty it is to make rain. So long as the seasons are good and the rain comes in due season, his office is no doubt a pleasant and lucrative one. It is not very laborious, and it is obviously the interest of all to keep him in good humour. But if, as sometimes happens, the hot dry weather of April and May is prolonged through June and July, and week after week the *ryot* sees his young sprouting crops withering beneath the pitiless hot winds, public feeling is wont to be roused against the peccant rain-maker, and he is led forth and periodically beaten until he mends his ways and brings down the much-needed showers.

You will hardly expect me, and I certainly cannot pretend, to impart to you the trade-secrets of the professional rain maker. Like some other branches of occult knowledge which Madam Blavatsky assures us are indigenous to India, this art of rain-making is perhaps not to be acquired by those who have been trained in European ideas; but we can at least watch and interrogate Nature, and learn something of her method of achieving the same end; and if her scale of operations is too large for our successful imitation, we shall find that not only is there much in it that may well challenge our interest, but it may enable us to some extent to exercise prevision of its results.

Stated in the most general terms, Nature's process of rain-making is extremely simple. We have its analogue in the working of the common still. First, we have steam or water vapour produced by heating and evaporating the water in the boiler; then the transfer of this vapour to a cooler; and finally we have it condensed by cooling, and reconverted into water. Heat is communicated to the water to convert it into vapour, and when that heat is withdrawn from it, the vapour returns to its original liquid state. Nature performs exactly the same process.

In the still, the water is heated until it boils; but this is not essential, for evaporation may take place at all temperatures, even from ice. A common little piece of apparatus, often to be seen in the window of the philosophical instrument maker, and known as Wollaston's cryophorus, is a still that works without any fire. It consists of a large glass tube with a bulb at each end, one of which is partly filled with water; and, all the air having been driven out of the tube by boiling the water, it is hermetically sealed and allowed to cool. It then contains nothing but water and water vapour, the greater part of which re-condenses when it cools. Now, when thus cold, if the empty bulb be surrounded by ice, or, better, a mixture of ice and salt, the water slowly distils over, and is condensed in the colder bulb, and this without any heat being applied to that which originally contained the water. And this shows us that all that is necessary to distillation is that the condenser be kept cooler than the evaporater.

Nevertheless, at whatever temperature it evaporates, water requires heat, and a large quantity of heat, merely to convert it into vapour; and this is the case with the cryophorus; for if the evaporating bulb be wrapped round with flannel, and so protected from sources of heat around, the water cools down until it freezes. That is to say, it gives up its own heat to form vapour. A simple experiment that anyone may try with a common thermometer affords another illustration of the same fact. If a thermometer bulb be covered with a piece of muslin, and dipped into water that has been standing long enough to have the same temperature as the air, it gives the same reading in the water as in the air. But if when thus wetted it be lifted out and exposed to the air, it begins to sink at once, owing to the evaporation of the water from the wet surface, and it sinks the lower the faster it dries. In India, when a hot wind is blowing, the wet bulb sometimes sinks 40° below the temperature of the air.

Now this is a very important fact in connection with the formation of rain, because it is owing to the fact that water vapour has absorbed a large quantity of heat—which is not sensible as heat, but must be taken away from it before it can be condensed and return to the liquid state—that vapour can be transported as such by the winds for thousands of miles, to be condensed as rain at some distant part of the earth's surface.

I have said that the quantity of absorbed heat is very large. It varies with the temperature of the water that is evaporating, and is the greater the lower that temperature. From water that is on the point of freezing it is such that one grain of water absorbs in evaporating as much heat as would raise nearly 5½ grains from the freezing to the boiling point. This is called the latent heat of water vapour. As I have said, it is quite insensible. The vapour is no warmer than the water that produced it, and this enormous quantity of heat has been employed simply in pulling the molecules of water asunder and setting them free in the form of vapour, which is merely water in the state of gas. All liquids absorb latent heat when they evaporate, but no other known liquid requires so much as water.

Many things familiar in everyone's experience find their explanation in this absorption of latent heat. For instance, we feel colder with a wet skin than with a dry one, and wet clothes are a fruitful source of chills when the body is in repose; although, so long as it is in active exercise and producing a large amount of heat, since the evaporation

[1] A Lecture delivered by H. F. Blanford, F.R.S., at the Hythe School of Musketry on November 13.

only carries off the excess, no ill consequence may ensue. Again, if a kettle be filled with ice-cold water and put on a gas stove, suppose it takes ten minutes to bring it to boil. In that ten minutes the water has absorbed as much heat as raises it from 32° to 212°, an increase of 180°. Now, if it be left boiling, the gas-flame being kept up at the same intensity, we may assume that in every succeeding ten minutes the same quantity of heat is being absorbed by the water. But it gets no hotter: it gradually boils away. And it takes nearly an hour, or more than five times as long as it took to heat it, before the whole of the water has boiled away, since all this heat has been used up in converting it into steam. It was by an experiment of this kind that Dr. Black, in the last century, discovered the fact of latent heat, and determined its amount; and it was the knowledge of this fact that led James Watt to his first great improvement in the steam-engine.

One more example I may give, which those who have been in India will be able to appreciate, and which those who intend to go there may some day find useful to know. Nothing is more grateful in hot dry weather than a drink of cold water. Now, ice is not always to be had, but when a hot wind is blowing, nothing is easier than to get cold water, if you have a pot or bottle of unglazed earthenware, such as are to be had in every bazaar, or, what is better, a leather water-bottle, called a *chhàgal*, or a water-skin. All these allow the water to soak through and keep the outside wet; and if any one of them be filled with water and hung up in a hot wind, in the course of half an hour or an hour, the evaporation from the outside will have taken away so much heat that the contents may be cooled 20° or 30°, notwithstanding that the thermometer may stand at 110° or 115° in the shade. Soda-water may be cooled in the same way if wrapped in straw and kept well wetted while exposed to the wind. But it is of little use to do as I have seen natives do sometimes, viz. put the bottles into a tub of water in a closed room. It is the evaporation that carries off the heat, otherwise the water is no cooler than the air around.

Now to return to our subject. The atmosphere always contains some vapour which the winds have taken up from the ocean, lakes, rivers, and even from the land, for there are but few regions so dry and devoid of vegetation that there is no moisture to evaporate. The quantity of water thus evaporated from large water surfaces is a question of some importance to engineers, who have to take account of the loss from reservoirs and irrigation tanks, and a good deal of attention has been given to measure the amount lost by evaporation. In England it has been found to vary in different years from 17 to 27 inches in the year, or say from 1½ to 2¼ inches per month on an average. Now, since in the east of England the rainfall is only about 24 inches in the year, it follows that in that part of the Kingdom the loss by evaporation from a water surface is not very much less than the rain falling directly on the surface.

In dry countries the evaporation may exceed the local rainfall. In the tropics it has been found to average from 3½ to 6 inches per month in the dry season. In the case of a large tank at Nagpur, constructed to supply the city with water, it was found that the loss by evaporation, in the hottest and driest weather, was two and a half times as great as the quantity supplied for consumption.

These statistics will give some idea of the enormous evaporation that goes on from the water surfaces of the globe, and to this must be added all that takes place from the land. In the case of light showers, nearly the whole of the rain is re-evaporated; and probably, on an average, half of the total rainfall on the land is thus lost sooner or later, leaving not more than half for the supply of springs and rivers.

The quantity of vapour in the air is very variable. To us, in England, the west and south-west winds are the dampest, coming direct from the Atlantic, and north-east winds are the driest. The cause of their extreme dryness I shall endeavour to explain presently. It is no doubt partly due to the fact that they reach us from the land surface of Europe, but partly also to another cause to which I shall have to advert later on.

The quantity of vapour in the air is usually ascertained by the hygrometer, the ordinary form of which is a pair of thermometers, one having the bulb wet, the other dry, and observing the depression of the wet bulb. The principle of this I have already explained. But the same thing may be ascertained more directly by passing a measured quantity of air through a light apparatus containing sulphuric acid, or some other substance that absorbs water vapour greedily, and weighing the whole before and afterwards. The increase of the second weighment gives the weight of water absorbed. By such means it has been ascertained that air at 60° can contain as much as 5¾ grains of vapour in each cubic foot, and that air at 80° can contain rather less than 11 grains in the same space. The quantity that air can hold increases therefore very rapidly with the temperature. But it is seldom that it contains this maximum amount, especially at the higher temperatures.

In order to condense any part of this vapour we must take away its latent heat. It is not sufficient merely to cool it till it reaches the temperature of condensation, but we have further to abstract 5½ times as much heat as would raise the condensed water from the freezing to the boiling point. Before, however, proceeding to consider how this cooling is effected, the question arises, What is the condensing point? For, obviously, since water can evaporate at all temperatures, so we should expect that it may condense at all temperatures. On what, then, does the condensing point depend?

I mentioned just now that air at the temperature of 60° can contain as much as 5¾ grains of vapour, and at 80° rather less than 11 grains in each cubic foot. Obviously, then, if air at 80°, containing this maximum quantity, be cooled to 60°, it must get rid of more than 5 grains, or nearly half its vapour, and this excess must be condensed. I speak of air containing these quantities, but in point of fact it makes no appreciable difference whether air be present or not. An exhausted glass vessel of one cubic foot capacity can hold 5¾ grains of vapour at 60° and no more, and nearly 11 grains at 80° and no more; and if, when thus charged at 80°, its contents be cooled to 60°, more than 5 grains will be condensed. If, however, it contain only 5¾ grains at 80°, none will condense until the temperature falls to 60°, but any further cooling produces some condensation. Thus, then, the condensing point depends on the quantity of vapour present in the air, and is the temperature at which this quantity is the maximum possible for that temperature.

This preliminary point being explained, we may now proceed to inquire what means Nature employs to condense the vapour in the air, producing at one time dew and hoar-frost, at another time fog and cloud, and at another rain, hail, and snow.

Let us take the case of dew and hoar-frost first, as they are comparatively simple. And in connection therewith I may relate a little incident that took place at Calcutta some years ago. A gentleman, who had not much acquaintance with physical science, was sitting one evening with a glass of iced brandy and water before him. It was in the rainy season, when the air, though warm, is very damp, and he had a large lump of ice in his tumbler. On taking it up, he noticed to his surprise that the glass was wet on the outside, and was standing in quite a little pool of water on the table. At first he thought his tumbler was cracked, but putting his finger to his tongue he found the fluid tasteless. "Very odd!" he remarked; "the water comes through the glass but the brandy doesn't."

Now, however with our present knowledge we may be inclined to smile at the simplicity of this remark, it so happens that up to the end of the last century very much the same explanation was popularly held to account for dew. It was supposed to be a kind of perspiration emitted from the earth, and no satisfactory explanation of the phenomenon had been arrived at by the physical philosophers of the day. It remained for Dr. Wells to prove, by a long series of observations and experiments, which have been quoted by Sir John Herschel and Mr. John Stewart Mill as a typical instance of philosophical inquiry, that the cold surface of grass and shrubs condenses the vapour previously held in suspension in the air, these surfaces being cooler than the air, and below its point of condensation. And such, of course, is also the case of the glass tumbler containing ice. Anyone may try the experiment for himself. To produce hoar-frost, it is only necessary to cool the condensing surface below the freezing point, which may be done by crushing some ice and mixing it with salt. A tin pot is better than a glass to make this experiment.

When not only the ground, but also the air to a considerable height above it, is cooled in like manner, we have the production of fog, fog being the form in which the vapour is first condensed, and consisting of water in drops too minute to be separately visible. The formation of fog is very much aided if the air be laden with smoke. Smoke consists of extremely minute particles of unburnt coal or other fuel, and these cool faster than the air at night, and so cool the air in contact with them. Each one of them, too, condenses water on its surface, and being thus weighted they sink and form that dense fog that Londoners know so well.

Clouds are essentially the same as fog, but formed high up in the air. But in their case, and that of rain, snow, and hail, another and different cooling agency comes into play, and this will require some preliminary explanation.

I dare say that some of you may at some time or other have charged an air-gun. And if so, you will be aware that when so charged the reservoir becomes pretty warm. Now this heat is produced, not, as might be supposed, by the friction of the piston in charging, but is due to the fact that work has been done upon the air by compressing it into a very small space; in other words, work has been converted into heat. If the compressed air be allowed to escape at once, its heat is re-converted into work. It has to make room for itself by thrusting aside the atmosphere into which it escapes, and when thus expanded it is no warmer than before it was compressed. Indeed, not so warm, for it will already have parted with some of its heat to the metal chamber which contained it. And if when compressed it is allowed to cool down to the ordinary temperature, and then to escape, it will be cooled below that temperature just as much as it was heated by compression. Thus, if in being compressed it had been heated 100°, say from 60° to 160°, and then allowed to cool to 60°, on escaping it will be cooled 100° below 60°, or to 40° below zero, which is the temperature at which mercury freezes. This is the principle of the cold air chambers now so extensively employed on ship-board for the transport of frozen provisions from New Zealand and Australia.

Bearing in mind, then, this fact—that air in expanding and driving aside the air into which it expands is always cooled—let us see how this applies to the case before us, the production of cloud and rain.

The volume of a given weight of air—in other words, the space it occupies—depends on the pressure to which it is subject: the less this pressure the greater its volume. If we suppose the atmosphere divided into a number of layers superimposed on each other, the bottom layer is clearly subject to the pressure of all those that rest on it. This is equal to about $14\frac{3}{4}$ pounds on every square inch of surface. Another layer, say 1000 feet above the ground, will clearly be under a less pressure, since 1000 feet of air are below it; and this 1000 feet of air weighs slightly less than half a pound for every square inch of horizontal surface. At 2000 feet the pressure will be less by nearly one pound per square inch, and so on. If, then, any mass of air begins to ascend through the atmosphere, it will be continually subject to less and less pressure as it ascends; and therefore, as we have already seen, it expands, and becomes cooler by expansion. Cooling from this cause is termed dynamic cooling. Its rate may be accurately computed from the work it has to do in expanding.

It amounts to 1° for every 183 feet of ascent if the air be dry or free from vapour, and if, as is always the case, it contains some vapour, the height will not be very much greater so long as there is no condensation. But so soon as this point is passed, and the vapour begins to condense as cloud, the latent heat set free retards the cooling, and the height through which this cloud-laden air must ascend to cool 1° is considerably greater, and varies with the temperature and pressure. When the barometer stands at 30 inches, and at the temperature of freezing, the air must rise 277 feet to lose 1°, and if the temperature is 60° nearly 400 feet.

Conversely, dry air descending through the atmosphere and becoming denser as it descends, since it is continually becoming subject to an increased pressure, is heated 1° for every 183 feet of descent; and fog and cloud-laden air at 30 inches of pressure and the freezing point will be warmed 1° in 277 feet only, or if at 60° nearly 400 feet of descent, owing to the re-evaporation of the fog or cloud and the absorption of latent heat.

Now let us see how these facts explain the formation of cloud; and first I will take the case of the common cumulus or heap-cloud, which is the commonest cloud of the day-time in fine weather.

When after sunrise the air begins to be warmed, the lowest stratum of the atmosphere, which rests immediately on the ground, is warmed more rapidly than the higher strata. This is because the greater part of the sun's heat passes freely through a clear atmosphere without warming it, and is absorbed by the ground, which gives it out again to the air immediately in contact with it. So soon as the vertical decrease of temperature exceeds 1° in 183 feet, the warm air below begins to ascend, and the cooler air above to descend, and this interchange gradually extends higher and higher, the ascending air being gradually cooled by expansion, and ceasing to rise when it has fallen to the same temperature as the air around it. This ascending air is more highly charged with vapour than that which descends to replace it, since, as was mentioned before, most land surfaces furnish a large amount of moisture, which evaporates when they are heated by the sun. This process goes on until some portion of the ascending air has become cooled to the point of condensation. No sooner does it attain this, than a small tuft of cumulus cloud appears on the top of the ascending current, and the movement which was invisible before now becomes visible. In a calm atmosphere each tuft of cloud has a flat base, which marks the height at which condensation begins, but it is really only the top of an ascending column of air. No sooner is this cloud formed than the ascent becomes more rapid, because the cooling which checked its further ascent now takes place at a much slower rate, and therefore the cloud grows rapidly.

On a summer afternoon when the air is warm and very damp, such cumulus cloud ascends sometimes to very great heights, and develops into a thunder-cloud, condensing into rain. Rain differs from fog and cloud only in the size of the water drops. In fog and cloud these are so minute that they remain suspended in the air. But as the cloud becomes denser, a number of them coalesce to form a rain-drop, which is large enough to overcome the friction of the air. It then begins to fall, and having to traverse an enormous thickness of cloud below, it grows larger and larger by taking up more and more of the cloud corpuscules, so that when finally it falls below the cloud it may have a considerable size.

Such, then, is the mode in which rain is formed in an ordinary summer shower; and the more prolonged rainfall of stormy wet weather is the result of a similar process, viz. the ascent and dynamic cooling of the moist atmosphere. But in this case the movement is on a far larger scale, being shared by the whole mass of the atmosphere, it may be, over hundreds or thousands of square miles; and to understand this movement we shall have to travel somewhat further afield, and to inquire into the general circulation of the great atmospheric currents set in movement by the sun's action in the tropics, and modified by the earth's diurnal rotation and the distribution of the continents and oceans on its surface.

Before, however, entering on this subject, which will require some preliminary explanation, and in which we shall have to take account both of ascending and descending currents on a large scale, I will draw your attention to another and simpler case, in which both these classes of movements are prominently illustrated, and in which they exhibit their characteristic features in a very striking manner.

In the valleys of the Alps, more especially those to the north of the central chain, in Switzerland and the Tyrol, there blows from time to time a strong warm dry wind, known as the Föhn. It blows down the valleys from the central chain, melting the snows on its northern face, and although there is more or less clear sky overhead, all the southern slopes of the mountains are thickly clouded, and heavy rain falls on the lower spurs and the adjacent plain, replaced by snow at the higher levels up to the passes and the crest of the range. Cloudy weather also prevails to the north in Germany, and the weather is stormy over some part of Western Europe.

It is only since the general introduction of telegraphic weather reports and the construction of daily weather charts have enabled us to take a general survey of the simultaneous movements of the atmosphere over the greater portion of Europe, that this Föhn wind has been satisfactorily explained.[1] It is found that when a Föhn wind blows on the north of the Alps, the barometer is low somewhere to the north or north-west, in Germany, Northern France, or the British Isles, and high to the south-east, in the direction of Greece and the Eastern Mediterranean. Under these circumstances, since the winds always blow from a place of high barometer to one of low barometer, a strong southerly wind blows across the Alps. On their southern face it is forced to ascend, and therefore, as just explained, it is cooled and gives rain in Lombardy and Venetia, and snow at higher elevations. But having reached the crest of the mountains, it descends to the northern valleys, and being by this time deprived of a large part of its vapour, it becomes warmed in its descent, owing to compression, absorbs and re-evaporates the cloud carried with it, and is then further warmed at the rate of 1° for every 183 feet of descent. Thus it reaches the lower levels as a warm dry wind, its warmth being the effect of dynamic heating.

Other mountain chains afford examples of the same phenomenon. A very striking instance, which much impressed me at the time, is one that I witnessed many years ago in the mountains of Ceylon; and it was afterwards mentioned to me by Sir Samuel Baker, who had been equally struck by it. My own experience is as follows:—In June 1861, I paid a week's visit to the hill sanitarium of Newara Eliya, at an elevation of 6200 feet, on the western face of Pedro Talle Galle, the highest mountain in the island. The south-west monsoon was blowing steadily on this face of the range; and during the whole time of my stay it rained, as far as I am aware, without an hour's intermission, and a dense canopy of cloud enveloped the hill face, and never lifted more than a few hundred feet above the little valley in which Newara Eliya is built. But on leaving the station by the eastern road that leads across the crest of the range to Badulla, at a distance of five miles one reaches the *col* or dip in the ridge near Hackgalle, and thence the road descends some 2000 feet to a lower table-land which stretches away many miles to the east. No sooner is this point passed than all rain ceases and cloud disappears, and one looks down on the rolling grassy hills bathed in the sunshine of a tropical sun, and swept by the dry westerly wind that descends from the mountain ridge. In little more than a mile one passes from day-long and week-long cloud and rain to constant sunshine and a cloudless sky.

As an almost invariable rule, or at least one with few exceptions, ascending air currents are those that form cloud and rain, and descending currents are dry and bring fine weather. And this holds good whatever may be the immediate cause of these movements. We may now proceed to consider these greater examples to which I have already referred.

In the great workshop of Nature, in so far at least as concerns our earth, with but few exceptions, all movement and all change, even the movements and energies of living things, proceed either directly or indirectly from the action of the sun. Nowhere is this action more direct and more strikingly manifested than in the movements of the atmosphere. Were the sun extinguished, and to become, as perhaps it may become long ages hence, a solid cold sphere, such as Byron imagined, "wandering darkling in eternal space," a few days would suffice to convert our mobile and ever-varying atmosphere into a stagnant pall, devoid of vapour, resting quiescent on a lifeless earth, held bound in a more than Arctic frost. From such a consummation, despite the supposed decaying energy of our sun, we may, however, entertain a reasonable hope that we are yet far distant.

Bearing in mind the all-embracing importance of the sun, let us see how the great movements of the atmosphere are determined by the way in which the earth presents its surface to the solar rays.

Since the quantity of solar heat received on each part of the earth's surface depends on the directness or obliquity of his rays—in other words, on the height to which the sun ascends in the heavens at noon—being greatest where he is directly overhead, as in summer in the tropics, it follows that the hottest zone of the earth is that in the immediate neighbourhood of the equator, and the coldest those around the poles.

Did time allow, and were the necessary appliances at hand, it would be easy to show you that both as a matter of experiment, and also as a deduction from physical laws, there must be under such circumstances a flow of air from the colder to the warmer region in the lower atmosphere, and a return current above. And to a certain extent we have these constant winds prevailing for about 30° on either side of the equator, in the trade-winds, which blow towards the equator in the lower atmosphere, and the anti-trades blowing in the opposite direction at a great height above the earth's surface.

In the neighbourhood of the equator there is a zone extending right round the earth in which the barometer is lower than either to the north or the south. It is due to the greater heat of the sun, and it is towards this that the trade winds blow. It shifts to some extent with the seasons, being more northerly in the summer of the northern hemisphere, and more southerly in that of the southern hemisphere; and its average position is rather to the north of the equator, owing to the fact that there is more land in the northern than in the southern hemisphere, and that land is more heated by the sun than the ocean.

This simple wind system of the trades and anti-trades does not extend right round the earth, nor beyond 30° or

[1] The explanation was originally given by Prof. J. Hann, of Vienna.

40° of latitude in either hemisphere. Were the earth's surface uniformly land or uniformly water, there probably would be a system of trade-winds all round the globe, blowing from both hemispheres towards the equator; but even in that case they would not extend much, if at all, beyond their present limits. In the first place, every great mass of land sets up an independent system of air currents, since the land is hotter than the ocean in the summer, and colder in the winter. In the summer, therefore, there is a tendency to an indraught of air from the sea to the land in the lower atmosphere, and an outflow above, and in the winter the opposite; and this tendency modifies or interrupts the system of the trades and anti-trades. We have this tendency shown most distinctly in the monsoons of South-Eastern Asia, where, both in the India and China seas, a south-west wind in the summer takes the place which in the absence of the Asiatic continent would be held by a north-east trade-wind. And it is only in the winter that a north-east wind blows, and this is then termed the north-east monsoon.

In the second place, as I have said, the system of trade-winds could not in any case extend far beyond their present limits in latitude, owing to the fact that the earth is a sphere and not a cylinder. Let us fix our attention for a moment on the anti-trades—the upper winds which blow from the equator towards the poles. The equator, from which they start, is a circle about 24,900 miles in circumference; the poles are mere points, and, therefore, the whole of the air that blows towards the poles must turn back in any case before it reaches the pole, and must begin to turn back before it has gone very far on its journey. And, as a fact, a great part of it does turn back between 30° and 40° of latitude, which I have already mentioned as being the limit of the trade-winds. A part of the remainder descends to the earth's surface, and sweeps the Northern Atlantic and the North Pacific as a south-west wind.

On the chart which represents the average distribution of atmospheric pressure in January, there are two somewhat interrupted zones of high pressure over the ocean in these latitudes. These mark the regions in which the anti-trades descend to the earth's surface, and from which the trade-winds start. Over the ocean in all higher latitudes, both in the northern and southern hemispheres, the barometer is low—for the most part, indeed, much lower than over the equator; and the region intervening between the zones of high pressure and the seat of lowest pressure is that of predominant south-west, or at all events westerly, winds. Since our islands are situated on the border of this region of low pressure, south-west are our prevailing winds.

But now two questions arise: first, Why are these winds westerly, and not simply south winds? and second, How is it that the barometer is so low over the North Atlantic and North Pacific Oceans, and also in the southern hemisphere in high latitudes, seeing that in these latitudes, at least in winter, the sun's heat is so much less than at the tropics? The chart represents the state of things in mid-winter of the northern hemisphere, and yet everywhere to the north of latitude 40° the deep blue tint indicates that the pressure is lower than even in the southern tropic, where the sun shines vertically overhead. Clearly this low pressure must be due to some other cause than the warmth of the air.

The explanation of this remarkable distribution of the atmospheric pressure, of the existence of two zones of high pressure in latitudes 30° to 40°, and of very low pressure in higher latitudes, except in so far as they are modified by the alternations of land and water, was first given by the American physicist, Prof. Ferrel. Its full demonstration is to be obtained only from the consideration of somewhat recondite mechanical laws, but a general idea of the causes operating may be gathered from very simple considerations, which may be demonstrated with a terrestrial globe.

Starting with the well-known fact that the earth revolves on its axis once in the twenty-four hours, let us see what will be the consequence, if we suppose a mass of any ponderable matter—that is, any substance having weight, no matter whether light or heavy—to be suddenly transferred from the equator to latitude 60°.

As the circumference of the earth at the equator is about 24,900 miles, any body whatever, apparently at rest at the equator, is carried round the earth's axis at the rate of 1036 miles an hour. But in latitude 60°, where the distance from the axis is only half as great as at the equator, it is carried round at only half the same rate, or 518 miles an hour; and at the pole it simply turns round on its own axis. Supposing, then, a mass of air to be suddenly transferred from the equator to latitude 60°, with the eastward movement that it had at the equator, it would be moving twice as fast to the east as that part of the earth, and, to any person standing on the earth, would be blowing from the west with a force far exceeding that of a hurricane. It would be moving eastwards 518 miles an hour faster than the earth. Indeed, its movement would really be far greater than this. In virtue of a mechanical principle known as the law of the conservation of areas, which means that any body revolving round a central point, under the influence of a force that pulls it towards that point, describes equal areas in equal times, instead of only 518 miles, it would be revolving round the earth's axis 1554 miles an hour faster than that part of the earth. I need not, however, specially insist on this point, because, as a matter of fact, the air which constitutes the anti-trades is not suddenly transferred, but takes a day or two to perform its journey, and in the meantime by far the greater part of its eastward movement is lost by friction against the trade-wind which blows in the opposite direction underneath it. The point on which we have to fix our attention is that, when the anti-trades descend to earth, they still retain some of this eastward movement, and blow, not as south, but as south-west or west-south-west winds.

On the other hand, the trade-wind, which blows towards the equator, is coming from a latitude where the eastward movement is less than at the equator, and its own movement eastward is therefore less than that of the surface over which it blows. A person, therefore, standing on the earth, is carried eastward faster than the air is moving, and the wind seems to blow against him from the north-east. Similarly, to the south of the equator, the trade-wind, instead of blowing from the south, comes from the south-east.

Thus, then, we have in both hemispheres a system of westerly winds in all higher latitudes than 40°, and a system of easterly winds—viz. the trade-winds—between about 30° and the equator; and if the globe were either all land or all water, these systems would prevail right round the earth.

Now, it is the pressure of these winds, under the influence of centrifugal force, that causes the two zones of high barometer in latitudes 30° to 40°, and the very low pressure in higher latitudes. It is not difficult to understand how this comes about. You are probably aware that the earth is not an exact sphere, but what is termed an oblate spheroid—that is, it is slightly flattened at the poles and protuberant at the equator, the difference of the equatorial and polar diameters being about 26 miles. It has acquired this form in virtue of its rotation on its axis. If you whirl a stone in a sling, the stone has a tendency to fly off at a tangent, and, so long as it is retained in the sling, that tendency is resisted by the tension of the cord. In the same way, every object resting on the earth, and the substance of the earth itself, has a tendency to fly off at a tangent, in consequence of its rotation on its axis, and this tendency is resisted and overcome by gravity. Were the earth not revolving, its form, under the influence of gravity alone, would be a true sphere. If it were to revolve more rapidly than at present, it would be still more oblate, flatter at the poles, and more bulging in the tropical zone; if less rapidly, the flattening and bulging would be less.

This is precisely what happens with the west and east winds of which we have spoken. West winds are revolving faster than the earth, and tend to make the atmosphere more protuberant at the equator than the solid earth; hence they press towards the equator, to the right of their path in the northern hemisphere, and this tendency increases rapidly in high latitudes. Easterly winds, on the other hand, tend to render the form of the atmosphere more nearly spherical, and they, too, press to the right of their path in the northern hemisphere or towards the pole. In the southern hemisphere, for the same reason, both press to the left. The result of these two pressures in opposite directions is to produce the two zones of high barometer in the latitudes in which we find them—viz. between the easterly trade-winds and the westerly winds, which are the anti-trades that have descended to the earth's surface. And the low barometer of higher latitudes is produced in like manner by the westerly winds pressing away from those regions.

Thus, then, we find that all this system of winds, and the resulting distribution of atmospheric pressure as indicated by the barometer, is the result of the sun's action in equatorial regions. It is this that gives the motive power to the whole system, so far as we have as yet traced it, and it is this that produces those great inequalities of atmospheric pressure that I have so far described.

It remains now to see how storms are generated by these westerly winds. In so far as they retain any southing, they are still moving towards the pole in the northern hemisphere—that is to say, they are advancing from all sides towards a mere point. Some portion of them must therefore be continually turning back as the circles of latitude become smaller and smaller. But they are now surface-winds, and in order so to return they must rise and flow back as an upper current. This they do by forming great eddies, or air-whirls, in the centre of which the barometer is very low, and over which the air ascends, and these great air-whirls are the storms of the temperate zone and of our latitudes. It is the ascent and dynamic cooling of the air in these great eddies that cause the prolonged rainfall of wet stormy weather. How the eddies originate, or, rather, what particular circumstance causes them to originate in one place rather than another, we can scarcely say, any more than we can say how each eddy originates in a rapidly-flowing deep river. Some very small inequality of pressure probably starts them, but, when once formed, they often last for many days, and travel some thousands of miles over the earth's surface.

Two such storms are represented on the charts of February 1 and 2, 1883, one on the coast of Labrador, the other to the south-west of the British Isles. The first of these appears on the chart of January 28, in the North Pacific, off the coast of British Columbia. On the 29th it had crossed the Rocky Mountains, and was traversing the western part of the Hudson's Bay Territory. On the 30th it had moved to the south-east, and lay just to the west of the Great Lakes, and on the 31st between Lake Superior and Hudson's Bay. On February 1 it had reached the position on the coast of Labrador shown in the chart, and on the 2nd had moved further to north-east, and lay across Davis's Straits, and over the west coast of Greenland. After this it again changed its course to south-east, and on February 4 passed to the north of Scotland, towards Denmark, and eventually on to Russia.

The second storm had originated off the east coast of the United States between January 28 and 29, and on the following days crossed the Atlantic on a course somewhat to north of east, till, on February 2, it lay over England.

These storms always move in some easterly direction, generally between east and north-east, and often several follow in rapid succession on nearly the same track. It is this knowledge that renders it possible for the Meteorological Office to issue the daily forecasts that we see in the newspapers. Were it possible to obtain telegraphic reports from a few stations out in the North Atlantic, these storm warnings could be issued with much more certainty, and perhaps longer before the arrival of the storm than at present. In the case of such storms as that which reached our islands on February 2, we often have such warnings from America, but their tracks are often more to the north-east, in the direction of Iceland, in which case they are not felt on our coasts, and hence the frequent failure of these American warnings.

It is the region of low pressure in the North Atlantic that is the especial field of these storms. As they pass across it, they produce considerable modifications in the distribution of pressure, but some of its main features remain outstanding. Thus there is always a belt of high barometer between the storm region and the trade-winds, and in the winter there is almost always a region of high barometer over North America, and another over Europe and Asia, however much they may shift their places, and be temporarily encroached on by the great storm eddies.

These regions of high pressure are the places where the winds descend, and, as I mentioned in the earlier part of this lecture, these winds are dry, and generally accompany fine weather. On the contrary, the eddies, where the air ascends, are damp and stormy, and especially that part of the eddy that is fed by the south-west winds that have swept the Atlantic since their descent, and so have become charged with vapour.

And now we are prepared to understand why east, and especially north-east winds are generally so dry. They are air that has descended in the area of high barometer that, especially in the winter and spring, lies over Europe and Asia, and has subsequently swept the cold land-surface, which does not furnish much vapour, and therefore they reach us as dry cold winds. To begin with, the air comes from a considerable height in the atmosphere, and in ascending to that height in some other part of the world, it must have got rid of most of its vapour in the way that has been already explained. In descending to the earth's level it must, of course, have been dynamically heated by the compression it has undergone, but all or nearly all this heat has been got rid of by radiation into free space on the cold plains and under the clear frosty skies of Northern Asia and Northern Europe, and it then blows outwards from this region of high barometer over the land, towards the warmer region of low barometer on the North Atlantic Ocean.

Thus we see that, in all cases, rain is produced by the cooling of the air, and that in nearly all, if not all, this cooling is produced by the expansion of the air in ascending from lower to higher levels in the atmosphere, by what is termed dynamic cooling. This last fact is not set forth so emphatically as it should be in some popular text-books on the subject, but it is an undoubted fact. It was originally suggested by Espy some forty years ago, but the truth is only now generally recognized, and it is one of the results which we owe to the great advance in physical science effected by Joule's discovery of the definite relation of equivalence between heat and mechanical work.

THE SOARING OF BIRDS.[1]

So much for sails. Now I want to make some suggestions, or suggest some queries, as to the *skimming* flight of birds, in reference to which a good deal of fresh observation has been possible during the voyage.

You perhaps recollect that when the British Association was at Glasgow, you asked me to put into writing, briefly, as a paper for your Section, some remarks on this subject which I had made to you in conversation, but that, owing to my hasty departure to attend the trial of H.M.S. *Shah*, I omitted to do this.

I had better briefly recite the above particulars here in order to make more clear the bearing of the new observations we (I and Tower) have made.

The view was that when a bird skims or soars on quiescent wings, without descending and without loss of speed, the action must depend on the circumstance that the bird had fallen in with, or selected a region where the air was ascending with a sufficient speed. In still air the bird, if at a sufficient height, could continue to travel with a steady speed, using his extended wings as a sort of descending inclined plane, the propelling force depending on the angle of the plane and on the equivalent of "slip,"—that is to say, on the excess of the angle of actual descent compared with the angle of the inclined plane. The steady speed would be attained when the weight of the bird and the sines of the angle of the plane = the bird's *air resistance*, including skin friction of wings—in fact one might say = simply the skin friction of the whole area, for the bird's lines are fine enough to justify this statement, since there is no wave-making to be done, and indeed experiment shows that the statement is true for "fish-formed" bodies moving wholly and deeply immersed in water. Of course the bird's angle of actual descent is greater than that of the quasi-inclined plane, owing to the equivalent of "slip" in the wings. Under these simultaneously acting and correlated conditions there is of course—or probably—some total angle of descent which enables the bird to minimize his rate of approach to the earth in still air. If when there is a wind the configuration of the ground or any other circumstances can produce a local ascent of air more rapid than the bird's minimum rate of descent when soaring in still air, he may continue to soar indefinitely by keeping in the region where the air is thus ascending.

Now, in most cases where one sees birds "soaring," it is easy to see that they have plainly selected such a region, and for a long time I felt confident that the only two even apparent exceptions I had encountered were such as to *prove* not to *invalidate* the rule. One of these exceptions was that once, when the sea in Torbay was in a state of glassy calm, I noticed a large gull thus soaring at some distance from the shore,—watching it with a pair of binoculars, so that I was sure of the quiescence of the wings. But here the riddle was at once solved by the observation of what I had not at first noticed—the dark trace of the front line of a fresh sea-breeze advancing all across the bay. Such an advance with a definitely marked front, encountering an extended body of quiescent air, involved of course an ascent of air in the region of the encounter, and this was where the bird was soaring. The other exception was that when at sea I had often noticed birds thus soaring near the ship. The solution was that, so far as I had then noticed, the birds always selected a region to leeward of the ship, where the eddies created by the rush of air past her hull, &c., might readily have created local ascending currents.

The new exceptions we have seen since we have approached the Cape entirely set these two solutions at defiance.

[1] Extract from a letter of the late William Froude to Sir W. Thomson, of February 5, 1878, received after Mr. Froude's death. Reprinted from the Proceedings of the Royal Society of Edinburgh, March 19, 1888.

The first exception we noticed was in the flight of some albatrosses. We were sailing, and steaming (at low speed, being short of coal), nearly due east in the latitude of the Cape, with the wind light and variable abaft the beam, and with a well-marked south-west swell of about 8 to 9 seconds period, and varying from 3 or 4 feet to 8 or 9 feet from hollow to crest. The speed of such waves would be from 24 to 27 knots.

Under these conditions the birds *seemed* to soar almost *ad libitum* both in direction and in speed; now starting aloft with scarcely, if any, apparent loss of speed; now skimming along close to the water, with the tip of one or other wing almost touching the surface for long distances, indeed now and then actually touching it. The birds were so large that the action could be clearly noted by the naked eye even at considerable distances; but we also watched them telescopically, and assured ourselves of the correctness of our observations. The action was the more remarkable owing to the lightness of the wind, which sometimes barely moved our sails, as we travelled only 5 knots before it, by help of the screw.

After long consideration the only explanation of at all a rational kind which presented itself was the following, which indeed presents the action of a *vera causa*, and one which was very often certainly in accordance with the birds' visible movements, though it was often also impossible either to assert or to deny the accordance; and anyhow the question arises, Is the *vera causa* sufficient? I will try to trace its measure.

When a wave is say of 10 feet in height and say 10 seconds period (a case near enough to ours to form the basis of a quantitative illustration) the length of the wave from crest to crest is just 500 feet, the half of which space, or 250, the wave of course traverses in 5 seconds, and assuming the wave to be travelling in a calm, it must happen approximately that during the lapse of this 5 seconds the air which at the commencement of the interval lay in the lowest part of the trough has been lifted to the level of the crest, or must have risen 10 feet, so that its mean speed of ascent has been 2 feet per second (10 feet in 5 seconds). And since (as is well known) the maximum speed of an harmonic motion is $\frac{\pi}{2}$ times, or nearly $1\frac{1}{2}$ times its mean speed, it follows that along the side of the wave at its mid-height the air must approximately be ascending at the rate of 3 feet per second, and if the bird were so to steer its course and regulate its speed as to conserve this position he would have the advantage of a virtual upward air current having that speed.

NOTES.

The Berlin Academy of Sciences has presented 2000 marks (£100) to Prof. Leopold Auerbach (Breslau), and the same amount to Dr. Franz Schütt (Kiel), to aid them in their physiological researches. Dr. Freudenthal, Professor of Philosophy at Breslau, and Herr von Rebaur-Paschwitz, the astronomer, have received 1500 marks (£75) each.

At the last meeting of the Scientific Committee of the Royal Horticultural Society, Mr. Henslow called attention to the fact that the year 1889, besides being the centenary of the chrysanthemum in Europe, is that of the dahlia in England. It was introduced by the Marchioness of Bute in 1789, and figured with single and double forms in the *Botanical Magazine*, vol. xliv., t. 1885, and the *Botanical Register*, vol. i. t. 55.

The death of Mr. J. J. Coleman, F.R.S.E., is announced. He died at the age of fifty. For some time he was manager of the works of Young's Paraffin and Mineral Oil Company, Glasgow; and in this capacity he carried out some important experimental investigations for the utilization of so-called waste products. Afterwards he invented the refrigerating machine which bears his name, a machine which has increased and cheapened the available supply of fresh meat.

ON Tuesday, the 8th inst., the second of the series of one-man photographic exhibitions at the Camera Club will be open to visitors on presentation of card. The Exhibition will continue for about six weeks. The object of this series of Exhibitions is to bring together in turn representative collections of the work of the best photographic artists. By the co-operation of Mr. Harry Tolley, of Nottingham, the Camera Club is enabled to exhibit a representative set of his photographs. These pictures are large direct work, and are printed in the permanent platinum process.

A COURSE of six lectures on "The Science of Brewing" will be given at the Finsbury Technical College by Dr. E. R. Moritz, commencing Wednesday, January 23, and being continued on successive Wednesday evenings. Other courses of special lectures will be given during the present term by Prof. Perry, on "The Differential Calculus and its Application to Problems of Electrical and Mechanical Engineering"; by Prof. S. P. Thompson, on "Optical Principles and Practice"; and a special laboratory course on "Electro-deposition" (plating and typing), by Mr. Rousseau.

ACCORDING to the *Standard* of December 29, 1888, Hampshire was visited, at 11 o'clock on the morning of the 28th, with what was believed to be an earthquake. There were, it is reported, a severe subterranean rumbling and a concussion in the neighbourhood of Emsworth Common. A horse and cart passing at the time were visibly shaken, and two men were nearly knocked off their legs. There was a violent rustling of the trees in the neighbourhood, and the shock appeared to extend over a wide area.

ON Sunday, December 23, a severe shock of earthquake was experienced in Calcutta and throughout Bengal. In Rajshahye large fissures opened, whence hot liquid mud was ejected. The Calcutta Correspondent of the *Times*, reporting these facts by telegraph on December 30, said no loss of life had been heard of.

SEVERAL severe shocks of earthquake were felt in Bosnia, on December 18, especially at Rogatica, Cajnica, Pleolje, and Poljancic.

THE University of Edinburgh continues to attract a very large number of students. During the past year, according to the Edinburgh Correspondent of the *Times*, the total number of matriculated students was 3532, as against 3459 last year, 2667 in 1878, and 1564 in 1868. Of this total, 1008 were enrolled in the Faculty of Arts, 108 in the Faculty of Divinity, 474 in the Faculty of Law, and 1942 in the Faculty of Medicine. Of the students of medicine 832 (or 43 per cent.) belonged to Scotland, 705 (or fully 36 per cent.) were from England and Wales, 36 from Ireland, 79 from India, 247 (or nearly 13 per cent.) from British colonies, and 43 from foreign countries.

AT the Bath meeting of the British Association, a Report was presented by the Committee which had been appointed to consider the advisability and possibility of establishing in other parts of the country observations upon the prevalence of earth tremors similar to those now being made in Durham. Considering that much is being done with the object of securing suitable forms of instruments, and that these investigations are still incomplete in many ways, the Committee felt that it would be premature for them to select and recommend any special recorder at present. They, however, emphasized the view that, whilst carefully finished, highly sensitive, and necessarily expensive seismoscopes, made to record with as much accuracy as possible the time, form, and intensity of each set of tremors, are very desirable, and indeed indispensable, yet only a comparatively small number of such instruments would be required in a general scheme of seismographical observatories. Such instruments, moreover, could only be used with effect in carefully selected situations, and otherwise under very special conditions. On the other hand, comparatively rough, cheap, and easily used instruments, which could do little more than afford fairly accurate time-records, would be required in large numbers, and must form a most important portion of such a scheme. The Committee hoped that at the next meeting of the Association they might be in a position to present a Report containing definite recommendations.

THE Chief Signal Officer of the United States reports that in the reorganization of the Record Division of the Signal Office he found a large amount of valuable rainfall data, furnished by voluntary observers prior to 1874. With a view of making these records available, he has published them in an atlas of rain charts of the United States, for each month of the years 1870–74; these will be found especially useful in the study of weather conditions over that country. He also states that an examination of the records of the voluntary observers shows that it will be possible to further utilize them in the preparation of normal temperature charts, which he hopes to be able to issue with the Monthly Weather Review at an early date. An eighteen-year normal monthly rain-chart is now being regularly issued in this Review.

IN December 1877, Prof. F. E. Nipher established a volunteer weather service in Missouri, the object being primarily to investigate the rainfall of that State, and he has now published the results in a paper entitled "Missouri Rainfall." It contains maps and tables showing the average monthly amounts for ten years ending 1887, at thirty-one stations, together with the maximum and minimum monthly and yearly falls. The yearly averages vary from 31·4 inches to 45·7 inches.

IN a recent lecture on Bacteria, delivered at Brooklyn, Dr. George M. Sternberg pointed out that the rapid progress of bacteriology in Germany has been due, to a very considerable extent, to the enlightened policy of the Government. Dr. Sternberg is of opinion that if, during the past ten years, the Americans had had a well-equipped laboratory, under proper direction, the medical corps of the army and navy could easily have supplied men who would have done good work in this department of research. He thinks it is not creditable to the United States as a nation that Americans have contributed so little to the advance of bacteriology. "Let us hope, however," he added, "that we are entering upon a new era. Here in Brooklyn private munificence has provided the means of research which the National Government should have provided long since; and here, at least, the fault will rest with the profession, if active workers are not found to avail themselves of the facilities provided for making original researches in bacteriology, in physiology, and in experimental pathology." The "private munificence" referred to is that of Dr. Hoagland, who has equipped a laboratory at Brooklyn. This laboratory, which has been organized in accordance with the best models, is to be devoted exclusively to scientific research, and to instruction in physiology, pathology, histology, and bacteriology.

THE tenth volume of the Proceedings of the United States National Museum has been issued. The series to which this volume belongs consists of papers prepared by the scientific corps of the National Museum; of papers by others, founded upon the collections in the National Museum; and of brief records of interesting facts from the correspondence of the Smithsonian Institution.

THE new number—the sixth—of the *Internationales Archiv für Ethnographie* completes the first volume of this excellent periodical. Among the plates is a coloured representation of the deerskin mantle, ornamented with-shell work, recorded to have belonged to the Virginian Chief, Powhattan. Dr. E. B. Tylor contributes, in English, an account of this interesting object, which forms one of the treasures of the Ashmolean. It belongs to the Tradescant collection, which was the nucleus of the museum of Elias Ashmole. It seems that there were in use among the Powhattans three kinds of mantles, viz. of dressed skins embroidered with beads (including shells), of furs, and of feather work. In the original collection of Tradescant there were specimens of all three kinds. Of these, the shell-embroidered mantle alone remains. It measures about 2·2 m. in length by 1·6 m. in width. The two deerskins forming it are joined down the middle; no hair remains. The ornamental design consists of an upright human figure in the middle, divided by the seam; a pair of animals; thirty-two spirally formed rounds (two in the lowest line have lost their shells); and the remains of some work in the right lower corner. Dr. Tylor says that the decorative shell-work is of a kind well known in America. The shells used are *Marginella*: so far as Mr. Edgar A. Smith is able to identify them in their present weathered state, *M. nivosa*. Among the other contents of the number are some notes, in German, on the ethnography of Mexico, by Carl Breker, and an attempt, by M Messikommer, to describe some elements of what may have been the intellectual life of the inhabitants of ancient lake-dwellings.

In the new number of the Transactions of the Leicester Literary and Philosophical Society, there is a useful and interesting paper, by Mr. Montagu Browne, Curator of the Town Museum, Leicester, on "Evidences of the Antiquity of Man in Leicestershire." He begins with objects of bone and horn, then examines the remains of pottery, and finally deals with articles in bronze and stone. Palæolithic implements have not yet been discovered in Leicestershire, but Mr. Evans, in a passage quoted by Mr. Browne, is of opinion that they may be found there. "It is by no means impossible," writes Mr. Evans, "that you may succeed in finding them. It is a little far north, but I doubt whether the glaciers persisted so long in that part of England as they did in the Lake District, and in Wales."

Messrs. Cassell and Co. are issuing, in monthly parts, a popular edition of "The Story of the Heavens," by Sir Robert S. Ball, the Royal Astronomer of Ireland. The work, which is well printed on good paper, will be completed in eighteen parts. With Part I., which we have just received, a star map is given.

The "Educational Annual" for 1889, compiled by Edward Johnson, has been issued. Messrs. G. Philip and Son are the publishers. The work is designed to place within reach of the general public a concise summary of authentic information, drawn from official or other trustworthy sources, relative to primary and secondary education, in a form convenient for reference. Information relating to training colleges for teachers and teachers' associations has been included.

A book, entitled "Rides and Studies in the Canary Islands," by Mr. Charles Edwardes, has just been published (Fisher Unwin). The author quaintly explains that it is written "for the entertainment both of those who visit the Canary Islands and those who do not." A considerable part of the book has already appeared in the form of articles in magazines and journals. Those who have already read these fragments will not object to find them again among Mr Edwardes's lively and pleasant sketches.

We have received a diary called "The Perennial Diary," which a good many people may find useful. It is not intended to supersede ordinary diaries. Each page is devoted to a single day of the year, and events occurring on that day in different years may al be entered on the same page. The volume is issued by Mr. John Heywood, of Manchester and London.

The Free Libraries Committee of Manchester are able to give, in their thirty-sixth Annual Report, a most favourable account of all departments of the institutions under their charge. The number of the buildings in which the work of the Committee is carried on has been increased to ten by the establishment of the Hyde Road Reading Room, which was opened some time ago by the Mayor, in the presence of a large and enthusiastic meeting of the inhabitants of the district. In the course of the last twelve months the number of readers at the various libraries and reading-rooms (*i.e.* the number of visits they have made) reached an aggregate of nearly four millions and a half, or about a quarter of a million more than in the previous year. There has been an increase also in the number of books read. The number used for home reading and for perusal in the reading-rooms has been 1,606,874, against 1,462,028 volumes read in the preceding twelve months. The daily average of volumes used in all the Libraries was 4464.

In the letter on "Nose-blackening as preventive of Snow-blindness," by Mr. A. J. Duffield (vol. xxxviii. p. 172), *for* "New Zealand" *read* "New Ireland."

The additions to the Zoological Society's Gardens during the past week include a Common Kestrel (*Tinnunculus alaudarius*) captured at sea, presented by Mr. Thomas Austin; a Pyxis Tortoise (*Pyxis arachnoides*) from Durban, Natal, presented by Colonel J. H. Bowker, F.Z.S.; a Rat-tailed Snake (*Trigonocephalus lanceolatus*) from St. Lucia, W.I., presented by the West Indian (Natural History) Exploration Committee; two Concave-casqued Hornbills (*Buceros bicornis*) from India, deposited; a —— Squirrel (*Sciurus* sp. inc.) from Burmah, two Ceylonese Hanging Parrakeets (*Loriculus asiaticus*) from Ceylon, purchased.

OUR ASTRONOMICAL COLUMN.

Detection of New Nebulæ by Photography.—Prof. Pickering gives a brief account, in No. 6 of the Annals of Harvard College Observatory, vol. xviii., of some experiments he has recently conducted as to the advantages of a photographic doublet over an ordinary astronomical object-glass for astronomical work, and especially in photographing nebulæ. A number of plates were exposed upon the region of Orion, the instrument used being the Bache telescope, which has a photographic doublet with an aperture of 8 inches and a focal length of 44 inches; each plate covered a region 10° square, the definition being good within $3\frac{1}{2}$° of the centre of the plate. The result of the experiments was the detection of twelve new nebulæ; fourteen nebulæ were seen on the photographs that were also given in Dreyer's Catalogue, and four nebulæ in the Catalogue were not represented on the plates. A similar proportion of discovery over the entire sky would mean some 4000 or 5000 new nebulæ, and 400 plates would be sufficient for a complete survey, provided here was no overlapping, and no plates proved defective.

Comets Faye and Barnard, October 30.—The following ephemeris for Faye's comet is in continuation of that given in Nature (vol. xxxix. p. 186). The ephemeris for Barnard's comet is by Herr Spitaler (*Astr. Nach.*, No. 2871). Both are for Berlin midnight:—

	Comet 1888 *d* (Faye).		Comet 1888 *f* (Barnard, Oct. 30).	
1888.	R.A. h. m. s.	Decl. ° ′	R.A. h. m. s.	Decl. ° ′
Jan. 4	7 58 38	0 11·7 N.	10 25 49	4 42·3 N.
6	7 56 57	0 14·2	10 24 55	5 45·1
8	7 55 15	0 17·7	10 23 51	6 49·1
10	7 53 32	0 22·3	10 22 40	7 54·4
12	7 51 49	0 27·9	10 21 22	9 0·6
14	7 50 6	0 34·5	10 19 58	10 7·6
16	7 48 26	0 42·1 N.	10 18 24	11 15·2 N.

Both comets are slowly diminishing in brightness.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1889 *JANUARY* 6-12.

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

At Greenwich on January 6

Sun rises, 8h. 7m.; souths, 12h. 6m. 17·2s.; sets, 16h. 6m.: right asc. on meridian, 19h. 11·3m.; decl. 22° 26′ S. Sidereal Time at Sunset, 23h. 12m.

Moon (at First Quarter January 9, 1h.) rises, 11h. 2m.; souths 16h. 22m.; sets, 21h. 53m.: right asc. on meridian, 23h. 27·9m.; decl. 8° 22′ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. and declination on meridian. h. m.	° ′
Mercury..	8 39 ...	12 30 ...	16 21 ...	19 35·5 ...	23 46 S.
Venus.....	10 7 ...	15 2 ...	19 57 ...	22 7·2 ...	13 15 S.
Mars......	10 0 ...	14 55 ...	19 50 ...	22 1·0 ...	13 16 S.
Jupiter....	6 32 ...	10 28 ...	14 24 ...	17 32·8 ...	22 56 S.
Saturn....	18 55* ...	2 24 ...	9 53 ...	9 27·8 ...	16 6 N.
Uranus...	0 54 ...	6 17 ...	11 40 ...	13 21·6 ...	7 55 S.
Neptune..	13 2 ...	20 45 ...	4 28* ...	3 52·0 ...	18 27 N.

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

Saturn, January 6.—Outer major axis of outer ring = 45″·1: outer minor axis of outer ring = 11″·0: southern surface visible.

Variable Stars.

Star.	R.A. (1889·0) h. m.	Decl. (1889·0) ° ′		h. m.	
U Cephei	0 52·5 ...	81 17 N. ...	Jan. 8,	21 53	*m*
R Tauri	4 22·3 ...	9 55 N. ...	,, 9,		*M*
ζ Geminorum ...	6 57·5 ...	20 44 N. ...	,, 7,	19 0	*m*
			,, 12,	19 0	*M*
R Canis Majoris ...	7 14·5 ...	16 11 S. ...	,, 11,	18 10	*m*
			,, 12,	21 26	*m*
U Geminorum ...	7 48·5 ...	22 18 N. ...	,, 10,		*M*
X Boötis	14 18·9 ...	16 50 N. ...	,, 7,		*m*
U Boötis	14 49·2 ...	18 9 N. ...	,, 9,		*m*
δ Libræ	14 55·1 ...	8 5 S. ...	,, 12,	22 53	*m*
R Herculis	16 1·2 ...	18 40 N. ...	,, 10,		*M*
U Ophiuchi	17 10·9 ...	1 20 N. ...	,, 11,	4 58	*m*
β Lyræ	18 46·0 ...	33 14 N. ...	,, 12,	6 0	*M*
R Aquilæ	19 0·0 ...	8 4 N. ...	,, 6,		*M*
T Vulpeculæ ...	20 46·8 ...	27 50 N. ...	,, 8,	20 0	*M*
			,, 12,	4 0	*m*
Y Cygni	20 47·6 ...	34 14 N. ...	,, 6,	5 40	*m*
		and at intervals of		36 0	
δ Cephei	22 25·0 ...	57 51 N. ...	Jan. 7,	2 0	*M*
			,, 10,	20 0	*m*
S Aquarii	22 51·3 ...	20 56 S. ...	,, 7,		*M*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A.	Decl.	
Near ξ Virginis	173° ...	9° N. ...	Swift; streaks.
			January 11.
,, ζ Boötis	218 ...	14 N. ...	Very swift; streaks.
,, β Boötis	222 ...	42 N. ...	,, ,,

NOTES ON METEORITES.[1]

VII.

Possible Connection between the Jets and Envelopes seen in Cometary Swarms.

THE jets observed in comets when near the sun are very various in form. The concentric envelopes seen at times are much more regular; an idea of their appearance will be gathered from the accompanying illustration of Donati's comet.

It has not yet been clearly ascertained whether the jets and envelopes are connected phenomena—that is, whether the jets are true whirls of the meteorites themselves—or whether they represent volatilization of the vapours of the nucleus in a particular direction, which vapours subsequently assume a concentric form. In Halley's comet, at all events, this was not

Fig. 21.—Concentric envelopes as illustrated by Donati's comet.

observed. Sir John Herschel writes concerning this: "The bright smoke of the jets, however, never seems to be able to get far out towards the sun, but always to be driven back and forced into the tail, as if by the action of a violent wind rolling against them—always from the sun—so as to make it clear that this tail is neither more nor less than the accumulation of this

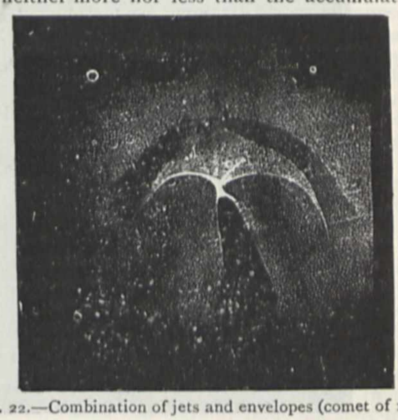

Fig. 22.—Combination of jets and envelopes (comet of 1861).

sort of luminous vapour, darted off in the first instance towards the sun, as if something raised it up, as if it were exploded by the sun's heat, out of the kernel, and then immediately and forcibly turned back and repelled from the sun."

The Concentric and Excentric Envelopes.

While in Donati's comet we get perhaps the finest exhibition of concentric envelopes successively thrown off from the nucleus towards the sun, in Coggia's comet, on the other hand, we had the most striking instance which has been yet observed in which the envelopes put on an appearance as if they belonged to two different systems of concentric envelopes cutting each other.

It is important here to enter into some details. In Coggia's comet (as observed with Mr. Newall's 25-inch refractor, with a low power), next to the nucleus the most brilliant feature was an object resembling a fan opened out some 160°. The nucleus, marvellously small and definite, was situated a little to the left of the pin of the fan—not exactly, that is, at the point held in the hand. If this comet, outside the circular outline of the fan, offered indications of other similar concentric circular outlines, astronomers would have recognized in it a

[1] Continued from p. 142.

great similarity to Donati's comet with its "concentric envelopes." But it did not do so. Envelopes there undoubtedly were, but instead of being concentric they were excentric, and of an entirely unique arrangement.

To give an idea of the appearance presented by these excentric envelopes, still referring to the fan, let us imagine a circle to be struck from the left-hand corner with the right-hand corner as a centre, and make the arc a little longer than the arc of the fan. Do the same with the right-hand corner. Then with a gentle curve connect the end of each arc with a point in the arc of the fan half-way between the centre and the nearest corner. If these complicated operations have been properly performed, the reader will have superadded to the fan two ear-like things (as of an owl), one on each side. Such "ears," as we may for convenience call them, were to be observed in the comet, and they at times were but little dimmer than the fan. It will be observed that there is a central depression between the ears.

At first it looked as if these ears were the parts of the head furthest from the nucleus in advance along the comet's axis, but careful scrutiny revealed, still further forwards, a cloudy mass, the outer surface of which was convex, while the contour of the inner surface exactly fitted the outer outline of the ears and the intervening depression. This mass was at times so faint as to be almost invisible. But at other times it was brighter than all the other details of the comet which remain to be described, now that I have sketched the groundwork. Occasionally to be seen outside all was still another fainter mass, both the surfaces of which were convex outwards, the inner one having a greater radius. This exterior envelope or "umhullung" was the faintest part of the head.

In the root of the excessively complex tail were to be observed prolongations of all the curves to which I have referred. Thus, behind the brightest nucleus was a region of darkness which opened out 45° or 60°, the left-hand boundary of which was a continuation of the lower curve of the right ear. All the boundaries of the several different shells which showed themselves, not in the head in front of the fan, but in the root of the tail behind the nucleus, were continuous in this way—the boundary of an interior shell on one side of the axis bent over in the head

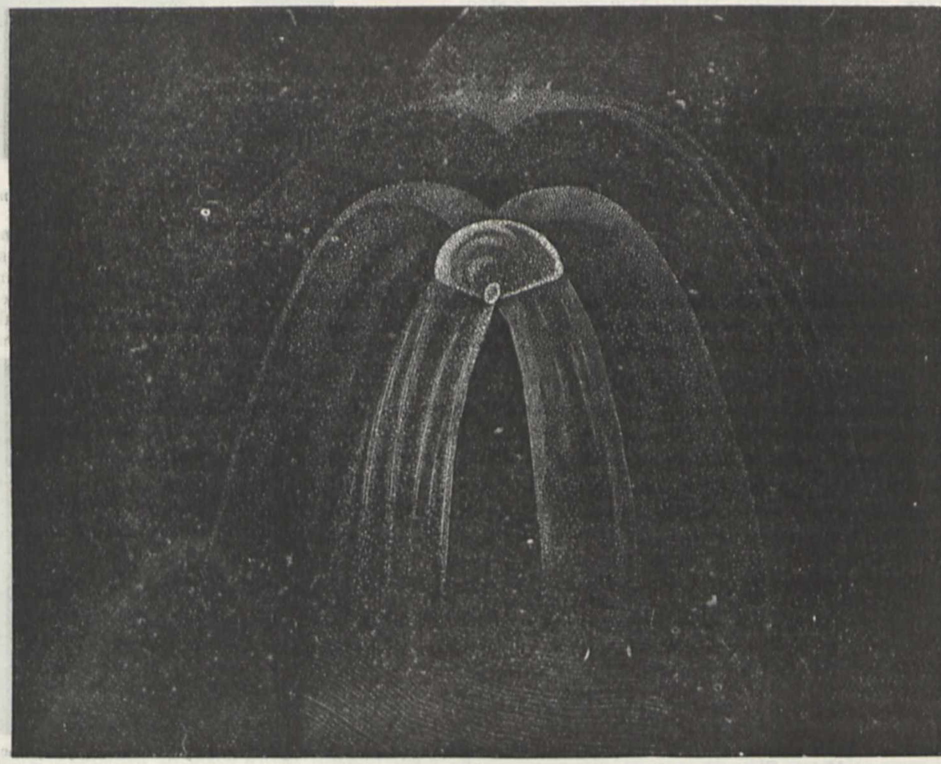

FIG. 23.—Rough outline sketch of head and envelopes of Coggia's comet as seen in Mr. Newall's 25-inch refractor on the night of July 12, 1874 (perihelion passage, August 27).

to form the boundary of an exterior shell on the other side of the axis.

I next draw attention to the kind of change observed. To speak in the most general terms, any great change in one "ear" was counterbalanced by a change of an opposite character in the other; so that, when one ear was thinned or elongated, the other widened; when one was dim, the other was bright; when one was more "pricked" than usual, the other at times appeared to lie more along the curve of the fan and to form part of it. Another kind of change was in the fan itself, especially in the regularity of its curved outline and in the manner in which the straight sides of it were obliterated altogether by light, as it were, streaming down into the tail.

There was nothing which in the slightest degree resembled the giving off of vapour.

The only constant feature in the comet was the exquisitely soft darkness of the region extending for some little distance behind the nucleus. Further behind, where the envelopes, the prolongation of which formed the tail, were less marked, the delicate veil which was over even the darkest portion became less delicate, and all the features were merged into a mere luminous haze. Here all structure, if it existed, was non-recognizable, in striking contrast with the region round and immediately behind the fan.

Next, it has to be borne in mind that the telescopic object is, after all, only a projection, from which the true figure has to be built up, and it is when this is attempted that the unique character of this comet becomes apparent. There were no jets, there were no concentric envelopes; but, in place of the latter, excentric envelopes indicated by the ears and their strange backward curvings, and possibly also by the fan itself.

It seems impossible that we can be here dealing with the mere volatilization of the materials of which the nucleus is composed; for, assuming that it is possible, as has hitherto been imagined, that shells of vapours can be thrown off to form concentric envelopes, and that the heads of comets like Donati's are thus built up, it is difficult at first to see how such appearances as here described could be thus produced.

On the Forces which produce the Various Forms and Parts of Comets.

Before we proceed further with any detailed description, it is necessary to inquire into the causes of the cometary phenomena with which we have so far become acquainted—namely, nucleus, jets, envelopes concentric and excentric, and tails.

We shall best do this by referring to the various memoirs with which Roche, of Montpellier, has enriched science. He dealt first with the atmospheres of planets; and, in concluding the third part of a memoir on the figure of a fluid mass subjected to the attraction of a distant point,[1] remarked that the inquiry might possibly apply to the theory of comets, if we suppose such an object, fluid and homogeneous, falling in a straight line towards the sun.

We have seen that a comet when it first makes it appearance at its greatest distance puts on a form resembling a planetary nebula. It is at this point that M. Roche closes with it in order to see what its change of form must be supposing it to be as above stated fluid and homogeneous.

As it approaches the sun, a tidal action will be set up, as the solar attraction will be greater on the particles nearest to it; hence there will be an elongation of the swarm, and possibly even one or more separations along a radius vector.

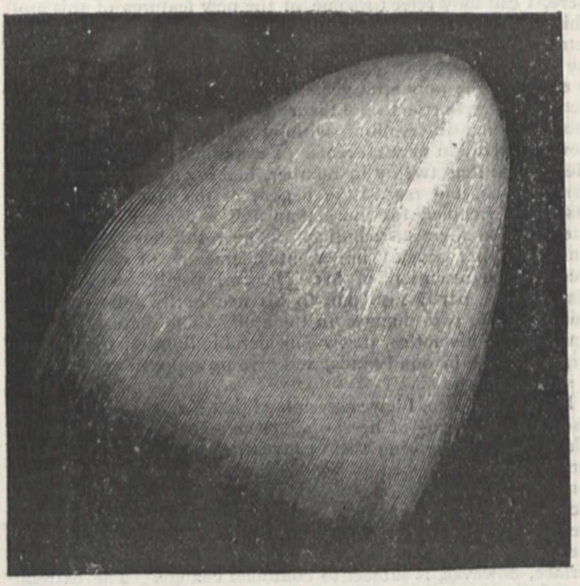

Fig. 24.—Elongation of a cometary swarm. Comet 1882 *b*, Washington equatorial.

If gravitation alone is concerned, the comet will remain symmetrical, it will reduce its size as it approaches the sun,[2] and part of its outer portions will be successively lost along the radius vector both towards and away from the sun; there, in fact, will be two outpouring streams—one directed towards the sun, the other away from it. There will be the greatest elongation and the greatest loss at perihelion.

M. Roche makes this out by considering the form of the envelope in which particles will be equally attracted by the sun and the general mass of the comet.

One chief point of the mathematical investigations was, in fact, to determine the surface on which the gravity of a small particle was *nil* in consequence of the solar and cometary attractions. This is called the limiting surface. On this point I quote from M. Faye:—[3]

"There exists, for every body placed within the sphere of action of our sun, a surface limit beyond which its matter may not pass, under pain of escaping to that body and falling within the domain of the solar action. This surface limit depends on two things—the mass of the body, and its distance from the sun. For a planet like the earth, whose mass is so considerable, this surface limit is very distant, and yet, within the still terrestrial region of its satellite, the moon, a child could lift, without much difficulty, a body which would weigh for us 36,000 kilogrammes, so feeble does the attraction of our globe become at that distance of 60 terrestrial radii. A little beyond the lunar orbit, a body would cease to belong to the earth, and would enter the exclusive domain of the sun. But for a comet this surface limit is much nearer the nucleus, and, moreover, it draws nearer and nearer in proportion as the comet approaches the sun. . . . The surface which so limits a body in the vicinity of the sun presents two singular points in the direction of the radius vector, setting out from which this surface is widened out into a conical network, in such a manner that the dissolution of a body the matter of which reaches or passes beyond these boundaries is effected principally in the vicinity of the points referred to, flying, so to speak, into two pieces, thus obeying at once the attraction of the comet and especially, the thenceforth preponderating attraction of the sun. . . .

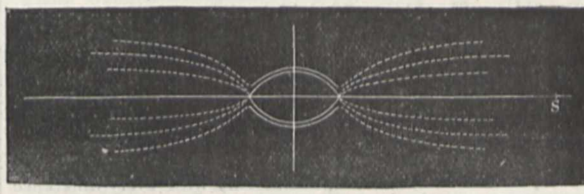

Fig. 25.—Showing how a comet approaching the sun, gravity alone being in question, loses its constituent particles beyond its free surface, which is constantly diminishing, by an outflow in both directions along the radius vector.

"All the conditions of instability are found united in comets. Their mass is extremely small, and, consequently, the surface limit is very near the centre of gravity. Their distance from the sun diminishes rapidly in the descending branch of their trajectory; consequently this surface limit becomes more and more contracted. Finally, their enormous volume tends unceasingly to dilate, because of the increasing heat of the sun, and to cause the cometary matter to shoot out beyond this surface limit.

"What becomes of this matter after it is set free by the action of the sun? Having escaped from that of the comet, it will none the less preserve the original speed, *i.e.* the speed which the comet itself had at the moment of separation; this speed will scarcely be altered by the feeble attraction of the cometary nucleus, or by the internal movements of which I have spoken, since these are measured by a few metres per second, while the general motion round the sun takes place at the rate of 10, 15, 20 leagues and more per second. The molecules, separated and thenceforward independent, then describe isolated orbits around the sun, differing very little from that of the comet. Those which are found in advance go a little faster and take the lead; those which are behind remain a little in the rear; so that the abandoned materials are divided along the trajectory of the comet in front and in rear of the nucleus. In time these materials are separated considerably from the body from which they emanate, and are more and more disseminated; but, considered at the moment of emission, they will form two visible appendages, two sorts of tails opposed and stratified on the orbit of the comet."

So much for the state of things if gravitation alone is in question.

But is gravitation alone concerned in building up a comet's form? That this is not so was fully recognized long ago, and it was suggested by the fact that the tails always appeared to be driven away from the sun; Seneca, indeed, was possibly acquainted with this fact, as he wrote: "Comæ radios solis effugiunt."[1] Kepler was the first to suggest that the matter of the tails was transported to the regions opposite the sun by the impulsion of the solar rays; Euler and Laplace accepted this explanation; and Newton was the first to give a complete explanation of the curve of the tail.

Olbers, whose researches dealt with the phenomena presented by the comet of 1811, considered that the approach of a comet to the sun might develop electricity in one or the other of these bodies, and to this were ascribed both the repulsive action of the

1 *Mémoires* of the Academy of Montpellier, vol. ii. p. 23.
2 *Annales de l'Observatoire de Paris,* vol. v. p. 376.
3 "Forms of Comets," Nature, vol. x. p. 247.

1 See Pliny, Book II. chap. xxii. *et seq.*, for many references to more ancient authorities.

sun on the materials of the comet, and that of the comet on the nebulous atmosphere by which it was surrounded.

Olbers was driven to consider the repulsive action of the comet on its atmosphere in order to explain the many luminous sectors visible in the comet in question. To this he also ascribed the gradual rise of successive envelopes, so well illustrated subsequently by the comet of Donati.

The energy of electrical repulsion depends upon the amount of surface of the bodies concerned, whereas the attraction of gravity depends upon the masses of the bodies. Small things have more surface in proportion to their masses than large ones, and there will therefore be attraction or repulsion between the sun and the particles composing comets according as the differential effect of the two opposite forces is repulsive or attractive. In the very small particles, the electrical repulsion will be stronger than the attraction due to gravitation, while in the larger particles the two forces may balance each other, or gravitation may preponderate. Only the finest particles composing the head of a comet are therefore repelled to form the tails.

Bessel[1] considerably modified this hypothesis. He considered that the action of the sun on the comet represented a polar force.

M. Faye has more recently held that this repulsive action is due to the radiant energy of the sun, and that it has an intensity inversely as the square of the distance, and proportional to the surface and not to the mass of the moving particles. Its action would therefore be in the inverse ratio of the density of the particles upon which it acted ; it would vary with every difference of cometary constitution ; it would be inappreciable on the nucleus itself ; (the idea being, of course, that the nucleus was a solid body) ; and it would be most effective in the case of the rarest vapours. The important part of M. Roche's later memoir consists in testing these views of repulsion, to determine whether the forms of comets could be explained by its introduction.

One result is very striking : the tail towards the sun demanded by gravitation alone at once disappears. The limiting surfaces which Roche's calculations demand are so very like some of the surfaces actually observed in the head of a comet, where they can be best seen, that it is suggested that the movement of the particles takes place in the precise direction where they would flow according to M. Roche's mathematical investigations.

Hence we are justified in attributing some cometary phenomena to the flow of matter acting under the influence of attraction and solar repulsion.[2] In concluding his memoir Roche points out (p. 393) that the hypothesis of a repulsive force acting along a radius vector, and varying inversely as the square of the distance, and only acting on matter reduced to a state of great rarefaction, gives figures identical with those observed. We see the germ of the tail is the part of the atmosphere the furthest removed from the sun, and it is easy to explain the enormous development of the emission of cometary particles near perihelion. The existence of a repulsive force which counterbalances the solar attraction M. Roche therefore considers established by his researches.

It must, however, be at once stated that much remains to be done before all the help that M. Roche's work can afford can be

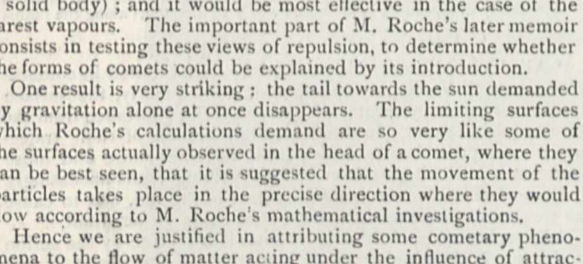

FIG. 26.—M. Roche's theoretical construction of the head of a comet, a repulsive force being taken into account.

utilized, and there is little question that the outflow in the solar direction has not been so entirely abolished as his figures indicate. This, however, may to a certain extent depend upon the fact that the observations of comets have been made at some distance from perihelion. But there may be another reason. If the outflow along the limiting surface is an outflow of solid particles, the solar repulsion will not be effective until collisions have reduced this dust to vapour. We shall still therefore have the quasi-conical surface *turned towards the sun*,[1] though it will be soon destroyed. Many of the phenomena presented by jets and excentric envelopes may be thus caused, and the very complicated phenomena presented by Coggia's comet, and others in which the section of the cone presents the appearance of birds with their wings more or less extended, do not seem opposed to this view.

J. NORMAN LOCKYER.

(*To be continued.*)

[1] Bessel's paper "On the Physical Constitution of Halley's Comet" is printed in the *Connaissance des Temps*, 1840.

[2] See *Annales de l'Observatoire de Paris*, vol. v.

[1] Although this does not figure in Roche's diagrams, Faye gives it in his lectures on the "Forms of Comets."

PRELIMINARY NOTE ON KEELING ATOLL, KNOWN ALSO AS THE COCOS ISLANDS.

MR. JOHN MURRAY, of the *Challenger* Expedition Office, has forwarded to us the following letter, which he has received from Dr. Guppy :—

DEAR MR. MURRAY,—

During my sojourn of nearly ten weeks in these islands, I was able to make a fairly complete examination of them. Here, I can only refer to some of the new features of this atoll which my investigations have disclosed, and must leave the details to be subsequently worked into a general description of the islands. Regarding myself as very fortunate in being able to examine the only atoll visited by Mr. Darwin—the atoll, in fact, which gave rise to the theory of subsidence—I at once set about making observations, without reference to any particular view of the origin of coral-reefs. I examined all the islands and islets, more than twenty in number, making a separate description of each, and reaped the benefit of the fact that this atoll has been occupied for more than half a century by residents interested in their surroundings. The result has been to convince me that several important characters of these islands escaped the attention of Mr. Darwin, partly owing to his limited stay, partly also due to his necessarily defective information of the past changes in the atoll. The features, in fact, that escaped his notice, throw considerable light on the mode of origin of these lagoon islands, and give no support to the theory of subsidence.

In the first place, I have ascertained that Keeling Atoll consists essentially of a ring of horse-shoe or crescentic islands inclosing a lagoon and presenting their convexities seaward. The crescentic form is possessed in varying degrees by different islands : some of the smaller ones are perfect horse-shoe atollons, and inclose a shallow lagoonlet ; others, again, exhibit only a semi-crescentic form ; whilst the larger islands have been produced by the union of several islands of this shape. The whole land-surface, however, is subject to continual change. The extremities of islands are often being gradually swept away or extended. Some islands are breached during heavy gales, others are joined, so that by the repetition of these changes the island in the course of time loses its original form. Hence it is that, although the crescent is the primitive shape of each island this structure is partly disguised in the case of some of the larger islands by the union of several of smaller size. The Admiralty chart gives but an imperfect idea of the true shape of the islands ; but, notwithstanding, its inspection will prove very instructive.

In truth, Keeling Atoll exhibits in an incomplete manner the features of the large compound atoll of the Maldive Group. If it was considerably larger and possessed a less protected lagoon, so that open-sea conditions prevailed in its interior, it would have all the features of a compound Maldive atoll—that is, an atoll consisting of a circle of small atolls or atollons. In its original condition, however, it was an atoll consisting of a circle of crescentic islands. Such it is essentially now, but extensive changes have often partly disguised this feature.

Before proceeding to explain the origin of the incompleted compound atoll of the Keeling Islands, it will be necessary to dwell on the exaggerated prevailing notion of an atoll. This kind of coral-reef is usually described as a circular reef inclosing a deep basin or lagoon ; but this description only applies to very small atolls less than a mile across. By drawing a section on a true scale of an atoll of average size, like Keeling Atoll, it will at once become apparent that such a description gives a very misleading idea of the real nature of this class of reef. A section of Keeling Atoll, drawn from the 1000-fathom line on a true scale of an inch to the mile, and intended to illustrate a breadth of six miles, and a depth in the lagoon of 9 or 10 fathoms, would represent to the naked eye a flat-topped mountain, the depth of the so-called basin on the summit being merely represented by a slight central depression of about 1/100 of an inch. If the lagoon possessed a depth of 30 fathoms, the inclosed basin so-called would only be indicated in this section by a central depression of about 3/100 of an inch. So trifling a proportion does the depth of an atoll of ordinary size bear to the breadth, that such a reef can only be accurately described as possessing a broad level surface, with very slightly raised margins. A correct model of Keeling Atoll would at once convey a just idea of the true relative dimensions of a reef of this class. The lagoon would be there only represented by a film of water occupying a slight hollow in the level mountain-top. By thus grasping these facts, we at once perceive that by reason of our failing to view an atoll in relation to its surroundings, and through our misconceptions of its dimensions, we have been led to introduce a great cause to explain a very small effect. The slightly raised margins can be easily explained by causes dwelt upon by Murray, Agassiz, and others. No movement of the earth's crust is necessary for this purpose. The mode of growth of corals, the action of the waves, and the influence of the currents, afford agencies quite sufficient to produce the slightly raised margins of an atoll.

The development of the islands of an atoll into horse-shoe or crescentic islands, as in the instance of Keeling Atoll, or into perfect small atolls or atollons, as in the Maldive Group, is a subsequent process to be shortly explained. These small atolls and horse-shoe islands only assume their characteristic forms *after the island has been thrown up by the waves.* Such was the conclusion I arrived at concerning small atolls and crescent-shaped coral islands in the Solomon Islands (Proc. Roy. Soc. Edin., 1885–86, p. 900); and as just stated I have formed the same opinion concerning the islands of Keeling Atoll. There is in the first place the island from which "lateral extensions grow out on either side so as to ultimately form a horse-shoe reef," which itself under favourable conditions may develop into a small atoll. In the Solomon Islands I imperfectly grasped the method by which these changes in form are effected. In Keeling Atoll I saw the process in operation, and I arrived at the conclusion that wherever a coral island stems a constant surface-current, the sand produced by the breakers on the outer edge of the reef will mostly be deposited by the current on each side of the island in the form of two lateral banks or extensions, giving the island ultimately a horse-shoe form, with the convexity presented against the current. The process may be aptly compared to the formation of a V-shaped ridge of sand when a stake or some other obstacle is placed in a river-bed. The stake represents the original small island thrown up by the waves. The V-shaped ridge of sand represents the arms of the horse-shoe island which are subsequently formed. The back-wash or eddy may in the river-bed join the arms of the V-shaped ridge of sand. In a similar manner a horse-shoe island may have a bank thrown up across the mouth, and thus a small atoll is formed. Such is the process, imperfectly disclosed to me in the Solomon Islands, that I found illustrated in all its stages in Keeling Atoll. In the Keeling Islands, however, it was necessary to satisfy myself of the reality of the agencies chiefly concerned in this process. For instance, I had to ascertain how and to what extent the surface-currents acted, and to discover the source of the sand. It was also necessary to observe what changes in the form and extent of the islands had occurred in the experience of the residents during the half-century of their occupation.

The westerly equatorial drift or south-east trade current, striking the south-east angle of the atoll, there divides and sweeps around the coasts, the two branches meeting and forming an eddy off the north-west island, a spot where drift timbers are often detained and stranded after having been swept around half the circumference of the atoll. Advantage of this current is taken by the proprietor of the islands, who directs his men to mark any logs of valuable timber thrown up on the weather or south-east coast, and then to launch them again outside the breakers. In this way huge logs are transported by the current to any particular island. If left alone, the logs, whether drifted around the north or south side of the atoll, arrive finally in the eddy off the north-west angle. This current finds its way into the lagoon through the several passages between the islands its rate there varying usually from half a knot to two knots in the hour. Only rarely is there any check to the inflow of water through the passages, as, for instance, during north-west gales.

The current in these passages carries daily a large amount of sand into the lagoon. I discovered this accidentally whilst using the tow-net for catching the pelagic animals brought in by the current. The source of this sand is the weather edge of the reef on the outer side of the islands, where the breakers are unceasingly at work in keeping up the supply. After several measurements under varying conditions of current, tide, and depth, I estimated that during every day of ordinary weather at least 10 tons of sand are carried through the passages into the lagoon. During gales and cyclones this amount is greatly increased; and probably the estimate for an ordinary year would not be less than 5000 tons. The bulk of this sand is deposited by the current near the inner mouths of the passages and on the margins of the lagoon, where it goes to extend the islands in the form of banks stretching into the lagoon. In this manner an island obtains a horse-shoe shape, just as the V-shaped ridge is formed by placing a stake in a river-bed. The first stage is represented by an island with two sand-banks extending into the lagoon, one from each extremity. The second stage is that in which the island has attained a semi-crescentic shape by the encroachment of its vegetation on the newly formed banks. In the course of time, when the vegetation of the island has entirely occupied the banks, the third stage, that of the horse-shoe island, is reached. In some instances, there is yet a further stage, when during a long continuance of westerly winds another bank is thrown up across the mouth of the horse-shoe, and a small atoll with a shallow lagoonlet is produced. Thus the currents are the principal agencies in forming the horse-shoe islands of Keeling Atoll. In large atolls, where more open-sea conditions prevail in the lagoon, and especially where, as in the Maldives, there are two opposite sets of winds and surface-currents, each prevailing in its own half of the year, we should expect to find the horse-shoe island replaced by an atollon. Keeling Atoll, however, lies for eleven months out of the twelve within the region of the constant trade-wind and westerly drift current, so that the situation is only one favouring the formation of horse-shoe islands facing to the southward and eastward. The protected character of the lagoon, also, is not a condition that would assist the growth of a circular island or atollon.

Another important feature in this atoll is to be found in the existence outside the seaward edge of the present reef of a series of submerged lines of growing corals separated from each other by sandy intervals. Unfortunately, I was not able to examine these to the extent I desired, since it can only be satisfactorily done later in the year, when the sea is sufficiently smooth to allow boats to approach the breaker edge of the reef. This feature, however, is familiar to the residents, who have supplied me with information on the subject. It would seem that all around the circumference of this atoll there is a space outside the present edge of the reef varying from 200 to 500 or 600 yards in width, where ships have anchored, and where boats in the calm season go with fishing parties. Here the submarine slope slopes gradually down to 20 or 30 fathoms; but beyond this the descent is precipitous. It is on this gradual slope that the lines of growing coral occur, separated by sandy intervals from each other. There may be two or three of these lines, the innermost covered by 4 or 5 fathoms, and the outer by from 20 to 30 fathoms.

We are thus able to perceive that the outward extension of the reef is effected, not so much by the seaward growth of the present edge of the reef, as by the formation outside it of a line of growing corals, which when it reaches the surface reclaims, so to speak, the space inside it, which is soon filled up with sand and reef-*débris*. The evidence, in fact, goes to show that a reef grows seaward rather by jumps than by a gradual outward growth. This inference is of considerable importance, since it connects all classes of reefs together in the matter of their seaward growth, the degree of inclination of the submarine slope being the chief determining factor.

Following Le Conte, I have previously shown (Proc. Roy. Soc. Edin. 1885–86, p. 884) that where there is a very gradual submarine slope the deposition of sand and the presence of much sediment in the water will prevent the growth of corals in the shallow water outside the seaward edge of the reef, and that in consequence a line of living corals will spring up in the clearer and deeper waters a considerable distance beyond. The appearance of this line of coral at the surface will result in the production of a barrier-reef with a lagoon-channel inside. In a similar manner the submerged line of growing corals immediately outside the weather-edge of the reef of Keeling Atoll would form a barrier-reef, if it was removed some miles from the shore instead of being only about 100 yards distant. As it is now situated, it lies too close to the edge of the present reef to prevent the obliteration of the channel inside it after it has reached the surface. Its lagoon-channel would be very quickly filled with sand and reef-*débris*, and as a result we should merely have a permanent addition to the present reef-flat, which, when the process was complete, would be 100 yards wider. The process is the same as in the case of a barrier-reef, the difference in the result being due to the submerged line of corals being too close to the edge of the reef for the preservation of the interior channel; and this circumstance is due to the fact of the submarine slope being greater than in the case of a coast fronted by a barrier-reef. These remarks are merely intended to be suggestive. They may, perhaps, direct the attention of of other observers to the examination of the outer slopes of atolls and to their mode of seaward growth. This can only be done during unusually calm weather.

I have discovered many other new features of minor interest in connection with Keeling Atoll, to which I will refer in my full description of these islands. The island of North Keeling, lying fifteen miles to the north, is a small atoll connected with Keeling Atoll by a bank. I hope to describe it at some future time.

In conclusion, I may state that most of my observations in these islands were directed towards estimating the age of Keeling Atoll. These data have yet to be worked up, and I am fairly confident of getting a satisfactory estimate. The lagoon is rapidly filling up with sand and coral, but it is almost impossible to state in precise terms the changes since the visit of the *Beagle*, as the survey then made was little more than a sketch. The present Admiralty chart is of but little service in inquiring into past changes, for in it the original survey of the *Beagle* in 1836 has received several later additions, and there is nothing to distinguish the one from the other. For the purpose of navigation, and for the advantage of science, a complete examination of these islands should be made. The best season for surveying is during the calm weather of the months of January and February, when boats can venture close to the edge of the reef, and a satisfactory examination of the outer shores, as well as the interior of the atoll, can then be made. In collecting information from the residents, it will be necessary to remember that no records are kept in the islands; and in studying past changes the observer will have to receive what may at first sight appear to be very interesting facts with scientific caution. Some corroboration of such facts should always be looked for.

Yours faithfully,

Batavia, November 8. H. B. GUPPY.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, December 20, 1888.—"Correlations and their Measurement, chiefly from Anthropometric Data." By Francis Galton, F.R.S.

Two organs are said to be co-related or correlated, when variations in the one are generally accompanied by variations in the other, in the same direction, while the closeness of the relation differs in different pairs of organs. All variations being due to the aggregate effect of many causes, the correlation is a consequence of a part of those causes having a common influence over both of the variables, and the larger the proportion of the common influences the closer will be the correlation. The length of the cubit is correlated with the stature, because a long cubit usually implies a tall man. If the correlation between them were very close, a very long cubit would usually imply a very tall stature, but if it were not very close, a very long cubit would be on the average associated with only a tall stature, and not a very tall one; while, if it were *nil*, a very long cubit would be associated with no especial stature, and therefore, on the average, with mediocrity. The relation between the cubit and the stature will serve as a specimen of other correlations. It is expressed in its simplest form when the relation is not measured between their actual lengths, but between (*a*) the deviation of the length of the cubit from the mean of the lengths of all the cubits under discussion, and (*b*) the deviation of the mean of the corresponding statures from the mean of all the statures under discussion. Moreover these deviations should be expressed on the following method in terms of their respective variabilities. In the case of the cubit, all the measures of the left cubit in the group under discussion, and which were recorded in inches, were marshalled in the order of their magnitude, and those of them were noted that occupied the first, second, and third quarterly divisions of the series. Calling these measures Q_1, M, and Q_3, the deviations were measured from M, in terms of inches divided by $\frac{1}{2}(Q_3 - Q_1)$, which divisor we will call Q. Similarly as regards the statures. [It will be noted that Q is practically the same as the probable error.] This having been done, it was found that, whatever the deviation, y, of the cubit might be, the mean value of the corresponding deviations of stature was $0{\cdot}8y$; and, conversely, whatever the deviation, y', of the stature might be, the mean value of the corresponding deviations of the cubit was also $0{\cdot}8y'$. Therefore this factor of 0·8, which may be expressed by the symbol r, measures the closeness of the correlation, or of the reciprocal relation between the cubit and the stature. The M and Q values of these and other elements were found to be as follow:[1] left cubit, 18·05 and 0·56; stature 67·2 and 1·75; head length, 7·62 and 0·19; head breadth, 6·00 and 0·18; left middle finger, 4·54 and 0·15; height of right knee, 20·50 and 0·80; all the measures being in inches. The values of r in the following pairs of variables were found to be: head length and stature, 0·35; left middle finger and stature, 0·70; head breadth and head length, 0·45; height of knee and stature, 0·9; left cubit and height of right knee, 0·8. The comparison of the observed results with those calculated from the above data showed a very close agreement. The measures were of 350 male adults, containing a large proportion of students barely above twenty-one years of age, made at the laboratory at South Kensington, belonging to the author.

These results are identical in form with those already arrived at by the author in his memoir on hereditary stature (Proc. Roy. Soc., vol. xl, p. 42, 1886), when discussing the general law of kinship. In that memoir, and in the appendix to it by Mr. J. D. Hamilton Dickson, their *rationale* is fully discussed. In fact, the family resemblance of kinsmen is nothing more than a special case of correlation.

The general result of the inquiry was that, when two variables that are severally conformable to the law of frequency of error, are correlated together, the conditions and measure of their closeness of correlation admits of being easily expressed. Let x_1, x_2, x_3, &c., be the deviations in inches, or other absolute measure, of the several "relatives" of a large number of "subjects," each of whom has a deviation, y, and let X be the mean of the values of x_1, x_2, x_3, &c. Then (1) $y = r$X, whatever may be the value of y. (2) If the deviations are measured, not in inches or other absolute standard, but in units, each equal to the Q (that is, to the probable error) of their respective systems, then r will be the same, whichever of the two correlated variables is taken for the subject. In other words, the relation between them becomes reciprocal; it is strictly a correlation. (3) r is always less than 1. (4) r (which, in the memoir on hereditary stature, was called the ratio of regression) is a measure of the closeness of correlation. Other points were dwelt upon in the memoir, that are not mentioned here: among these was as follows: (5) The probable error, or Q, of the distribution of x_1, x_2, x_3, &c., about X, is the same for all values of y, and is equal to $\sqrt{(1 - r^2)}$ when the conditions specified in (2) are observed.

It should be noted that the use of the Q unit enables the variations of the most diverse qualities to be compared with as much precision as those of the same quality. Thus, variations in lung-capacity which are measured in volume can be compared with those of strength measured by weight lifted, or of swiftness measured in time and distance. It places all variables on a common footing.

"Preliminary Account of the Morphology of the Sporophyte of *Splachnum luteum*." By J. R. Vaizey, M.A., of Peterhouse, Cambridge. Communicated by Francis Darwin, F.R.S.

[1] The head length is here the maximum length measured from the notch below the brow. The cubit is measured with the hand prone, from the flexed elbow to the tip of the middle finger. The height of knee is taken from a stool, on which the foot rests with the knee flexed at right angles; from this the measured thickness of the heel of the boot is subtracted. All measures had to be made in the ordinary clothing. The smallness of the number of measures, viz. 350, is of little importance, as the results run with fair smoothness. Neither does the fact of most of the persons measured being hardly full grown affect the main results. It somewhat diminishes the values of M, and very slightly increases that of Q, but it cannot be expected to have any sensible influence on the value of r.

Royal Meteorological Society, December 19.—Dr. W. Marcet, F.R.S., President, in the chair.—The following papers were read:—On the prolonged spell of cold weather from September 1887 to October 1888, by Mr. C. Harding. During the fifty-nine weeks ending the third week in October, there were but four warm weeks in the north-west of England, and only five warm weeks in the south-west of England, whilst in the latter district there was not a single warm week between March 12 and October 22. The mean temperature for the whole period was dealt with for the twelve districts into which the Meteorological Office divides the whole area of the United Kingdom, and with the single exception of the north of Scotland the weather for the period ending in October this year was the coldest of any during the past ten years. At Greenwich the temperature during the fourteen months was below the average on 312 days out of 427, or 73 per cent., and in July there was not a single warm day, the temperature being continuously below the average from June 27 to August 6. The means for July 11 and 12 were colder by several degrees than those for March 9 and 10.—Report on the phenological observations for 1888, by the Rev. T. A. Preston. Vegetation was generally backward throughout the season. In the south-west of England and south of Ireland plants were earlier than usual, but not elsewhere. In February they were from one to four weeks later, and gradually gained ground till June. In the south of Ireland they were slightly in advance of the average in June and July; in the south-west of England they just reached the average in July; whilst in Guernsey they were a fortnight later. Fruits generally were a failure; very few really ripened, and from want of sun were deficient in flavour. Haymaking was unusually late (as much as five weeks); it began in July or August, and was not entirely finished till late in September; much of it was spoilt or secured in bad condition. Straw was plentiful, and though the corn was not an average crop, the fine October enabled farmers to secure a better one than could have been expected. Roots were often a failure, and potatoes were much diseased.—A winter's weather in Massowah, by Captain D. Wilson-Barker. This paper gives the results of four-hourly observations during December 1887 to February 1888. The highest shade temperature was 95°, and the lowest 68°.

Zoological Society, December 18, 1888.—Mr. Howard Saunders in the chair.—The Secretary read a report on the additions that had been made to the Society's Menagerie during the month of November 1888, and called attention to a specimen of the Small-clawed Otter (*Lutra leptonyx*), presented by Mr. W. L. Sclater, Deputy Superintendent, Indian Museum, Calcutta, new to the Society's Collection, and to a Monkey of the genus *Cercopithecus*, from South Africa, apparently referable to the Samango Monkey (*Cercopithecus samango*), also new to the Society's Collection.—Mr. G. B. Sowerby read descriptions of fourteen new species of Shells from China, Japan, and the Andaman Islands, chiefly collected by Deputy Surgeon-General R. Hungerford.—A communication was read from Mr. Herbert Druce, in which he gave an account of the Lepidoptera-Heterocera collected by Mr. C. M. Woodford in Guadalcanar Island, Solomon Islands. The collection was stated to contain examples of 53 species, 18 of which were described as new to science.—Mr. J. H. Leech read the second portion of a paper on the Lepidoptera of Japan and Corea, comprising an account of the Sphingidæ, Bombycidæ, Notodontidæ, and Cymatophoridæ. in all 352 species. Of these, 38 species were now described as new to science.—Dr. Hans Gadow read a paper on the numbers and on the phylogenetic development of the remiges of Birds. The author showed that the number of primaries is of very limited taxonomic value, as was proved by the numerous exceptions mentioned in the lists contained in the paper. A comparison of the remiges of the Penguins with those of other Carinatæ seemed to indicate an extremely low stage in the Penguins, which, however, was not borne out by other anatomical features. The Ratitæ were most probably descendants of birds which formerly possessed the power of flight and had lost it. This view was strengthened by an examination of the structure of the wings and of the feathers of their nestlings. The paper concluded with general remarks upon the probable gradual development of the organism of flight in birds.

PARIS.

Academy of Sciences, December 17, 1888.—M. Janssen in the chair.—On the analytical theory of heat, by M. H. Poincaré. In a previous note (*Comptes rendus*, civ. p. 1754) the author studied the problem connected with the cooling of a homogeneous and isotropous solid body; here a more satisfactory demonstration is given of the theorem growing out of that problem.—On the abruptly and slowly contracting muscles of the hare, by M. L. Ranvier. A recent experiment is described, which has been carried out for the purpose of studying in the hare the two species of muscles, which in the rabbit differ in colour, structure, and functions, but which in the hare are all alike red.—On M. Zédé's submarine boat, the *Gymnote*, by Admiral Paris. An account is given of the first trial of this boat, recently launched at Toulon, and constructed for the purpose of realizing the suggestions made by M. Dupuy de Lôme on the subject of submarine electric navigation. As this is an engine of warfare, the details of its mechanism are suppressed; but it is stated that the trial more than realized the expectations of its inventor. It works by electricity, with perfect ease, on, and at any desired depth below, the surface, obeys the helm in all positions, fully attains the hoped-for velocity, and its ventilation and lighting are all that can be expected down to a certain depth. By introducing sundry obvious modifications, boats of this description may be turned to the best account for the purpose of scientific marine exploration.—Eocene Echinidæ in the province of Alicante, Spain, by M. Cotteau. The recent explorations of the Eocene formations in this region have yielded as many as 76 species of fossil Echinidæ, grouped in 36 genera, and representing nearly all existing groups of this family. Of the species, 50 are new to science, and some of these are specially interesting, as they belong to extremely rare genera, well deserving the attention of palæontologists, and four of which are quite new. A striking feature of this Eocene Echinidian fauna is the enormous preponderance of irregular over regular forms, the former comprising as many as 67 out of the 76 species here described.—On the nutriment of castaways at sea, by Prince Albert of Monaco. The researches made during the *Hirondelle's* last expedition in the North Atlantic tend to show that the crew of a vessel short of provisions might support life indefinitely if supplied with the proper appliances for capturing the small marine fauna which is found to exist in great abundance in the Atlantic, and probably in all temperate and warm marine waters.—On the diurnal variation of the barometer, by M. Alfred Angot. It is shown that diurnal barometric variation results from the interference of two distinct waves. One of these is exclusively due to the diurnal variation of temperature in the given region, and subject like it to local influences. The other, of semi-diurnal periodicity, is produced by a general cause independent of all local influence; its phase is constant, approximating to 63°, and its amplitude for all regions and all seasons is determined by an equation, whose terms show a certain analogy with those corresponding to the theory of the tides.—On certain new properties and on the analysis of the fluoride of ethyl, by M. H. Moissan. In a previous communication the author showed that ethylfluorhydric ether (ethyl fluoride) was a gaseous body capable of being obtained in a very pure state, and causing ethyl iodide to react on the anhydrous fluoride of silver. Here he describes several other properties of the same substance. Heated to a dull red for several hours in a glass ball, the fluoride of ethyl yields a complex mixture of carburets containing traces only of the fluoride of silicium. Under the action of a weak induction spark the volume increases greatly, yielding hydrofluoric acid, a small quantity of acetylene, and especially ethylene, without depositing carbon. In the presence of a powerful spark, carbon is deposited with formation of acetylene, ethylene, propylene, &c.—On the employment of oxygenated water for the quantitative analysis of the metals of the iron group (continued), by M. Ad. Carnot. Here the author deals more especially with chromium and manganese.—On the reproduction of zircon, by MM. P. Hautefeuille and A. Perrey. Zircon, obtained at a very high temperature by Sainte-Claire Deville and Caron, by making the fluoride of zirconium to act on silica or on silicium fluoride, is here reproduced at a temperature not exceeding 700° C. by the action of the bimolybdate of lithin on a mixture of zircon and silica. This is the same process by means of which these chemists have obtained the emerald and phenacite.—Papers are contributed by M. Raoul Varet, on the action of the cyanide of mercury on the salts of copper; by M. Albert Colson, on a diquinolic base; by M. W. Louguinine, on the heats of combustion of the camphors and borneols; by M. Louis Crié, on the affinities of the Jurassic and Triassic floras of Australia and New Zealand; and by M. Michel Hardy, on the discovery of a Quaternary burial-place at Raymonden, in the commune of Chancelade, Dordogne.

December 24.—M. Janssen in the chair.—After the usua annual allocution pronounced by the President, M. Janssen, the names were announced of the successful competitors in the prize essays proposed for the year 1888. These were as under:—*Geometry:* Grand Prize of the Mathematical Sciences, M. Emile Picard; Prix Bordin, Madame Sophie de Kowalewsky; Prix Francœur, M. Emile Barbier; Prix Poncelet, M. E. Collignon. *Mechanics:* Extraordinary Prize of 6000 francs, MM. Banaré, Hauser, and Reynaud, 2000 francs each; Prix Montyon, M. H. Bazin; Prix Plumey, Madame Benjamin Normand and family; Prix Dalmont, M. Jean Resal. *Astronomy:* Prix Lalande, M. Joseph Bossert; Prix Valz, Mr. E. C. Pickering; Prix Janssen, Dr. William Huggins; Prix Damoiseau, not awarded. *Statistics:* Prix Montyon, M. Félix Faure, M. I. Teissier, and MM. Lallemand and Petitdidier. *Chemistry:* Prix Jecker, M. Maquenne and M. Cazeneuve. *Geology:* Prix Cuvier, M. Joseph Leidy. *Botany:* Prix Desmazières, M. V. Fayod; Prix Montagne, M. Gaston Bonnier. *Agriculture:* Prix Vaillant, not awarded. *Anatomy and Zoology:* Prix Savigny, not awarded; Prix Thore, Dr. Carlet; Prix da Gama Machado, not awarded. *Medicine and Surgery:* Prix Montyon, Dr. Hardy, Dr. Albert Hénocque, and MM. Follin and Duplay; Prix Bréant, Dr. Hauser; Prix Barbier, MM. Leroy, Raphael Dubois, and Dr. Ehrmann; Prix Godard, Dr. Maurice Hache; Prix Lallemand, MM. François-Franck and Paul Blocq. *Physiology:* Prix Montyon, Dr. Augustus D. Waller (London) and M. Léon Fredericq. *Geography:* Prix Gay, M. Simart. *General Prizes:* Prix Montyon (Unhealthy Industries), Dr. Paquelin and M. Fumat; Prix Trémont, M. Fénon; Prix Gegner, M. Valson; Prix Delalande-Guérineau, Père Roblet; Prix Jérome Ponti, M. Kœnigs; Prix Laplace, M. Paul-Louis Weiss.—The programme of prizes proposed for the year 1889 comprises the following:—*Geometry:* Prix Francœur (1000 fr.), discoveries or works useful to the progress of pure or applied mathematical sciences; Prix Poncelet (2000 fr.), same subject. *Mechanics:* Extraordinary Prize of 6000 francs for any invention tending to increase the efficacy of the French naval forces; Prix Montyon (700 fr.), invention or improvement of instruments useful to the progress of agriculture, the mechanical arts or sciences; Prix Plumey (2500 fr.), any invention or improvement tending most to the progress of steam navigation; Prix Fourneyron (500 fr.), theoretical and practical essay on the progress of aërial navigation since 1880. *Astronomy:* Prix Lalande (540 fr.), any essay or observation most useful to the progress of astronomy; Prix Valz (460 fr.), the most interesting astronomical observation during the year; Prix Janssen (gold medal), any discovery or work tending to the progress of physical astronomy. *Physics:* Prix L. La Caze (three of 10,000 fr. each), the best work on physics, chemistry, and Physiology. *Statistics:* Prix Montyon (500 fr.), the best work on the statistics of France. *Chemistry:* Prix Jecker (10,000 fr.), any work tending most to the progress of organic chemistry. *Geology:* Prix Delesse (1400 fr.), best work on geology or mineralogy. *Botany:* Prix Barbier (2000 fr.), most useful discovery in medicine, surgery, pharmacy, or botany; Prix Desmazières (1600 fr.), the most useful work on all or any section of Cryptogamy; Prix Montagne (1000 and 500 fr.), useful works on the anatomy, physiology, development, or description of the lower Cryptogamous plants; Prix de la Fons Mélicocq (900 fr.), best work on the botany of North France. *Agriculture:* Prix Vaillant (4000 fr.), best work on the diseases of cereals in general. *Anatomy and Zoology:* Grand Prix des Sciences Physiques (3000 fr.), the complete study of the embryology and development of any animal; Prix Bordin (3000 fr.), a comparative study of the auditory apparatus in mammals and birds; Prix Savigny (975 fr.), in aid of young zoologists studying the invertebrates of Egypt and Syria. *Medicine and Surgery:* Prix Montyon (one or more prizes not otherwise specified), for the best work on the healing art; Prix Bréant (100,000 fr.), for a specific against cholera; Prix Godard (1000 fr.), anatomy, physiology, and pathology of the genito-urinary organs; Prix Lallemand (1800 fr.), researches on the nervous system in the widest sense of the term; Prix Ballion (1400 fr.), any work most useful to the health and improvement of the human race; Prix Mège (10,000 fr.), to continue and complete the essay of Dr. Mège on the causes that have retarded or advanced the progress of medicine. *Physiology:* Prix Montyon (750 fr.), for the promotion of experimental physiology; Prix Pourat (1800 fr.), experimental researches on muscular contraction; Prix Martin-Damourette (1400 fr.), therapeutic physiology. *Physical Geography:* Prix Gay (2500 fr.), comparative study of the floras and faunas and relations existing between the Polynesian Islands and surrounding lands. *General Prizes:* Prix Montyon, one or more prizes for the best means of rendering unhealthy industries less dangerous; Prix Trémont (1100 fr.), for any work tending in any way to promote the interests of France; Prix Gegner (4000 fr.), to promote the positive sciences; Prix Petit d'Ormoy (10,000 fr.), researches in pure and applied mathematical sciences and the natural sciences; Prix Laplace (a complete collection of the works of Laplace), the first student leaving the Ecole Polytechnique.

Astronomical Society, November 7.—M. Moussette in the chair.—Colonel Laussedat read a paper on national time, in which he urged the adoption of Paris time throughout France.—M. Gunziger observed Barnard's comet on November 4. It was about the size of the nebula in Andromeda, with scarcely any tail, but a bright nucleus of about the sixth magnitude.—Rev. S. J. Perry, of Stonyhurst College, was elected an honorary member.—The Royal Astronomical Society and the Liverpool Astronomical Society were elected Corresponding Societies.

December 5.—M. Flammarion, President, in the chair.—The President announced that important gifts had been offered for the Society's proposed Observatory: M. Bardou offered a 4-inch equatorial, M. Secretan a transit instrument, M. Lütz spectroscopic and photographic apparatus, M. Lévy a set of binocular glasses. Thanks were voted to the above donors.—M. Flammarion read a paper on the changes observed in Mars, specially referring to Dawes's forked bay and Lake Mœris.—M. Gérigny read a paper on the aberration of light, showing the influence of the sun's motion upon that phenomenon by Yvon Villarceau's method.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Boilers: their Construction and Strength: T. W. Traill (Griffin).—Our Fishery Rights in the North Atlantic: J. I. Doran (Philadelphia).—Methods of Analysis of Commercial Fertilizers, Cattle Foods, &c. (Washington).—The Probable Cause of the Displacement of Beach Lines: A. Blytt (Christiania).—Bulletin of the New York State Museum of Natural History, Nos. 4, 5, 6 (Albany).

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