THURSDAY, OCTOBER 4, 1888.

DETERMINANTS.

Teoría Elemental de las Determinantes y sus principales aplicaciones al Algebra y la Geometría. Por Félix Amorétti y Carlos M. Morales. (Buenos Ayres: Imprenta de M. Biedma, 1888.)

"SOME books," says Bacon, "may be read by deputy, and extracts made of them by others;" and, at any rate so far as English readers are concerned, the work now under review belongs to this category. A very considerable portion of it is taken up with translations of selected passages from Muir's "Treatise on the Theory of Determinants," of which the following is a sample:—

"Teorema III.—*Toda determinante centrosimétrica del orden* 2m*ésimo* *es igual á la diferencia de los cuadrados de dos sumas de determinantes menores del orden* m*ésimo* *formadas con las* m *primeras filas.*"

"En efecto; el producto de las dos determinantes factores, por ejemplo, D y D′, en el caso del párrafo 37, es igual á

$$\tfrac{1}{2}\{[D + D']^2 - \tfrac{1}{2}[D - D']^2\},$$

y si D y D′ se desarrollan en función de determinantes de elementos monomios (22), las determinantes de una de las expresiones son iguales á las de la otra. Luego queda demostrado el teorema."

The above is almost word for word the same as § 138 of Muir's "Treatise," which we subjoin for the sake of comparing the translation with the original:—

"*A centro-symmetric determinant of the* 2m*th* *order is expressible as the difference of the squares of two sums of minors of the* m*th* *order formed from the first* m *rows.*

"The product of the two factors, D and D′ say, in the first case of § 137 is equal to

$$\{\tfrac{1}{2}(D + D')\}^2 - \{\tfrac{1}{2}(D - D')\}^2,$$

and when D and D′ are expanded (§ 29) in terms of determinants with monomial elements, the determinants in the one expansion are in magnitude the same as those in the other: hence the theorem."

We notice that Muir's formula is incorrectly printed in the translation; but it is only fair to add that such inaccuracies are rarely met with in the volume before us, which is more free from misprints than the first editions of mathematical books usually are. The translators do not appear to have caught the exact meaning of the words "are in magnitude the same as," which they have changed into "son iguales á." Quantities which are the same in magnitude (though differing, it may be, in sign) they call equal, and are consequently forced to translate the words "are equal" by "son iguales y del mismo signo" as they have done elsewhere in more than one place. But a much worse mistranslation (also from Muir) occurs on p. 85, where the single word "es" is used as the equivalent of "contains the term." A worse mistake than this one could not have been committed, even by those who, according to Hudibras, "translate,—

> Though out of languages, in which
> They understand no part of speech."

The above extract is taken from the second of the three distinct portions, or books, into which the "Teoría Elemental" is divided. The first of these books has to do with determinants in general, the second (consisting mainly of translations from Muir) treats of determinants of special form, and the third is reserved for algebraical and geometrical applications. The nomenclature adopted in the second book differs in some particulars from that employed by Muir. Thus our authors do not follow him in substituting "adjugate" for the more euphonious and more familiar adjective "reciprocal," and they agree with Scott and others in calling those determinants "orthosymmetrical" which Muir names "persymmetric." We think that their name "determinante *hemisimétrica*" is a distinct improvement on the old "*zero-axial skew* determinant," but we cannot see any special reason for speaking of determinants in which all the elements in one row are equal to unity as "determinantes multiples," and we do not consider that the fact of the equality of all the elements in the principal diagonal of any skew determinant is of sufficient importance to necessitate the use of the distinctive appellation "*pseudo-simétrica*" to denote such a skew determinant.

The second book contains most of the principal properties of the various kinds of symmetrical determinants, and of Pfaffians, alternants, circulants, and continuants, but not of compound or functional determinants: these are mentioned, but their properties are not investigated. The short chapter devoted to them merely defines compound determinants, Jacobians, Hessians, and Wrouskians, and then concludes abruptly with these words: "Por más interesantes que sean estas formas, la índole de esta obra no permite entrar en el estudio de ellas, para el cual se recomienda especialmente el notable tratado del profesor R. Scott, 'Determinants,' Cambridge, 1880."

Here our remarks on the second book (which finishes with this sentence) would come to a close if we did not wish to correct a mistake into which the authors have fallen as to the origin of the name *continuants*. These they say (see p. 112) "se denominan continuantes, por sugestión del profesor Sylvester." The real facts of the case are these. Prof. Sylvester was the first to discover the forms called *continuants*, to which he gave the name of *cumulants*. It was Muir who suggested the name *continuant* "as an exceedingly suitable and euphonious abbreviation for *continued-fraction determinant*," and as a "short literal translation of the equivalent term *Kettenbruch-Determinante*, which is the received name in Germany" (vide *American Journal of Mathematics*, vol. i. p. 344: letter from Mr. Muir to Prof. Sylvester on the word continuant, September 4, 1878).

Of the third book we have very little to say. It is nice easy reading for young beginners, and teaches them how to solve systems of linear equations, how to perform eliminations by means of Euler's, Bezont's as modified by Cauchy, or Sylvester's dialytic method, and how to calculate the roots common to two equations or the double roots of a single equation. There is a short chapter in which some of the most simple properties of the resultant of two equations are explained. The last chapter in the book is the only geometrical one; its principal contents are determinant expressions for the area of a triangle, a quadrilateral, and a polygon, in terms of the co-ordinates of their respective vertices, and some simple trigonometrical formulæ. On p. 173, in this chapter, we notice a curious misprint: in each of three successive formulæ (the usual expressions for the sine, cosine, and tangent of half an angle of a triangle in terms of its sides) a capital V takes the place of the sign of the square root.

The opening paragraph of the first book tells us of the origin of determinants, citing as evidence of their invention by Leibnitz his celebrated letter to L'Hopital, dated April 28, 1693. Their re-discovery by Cramer in 1750, and the rule (for the solution of a system of linear equations) which still bears his name, are next mentioned; but authors of a more modern date are summarily dismissed with the following brief notice:—

"Desde el tiempo de Cramer la teoría de las determinantes ha hecho notables progresos debido á los trabajos de Vandermonde, Laplace, Gauss, Cauchy, Jacobi, Sylvester, Muir, Baltzer y otros, no habiendo rama de las matemáticas en que no haya sido aplicada con ventaja."

We are not, however, left entirely in the dark as to the contributions to the theory made by these writers; for some theorems are called by the names of their respective authors, and a large number of others have these names indicated in brackets. For instance, the proposition which concludes the third chapter in the first book is thus enunciated:—

"Descomponer una determinante de orden $n^{\text{ésimo}}$ en una suma de productos formados cada uno de una determinante de orden $p^{\text{ésimo}}$ y de una determinante de orden $[n-p]^{\text{ésimo}}$ { Laplace }."

This is immediately preceded by—

"Teorema de Cauchy.—Si se elige una fila y una columna de una determinante cualquiera, el elemento común de ellas multiplicado por el respectivo complemento algebraico, más la suma de productos obtenidos multiplicando el producto de un elemento de la fila y de la columna por su respectivo complemento algebraico, es equivalente á la determinante dada."

The way in which these two propositions are treated in the present work will serve to exemplify the methods employed by its compilers for imparting knowledge to their readers. The proof of Laplace's theorem given by Scott, in § 5, chap. iii. of his "Determinants," is clearer than any other we are acquainted with; but it depends on some of the properties of alternate numbers. It is true that these properties are of the simplest kind, but then the notion of alternate numbers is a highly abstract one, quite as much so as the idea of a four-dimensional space. In order, therefore, to convey a clear conception of Laplace's theorem to students of average capacity, our authors have turned it into a problem, and, by considering what Prof. Sylvester calls a simple diagrammatic case, have shown how this problem can be solved, thereby bringing the *theorem* within the grasp of those whose minds are as yet unprepared to revel luxuriously in such abstractions as the alternate numbers.

On the other hand, the proof of Cauchy's theorem and the illustrative example appended to it have been reproduced, with only some slight vertal alterations, from § 62 of Muir's "Determinants," where the theorem in question is presented in a form eminently adapted for elementary instruction.

The first book ends with a rule for the division of determinants, which may be briefly stated thus: To divide $|a_{1n}|$ by $|b_{1n}|$, assume the quotient to be $|x_{1n}|$ and equate each element of the determinant formed by multiplying $|x_{1n}|$ and $|b_{1n}|$ to the corresponding element of $|a_{1n}|$.

The values of the elements $x_{11}, x_{21}, \ldots \ldots x_{nn}$, of the assumed quotient $|x_{1n}|$ will then be determined by solving a system of equations of the form

$$b_{1n}\,x_{1n} + b_{2n}\,x_{2n} + \ldots\ldots + b_{nn}\,x_{nn} = a_{nn}.$$

The article containing this rule should be expunged from all future editions of the work. Its practical inutility becomes apparent when we remember that, on solving the system of equations to which it leads, each x is found in the form of the quotient of two determinants; so that we have to perform many divisions instead of one. Those who are practically engaged in the work of mathematical tuition in the University of Buenos Ayres will doubtless be able to suggest other improvements, and if these suggestions are attended to, students in that University will possess in the second edition of the "Teoría Elemental" an introduction to the theory of determinants written in their own language and suited to their requirements.

In some respects we do not desire to see any improvement. The appearance of the book is as attractive as good paper, wide margins, and a bold clear type can make it. The authors have chosen for their motto the appropriate quotation from Sylvester: "For what is the theory of determinants? It is an algebra upon algebra; a calculus which enables us to combine and foretell the results of algebraical operations, in the same way as algebra enables us to dispense with the performance of the special operations of arithmetic." The table of contents is a model of completeness, and gives the enunciations of the theorems in full instead of merely indicating the pages and articles in which they occur. The volume ends with a selected list of treatises on determinants "que pueden servir de texto y que son dignas de especial mención." This will be of use to students who only want to be told what authors they should read, for the names mentioned are few and well chosen; while those whose object is to improve their acquaintance with the bibliography of determinants may fully satisfy their desire by consulting the two papers by Muir in the *Quarterly Journal of Mathematics* (one of them published in 1881, the other in 1886) to which reference is made.

Responding to the invitation—"agradeceríamos las indicaciones que se nos hicieran sobre omisiones ó errores que no hubiéramos advertido"—we call attention to a slight misprint in this reference, in which the word "Quarterly" has been mis-spelt "Quaterly." With the exception of those previously mentioned, no other *erratum* has come under our notice.

OUR BOOK SHELF.

The Geological History of Plants. By Sir J. W. Dawson, C.M.G., LL.D., F.R.S., &c. 8vo, pp. 290. With Illustrations. "International Scientific Series." (London: Kegan Paul, Trench, and Co., 1888.)

THIS book gives, in a connected form, a summary of the development of the vegetable kingdom in geological time.

Though likely to be of use to geologists and botanists, the treatment is sufficiently popular to be intelligible to the general reader. The floras of the successive geological formations are treated of in turn, from the oldest rocks down to comparatively recent times. The two longest chapters in the book are devoted to the vegetation of the Devonian and Carboniferous ages respectively, much of the matter here traversed having formed the subject of numerous scientific memoirs by the author. In the body of the work, accounts of the morphology and minute anatomy of the various plant-remains are given, with speculations as to their affinities, and in many cases restorations are attempted, illustrated by figures. The more special details as to classification, &c., are wisely placed in small type as a series of notes at the end of each chapter. The last chapter in the book consists of an interesting essay on the general laws of origin and migrations of plants. Many of the woodcuts leave much to be desired, more especially those dealing with histological subjects. These are, for the most part, scrappy and insufficiently described, and convey little to the mind. Comparisons between fossil remains and recent plants are often rendered valueless by strange inaccuracies as to the morphological value of the parts so compared. Thus the leaves of *Marsilea* (pp. 60 and 67) are described as being in whorls and cuneate in form, and in *Azolla* and *Salvinia* the leaves are "frondose and more or less pinnate in their arrangement." *Spenophyllum*, which possesses wedge-shaped leaves arranged in verticels on the stem, is set down as of probable Rhizocarpian affinity, on this mistaken comparison between its *leaves* and the *leaflets* of *Marsilea!* Much confusion also arises from a careless use of the terms sporocarp, sporangium, macro- and micro-spore, antheridium, &c., in connection with certain small bodies found in the Erian and Carboniferous beds, and conceived by the author to be the reproductive bodies of a rich, then-existing Rhizocarpian flora. Though there are many points in which palæobotanists may not be at one with the author—such as the reference of so many Palæozoic forms to Rhizocarps—the volume will be of service, especially to those to whom the larger treatises are not available.

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.*]

Prophetic Germs.

I HAVE but just returned from abroad, and have hastened to read the number of NATURE for August 30. I find that the Duke of Argyll in his letter of that date makes some remarks which call for a few words from me. The Duke is not, it appears, prepared to defend the theory that the electric organ of *Raia radiata* is a "prophetic germ." He refers me to the paper of Prof. Ewart on this subject, whose opinion he quotes and accepts. I am not sure how far Prof. Ewart himself had considered the significance of the view which he put forward in regard to the nature of the rudimentary electric organs of skates; but I do not hesitate to say that there are no facts which have been made known at present, either by earlier observers or by Prof. Ewart, with regard to the electric organ of skates, which necessitate such a theory of prophetic germs as that imagined by the Duke of Argyll, or which can be shown to be inconsistent with the doctrine of progressive development by the natural selection of fortuitous congenital variations. If the Duke of Argyll will point out such facts, he will have made a contribution of some value towards the understanding of the laws of organic evolution.

In a subsequent portion of his letter the Duke of Argyll states: "If Prof. Ray Lankester will explain how 'natural selection' can act upon 'congenital variations,' which he calls 'non-significant'—*i.e.* which are not yet of any actual use—and if he will explain how this action can afford 'the single and sufficient theory of the origin' of (as yet) useless variations, he will have accomplished a great triumph in logic and philosophy."

I am unwilling to entertain the notion that the Duke of Argyll has intentionally constructed the above sentence by garbled quotations from my previous letter in order to produce the false impression that I have maintained such a view as to the action of natural selection. At the same time, I will observe that the method of discussion adopted by the Duke—namely, that of half-quoting the opinion which he attributes to an opponent and desires to render illogical in the judgment of others—is, to say the least of it, objectionable. It becomes easy when this method of partial re-statement is adopted for the disputant to insert words of his own mixed with the words of his opponent, and thus to misrepresent the latter's statement by unconsciously fabricating what the poet has condemned as the worst of fabrications—namely, one which is half a truth.

The point of the sentence which I have above quoted from the Duke of Argyll's letter depends upon the unwarrantable introduction on his part after the quotation of the word "non-significant" of certain words in explanation of that word. The Duke is kind enough to say that by "non-significant" I mean "which are not yet of any actual use." I have not had any private communications with the Duke of Argyll upon this matter, and am at a loss to understand how he should have come to think that he knows that this was what I meant by the word "non-significant." By whatever process he arrived at that conclusion I regret to have to say that it is absolutely erroneous. My meaning was nothing of the kind, and I was under the impression that I had stated with sufficient clearness what my meaning was. It appears that I did not state it clearly enough for all readers. I called the congenital variation which survives in the struggle for existence "non-significant" in regard to its origin and not in regard to its survival. It was, I think, clear to most readers that I was distinguishing between the Lamarckian theory of variation as due to the transmission of parental acquired characters and the Darwinian theory of variation as due to a "shaking up" of the germ-plasma at the union of egg-cell and sperm-cell. The variation—that is, the departure of a young animal or plant from the normal character of the species—would be, if it could be traced to the transmission from a parent of a character acquired by that parent in adaptation to the environment, significant; that is to say, it would have significance for the adjustment of the species in its very origin in the parent. On the other hand, the thousand and one slight or considerable departures from the mean specific form which occur in every possible direction in a brood of young fish or other organisms are "non-significant." They are due to a long-precedent disturbance of the germ-plasma when the form of the organism was undeveloped. No possible reaction of adjustment can be imagined which could produce adaptation in the structure of an animal or plant developed from a germ, if it be a proviso that such adaptation is to have relation to a physical cause of disturbance which once acted upon the germ whilst the adaptational results are to come into effective existence in the developed product of the germ. Hence I am led to speak of congenital variations as "non-significant" in relation to the disturbing causes which produce them.

The proposition that congenital variations are selected when they are not yet of any actual use is an absurdity which the Duke of Argyll had no justification whatever for suggesting as likely to be defended by me, and one which he arrives at by misrepresenting the meaning of the adjective "non-significant." As a matter of course, some one combination of congenital variations is "significant" in the sense which the Duke of Argyll chooses to give to that word—a sense in which I do not employ it: some one combination of congenital variations in each generation survives because it is "significant" in the sense of being useful. It is a common fallacy to suppose that natural selection is only operative in producing *new* species; on the contrary, it is never in abeyance, but is equally as active in maintaining an existing form as in producing a new one.

With regard to the origin of useless variations and the general question of uselessness, it is not to be expected that your columns should be given over to an exposition of the common-places of Darwinism. It is to be noted, firstly, that we have no right to conclude that a structure is useless to the organism in which it occurs because the Duke of Argyll is unable to see in what way it is useful; secondly, we have established the great principle of "correlation of growth," which is a brief way of stating that in organisms there is such an intricate binding together of the mechanism that when one part varies other parts vary concomitantly—so that a useful variation of the beak or eyelid of a bird (for example) may *necessitate* a concomitant and perfectly useless variation in the toe-nails or the tail-feathers; thirdly, useless structures undoubtedly exist owing to the potency of heredity, which is of such strength that long after a structure has ceased to be a matter of selection it is transmitted from generation to generation, though dwindled in size and more or less imperfect in structure.

I think there will be no difficulty by reference to one or other of the three considerations above stated in disposing of cases of so-called "uselessness," or "prophetic" functionless organs "on the way to use," which the Duke of Argyll may find to be stumbling-blocks in the way of his faith in Darwin, if he will submit them one by one for pulverization, though I am afraid the process will not interest your readers.

September 21. E. RAY LANKESTER.

A Shadow and Halo.

A FEW evenings ago, whilst walking down a sloping pasture, with the moon shining brightly at an altitude of about 20° behind me, and with no visible dew nor fog, yet with heavy dew on the grass, I noticed that the shadow of my head and shoulders was very sharply defined, but that it was surrounded by a halo of light, and that this halo or nimbus increased in brightness as my shadow was lengthened out because of the increasing slope; and not only was the brightness increased, but it extended now to my hips. That this was due to the greater depth of moist air through which the moon's light passed, by reason of the increase of the slope, I think was proved by the fact that in the neighbourhood of a high hedge, which would to some extent alter the conditions, this halo nearly wholly disappeared. At one time I thought that my eyes were deceiving me concerning this appearance, the contrast of the dark shadow with the surrounding brightly illuminated grass giving rise to the appearance above mentioned, but, by holding up my hand so as to cut off the view of the shadow, I still saw the brighter light which surrounded it, and this brightness still increased or decreased in intensity as the slope on which I took up my position was greater or less. There was no casting of a shadow on a fog-bank, as there was no fog at all, but rather the air was particularly clear. I noticed this phenomenon three nights in succession. I shall be glad to know if any other amongst your readers has noticed this occurrence, and will explain it. E. W. P.

Tamworth, September 29.

Sonorous Sands.

REFERRING to Mr. Carus-Wilson's letter recording the supposed discovery of musical sand in Dorsetshire, I may mention that about two years ago the late Admiral E. J. Bedford sent me three boxes of musical sand, one of them being labelled, "Musical sand; Studland Bay, Dorset, 1885; sonorous when collected." I am not aware whether Admiral Bedford himself discovered the sonorous properties of this sand, but it is clear that he was well acquainted with both the sand and its character in 1885. A. R. HUNT.

Torquay, September 27.

THE REPORT OF THE KRAKATÃO COMMITTEE OF THE ROYAL SOCIETY.

I.

AFTER an interval which has been prolonged partly by the unexpected continuance of the subsequent atmospheric phenomena, and partly through other circumstances incidental to publishing, the Report on the great eruption of the volcanic island of Krakatão in August 1883 is now before the world.

Every Committee is bound to issue a Report of some kind, but it rarely falls to the lot of a Committee to deal with anything at once so stupendous in its character and far-reaching in its consequences as the eruption which not only figuratively, but literally, vibrated through the world on August 27, 1883.

We, in these islands, may boast of our Essex earthquakes, and of the frequent little tremors to which a certain district in Perthshire is subject; but few of us, or our immediate neighbours, can, from our local experience, form the faintest conception of the terrific subterranean powers which ordinarily manifested themselves in the volcanic region of which Krakatão may be fitly termed the *focus*.

The first accounts which reached us by telegram, inaccurate though they were bound to be as regarded details, were scarcely exaggerations in point of magnitude; and, indeed, the cataclysm in this case rose superior to all artificial modes of transmission, by announcing the very date and hour, if not minute, of its culminating explosion through a series of air-waves, which recorded themselves no less than four times on every automatically recording barometer throughout the world.

Three other distinct and abnormal phenomena were: (1) the immense distance to which the sound-waves were propagated (altogether transcending anything hitherto on record; (2) the immense local height, destructive power, and subsequent wide diffusion of the accompanying sea-waves, which in this case were *not*, as is usually the case, due to earthquake action; (3) the simultaneous occurrence in the Javan and Indian area, and subsequently rapid extension, first round the equatorial zone, and, finally, to the whole world, of a most remarkable group of optical phenomena, including coloured suns, lurid and prolonged glows at twilight, large coronæ round the sun and moon, and a peculiar cirriform haze which was evidently connected in some way with these and the eruption.

It was plain, in the face of these preliminary facts, that the collection and discussion of such a grand series of exceptional phenomena gratuitously evolved out of Nature's own laboratory, could not fail to be of service to science, and that while the more local features and practical results of the episode might be left to the Dutch Government, to whom the district belonged, its attendant and subsequent phenomena deserved permanent record in the pages of scientific history.

On this basis, a Committee of the Royal Society was appointed on January 17, 1884, in the following terms:—

"That a Committee, to consist of Sir F. Evans, Prof. Judd, Mr. Norman Lockyer, Mr. R. H. Scott, General Strachey, and Mr. G. J. Symons, with power to add to their number, be appointed to collect the various accounts of the volcanic eruption at Krakatão and attendant phenomena in such form as shall best provide for their preservation and promote their usefulness."

The subsequent expansion of the Committee by cooperation of additional members, and the substitution of one—Captain Wharton—in consequence of the death of Sir Frederick Evans, is detailed in the preface.

The main object of the Committee was thus to collect facts and reduce them into a systematic and useful form. While this has been its primary object, it has been thought advisable to enlarge upon the original basis of the Report, and, while giving a *résumé* of all the leading opinions, especially those relating to the debated question of the relation of the optical phenomena to the eruption, to enter at some length into a discussion of the facts thus systematized. Though it is hardly to be expected that everybody will agree with the deductions arrived at by each author, and though it has been impossible to avoid omissions in a work embracing, in its latter sections, observations extending over three years, and a literature of its own, the main facts have not only been recorded, but, as the Chairman, Mr. G. J. Symons, says, can be readily verified.

Although, therefore, as time progresses, and human knowledge changes and enlarges, some of the conclusions drawn by the authors of the Report may be modified or reversed, the value and permanence of the facts and opinions quoted, will be secured by the unusual care which Mr. Symons has taken to verify all the references and quotations.

The work is divided into five parts:—

I. On the volcanic phenomena of the eruption, including the nature and distribution of the ejecta, by Prof. Judd, F.R.S.

II. On the air-waves and sounds caused by the eruption, prepared under the direction of Lieut.-General R. Strachey, F.R.S.

III. On the seismic sea-waves caused by the eruption, by Captain W. J. L. Wharton, R.N., F.R.S.

IV. On the unusual optical phenomena in the atmosphere which began in 1883 and continued in part up to 1886 inclusive, and which included coloured suns, twilight effects, coronal appearances round sun and moon, sky haze, &c., by the Hon. F. A. Rollo Russell and Mr. E. Douglas Archibald. And

V. A short discussion of the magnetical and electrical phenomena, by Mr. G. M. Whipple.

Prof. Judd commences by pointing out how peculiarly favourable for the gigantic outburst was the position occupied by Krakatão. The marked linear arrangement of the volcanoes of Java and Sumatra points to the existence of a corresponding great fissure in the earth's crust; while across the Straits of Sunda lies another line of weakness, along which five volcanoes have been thrown up at different epochs. Krakatão lies precisely at the intersection of these lines. It is therefore a position where volcanic action, once having commenced, might be expected to display itself on its grandest and most intense scale.

The history of Krakatão, as traced by Prof. Judd, shows that, both in dimensions and activity, it may be considered to have been one of the largest and most destructive volcanic craters in the world. At one time, "its circumference, at what is now the sea-level, could not have been much less than twenty-five miles, and its height above the same datum plane was perhaps not less than 10,000 to 12,000 feet."

Then, at some unknown period, a terrible outburst seems to have occurred, far transcending the present one, which completely eviscerated the volcano, and reduced it to the condition of a basal wreck of three islands, one of which contained Rakata, a basaltic lava cone from which the island derived its name, and two smaller parasitical cones; while the other two represented the relics of the original crater, formed of the same material as the latter, viz. enstatite dacite. The relatively inconspicuous character of Rakata, and the adjacent cones and islets, as well as the absence of any serious volcanic action since 1680, seem to have warded off any suspicions which might have been entertained by the inhabitants on the adjacent coasts regarding either the former grandeur of the volcano or the possible renewal of its activity, certainly on such a scale as was witnessed on August 27, 1883.

Nature, however, rarely displays its grandest effects without giving premonitory warnings, and, in volcanic and seismic phenomena more particularly, by exhibiting the culminating outburst as the cumulative result of an aggregation of small and continuously operating hypogenic causes.

For some years prior to 1883, earthquakes had been of frequent occurrence in the vicinity, one of which destroyed the lighthouse on Java's First Point, and was felt even in North Australia, while on May 20 and 21 an eruption proceeded from Perboewatan, the most northern of the three craters which occupied the place of the original prehistoric volcano, and the same that was in eruption in 1680. This eruption, though only of a relatively mild (Strombolian) type, compared with its successor, was yet sufficiently striking to be accountable for some of the sporadic sky effects which, as we shall see, were noticed in its vicinity during and for some little time after its occurrence. For example, the captain of the German ship *Elizabeth*, when passing through the Straits on May 20, observed the height of the smoke column as it issued from the volcano to be over 30,000 feet, and found dust fall on his ship when it was more than 300 miles distant; while, according to Verbeek, the writer of the Dutch Report, the sounds were heard not merely at Batavia and Buitenzorg, 100 miles off, but even at Singapore, which is 518 miles away.

After this relatively minor, though absolutely violent eruption, a period of intermittent and subordinate activity prevailed, during which two other dormant cones reopened, the decrease in violence being thus probably made up for by the larger area in eruption. Finally, after a period of growing intensity—a fact which was attested by observations at Batavia and on board ships passing through the Straits—the entire volcano appears, on August 26, to have passed from the moderate (Strombolian) stage to the paroxysmal (Vesuvian) stage.

It would be unnecessary to recapitulate the accounts given of this terrific outburst, which lasted from 2 p.m. on Sunday, August 26, to the evening of August 27, and reached its culmination at about 10 o'clock on the latter day. The originals read like romances from the "Arabian Nights," though to attempt to adequately describe such a chaos would need the pen of a Dante coupled with the pencil of a Doré. The salient features were (1) the unusual height to which the smoke column was observed to ascend, viz. seventeen miles, by Captain Thomson, of the *Medea*—the nearest approach to which on any former occasion seems to have been thirteen miles at the eruption of Graham's Island (Julia) in 1831; (2) the extraordinary violence of the detonations; and (3) the accompanying atmospheric and electric phenomena. With respect to this latter point, the volcano was, in fact, a frictional hydro-electric generator of electricity on the largest possible scale.

One of the most important deductions arrived at by Prof. Judd from a study of this and other eruptions is the precise part played by water in aiding eruption.

It appears to be often thought that both slow percolation and the rapid introduction of water into reservoirs of lava are the direct causes of eruption; but Prof. Judd shows that, while the percolation of water is one of the contributory causes, it is not *the* primary cause, which he attributes, when discussing the nature of the materials ejected, to "the disengagement [by heat] of volatile substances actually contained in those materials."

According to this, which may be termed the "cartridge" doctrine of eruption (the lava representing both the powder and shot), the action of inrushes of sea-water, such as occurred in the present case, by chilling the surface of the lava, and augmenting the tension of the imprisoned gases, caused "a check and then a rally," analogous to what occurs in a geyser when sods are thrown into it. Prof. Judd attributes the excessively violent nature of the last stages of the great eruption of Krakatão to this "check and rally action," caused by the dissolution after evisceration of the crateral framework of the volcano, and the consequent admission of the sea in large quantities, a circumstance to which its position rendered it peculiarly accessible.

Prof. Judd considers the "*excessively violent though short paroxysms* with which it terminated" to be the special feature by which the eruption of Krakatão differed from others of similar rank. These, while characterized by a larger quantity of materials ejected, present no parallel to the final "exhaustive explosions of abnormal violence," together with the vast sea and air waves, and the subsequent optical phenomena, which accompanied that of Krakatão.

Prof. Judd next deals with the nature of the materials ejected, and draws attention to the different *physical* characters presented by the lavas ejected from Krakatão at different epochs, the final compact lavas of 1883 being porphyritic pitchstone, and obsidian, containing about 70 per cent. of silica, and so nearly identical *chemically* with those of some of the earlier outpourings as to suggest refusion.

The heavier lava dust which fell in Java and was examined by numerous geologists, including Prof. Judd himself, exhibits a peculiarity which he considers to be without precedent, in that it contains almost every variety of feldspar crystals. The base in which these crystals was found to be embedded presents great differences in its fusibility, the pitchstone melting with great difficulty, and the obsidian with ease. This latter point, in combination with other circumstances, leads Prof. Judd to one of the most important of his conclusions, viz. as to how eruptions come to differ not merely in magnitude but in quality: for example, how a volcano such as Krakatão at one time emits massive and viscous lava-streams as it did in former times; at another, pours forth a more liquid lava; and again, as on this occasion, bursts out with explosive violence into an eruption in which most of the lava is converted into pumice. He considers that the older lavas have been *chemically* acted on by water which has slowly percolated through the crust in the vicinity, and that the new compounds thus produced are not only more readily fusible, but more easily convertible into pumice. Volcanic action is thus concluded to be brought about not directly by the physical action of externally derived water, but by changes in the physical properties of rocks chemically altered through the medium of such water.

In connection with the optical effects which were witnessed subsequent to the eruption, and which are found to be connected chiefly with the finer solid ejecta, Prof. Judd finds evidence, both from a study of the Krakatão pumice as well as the finer dust which fell at great distances, that by the unusual violence of the explosions during the major outburst a large quantity of the very finest threads and dust of volcanic glass was thrown out into the higher atmospheric regions, where it might remain suspended for very long periods. He also points out that the absence of any sign of materials characteristic of Krakatão in the rainfall of distant places is no evidence against their wide diffusion, since the most characteristic substance in the Krakatão dusts was rhombic pyroxene, and this by reason of "its high specific gravity and its slight friability would be among the first to fall."

Prof. Judd brings his section to a close by a general review of the circumstances which have led him to adopt the view already enunciated regarding the cause of volcanic action, viz. that the liquidity of a lava and the violence of an eruption depend on the extent to which the lava has, as it were, been hydrated under the influence of slow aqueous percolation. Lavas of precisely the same composition, and at the same *temperature* may vary greatly in their eruptive action simply by the changes thus effected in their fusion-points. This refined form of the volcanic theory, which is put forward by Prof. Judd, appears to show that the Vesuvian stage of eruption is a paroxysmal form of earth sickness, due to lava gases indirectly generated by water action, while the quiet outpourings both from cones and fissures which have taken place so widely both in the past and present ages, represent the more normal welling up of lava which has been less altered by water action. For this reasonable deduction and the clearer insight afforded into the *modus operandi* of volcanic and seismic phenomena, we are, without doubt, indebted to Krakatão.

(*To be continued.*)

THE BRITISH ASSOCIATION.

SECTION H.[1]

ANTHROPOLOGY.

OPENING ADDRESS BY LIEUTENANT-GENERAL PITT-RIVERS, D.C.L., F.R.S., F.G.S., F.S.A., PRESIDENT OF THE SECTION.

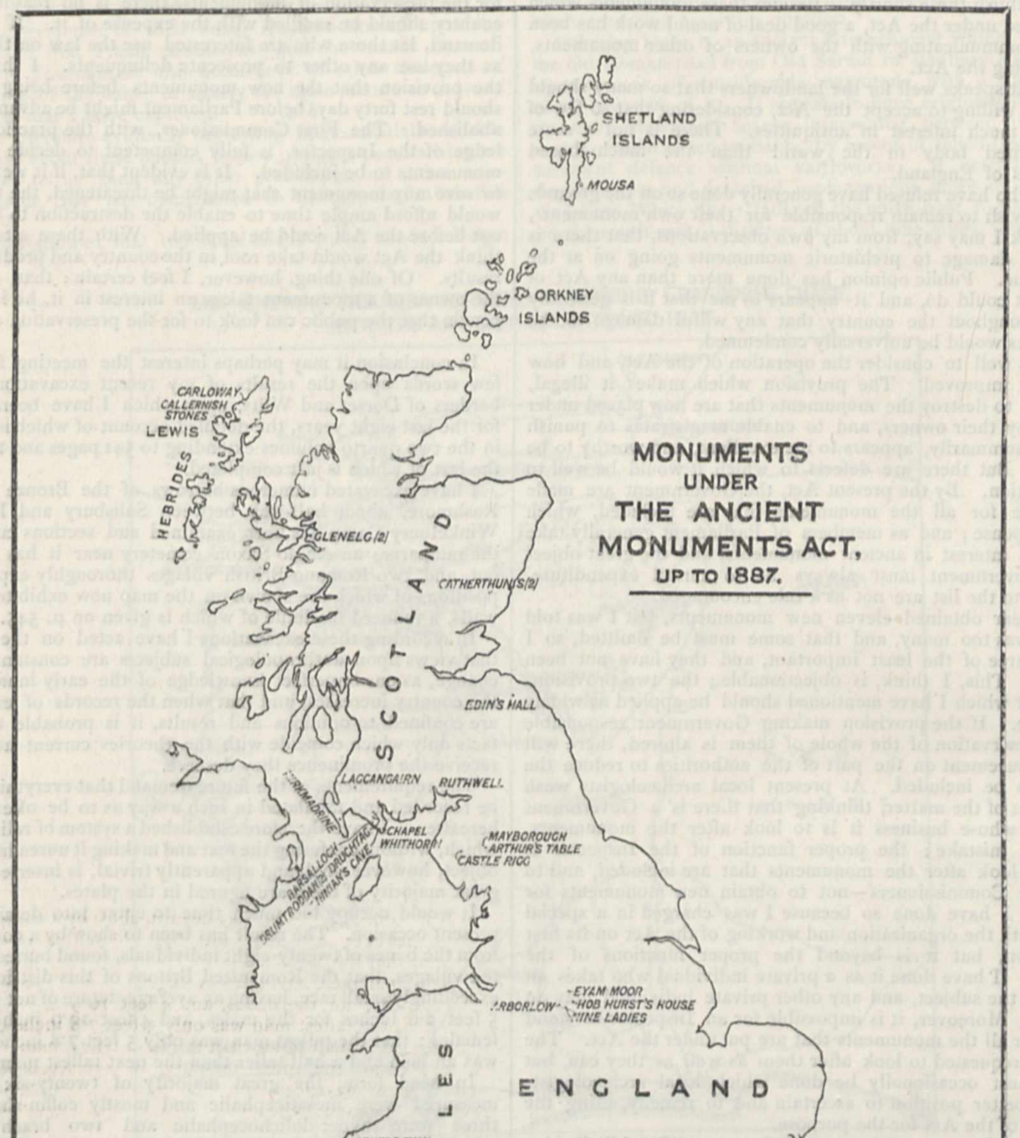
II.

THE accompanying map of Great Britain shows the monuments that I have been the means of obtaining by the consent of their owners.

The Pictish Tower at Mousa in the Shetlands, which is well known to be the best preserved monument of this class in the country, has been included by the owner, Mr. Bruce, and some necessary repairs have been done to it by the Government. In the Orkneys the owners of the scheduled monuments declined to make use of the Act, but they are well looked after. The same applies to the Bass of Inverurie, the Vitrified Fort on the hill of Noath, the Pillar Stones at Newton, in the Garioch, and the British settlement at Harefaulds, in Lauderdale, which latter, however, is in such ruinous condition that the remains of it are scarcely worth preserving. The Suenos Stone near Forres; the Cairns at Clava, on the banks of the Nairn; the Cat-stane at Kirkliston; the Burgh of Clickanim, have also been withheld by their owners, but most of them are very well taken care of. The Cairns at Minnigaff were nearly destroyed before they were scheduled, and are not worth preserving. The inscribed stone in St. Vigean's churchyard is preserved in the porch of the church, but it is not included. On the other hand, Edin's Hall, the largest and most southern of the remains of the Pictish Towers in Berwickshire, has been included by Mr. J. S. Fraser-Tytler; the Black and White Catherthuns have been added by Miss Carnegy Arbuthnot; both these are large camps having ramparts of stones and earthworks round them, and they are described in General Roy's work. The Pictish Towers at Glenelg have been included by Mr. James Bruce Bailey; they are in a very bad state of repair, and have been propped up by the Government. The inscribed stones at Laggangairn, New Luce, have been included by Lord Stair; they are at a great distance from any road or habitation, and the protection afforded them, beyond the powers contained in the Act, must be regarded as nominal. The Peter's stone, on the road from Wigton to Whithorn, has not been added; it is an important stone, and is in a dangerous position; it has already suffered damage, and it is to be hoped it will be included hereafter. The chapel on the Isle of Whithorn, supposed to be that built by St. Ninian, has been included by Mr. R. Johnstone Stewart; this was not in the schedule. The Pillars of Kirkmadrine have been included by Mrs. Ommaney McTaggart; they are the earliest Christian monuments in the country. I suggested that Government should contribute towards building a small chapel to contain them, which has been done. The Cross at Ruthwell, with its remarkable runes, which were gradually being destroyed and covered with lichen, so that its inscription could not be read, has also been added. I suggested that the Government could contribute towards building an annex to the neighbouring church to contain it, which has been done. This was not in the schedule. The cup-marked rock of Drumtrodden, Wigtonshire, has been added by Sir Herbert Maxwell, and Government has granted a certain sum towards building a shed over it to preserve it. It was not in the schedule, but is a good example of its class. Barsalloch Fort, Wigtonshire, the Moat Hill of Druchtag, the Drumtrodden standing stones, Wigtonshire, have also been added by Sir Herbert Maxwell. St. Ninian's Cave, with its early Christian crosses, has been included by Mr. Johnstone Stewart. In the Island of Lewis the remarkable standing stones in the form of a cross at Callernish, and the Broch at Carloway, have been added by Lady Matheson. This latter is, next to Mousa, the best Pictish tower in the country. In Cumberland, the Stone Circle on Castle Rigg has been put under the Act by Miss Edmondson. In Westmoreland, Arthur's Round Table, an earthen circle with a ditch in the interior, and Mayborough, a large circle with an embankment of stones and the remains of a stone circle within, has been included by Lord Brougham. In Derbyshire, Arborlow, a large circle similar to Arthur's Round Table, with the remains of a stone circle, the stones of which are prostrate, and a large tumulus near it, has been added by

[1] Continued from p. 518.

the Duke of Rutland. Hob Hurst's House, and the Circle on Eyam Moor, which also has a large cairn close to it, have been included by the Duke of Devonshire, and the Nine Ladies, a circle of small stones on Stanton Moor, by Major Thornhill. In Gloucestershire, Uleybury, a long barrow with a well-preserved stone chamber, has been added by Colonel Kingscote. In Oxfordshire, the Rollrich stones have been included by Mr. J. Reade. In Kent, Kit's Coty House by Mr. Brassey, which is the remains of a long barrow, the traces of which can be seen, with part of the stone chamber remaining. In Somerset, the



Stone Circles at Stanton Drew, by Mrs. S. B. Coates, and the Cove there by Mr. Fowler; the chambered tumulus at Stoney Littleton by Lord Hylton. It Wiltshire, the long barrow at West Kennet by the Rev. R. M. Ashe, and Silbury Hill by Sir John Lubbock. In Dorsetshire, the chambered long barrow, called the Grey Mare and Colts, near Gorwell, by Mr. A. B. Sheridan; the circle of Nine Stones near Bridehead Park by Mr. R. Williams; the Stone Circle on Kingston Russell Farm by the Duke of Bedford; and in Wales the Pentre Evan cromlech, one of the largest in the country, by Lord Kensington—

making in all thirty-six which have been placed under the Act with the consent of their owners. All these and many others have been surveyed; plans, drawings, and sections have been made of them, which are contained in the book now upon the table, which is open for the inspection of the members. I hope to publish these shortly. Besides these monuments which are included under the Act, a good deal of useful work has been done by communicating with the owners of other monuments, without using the Act.

I think it speaks well for the landowners that so many should have been willing to accept the Act, considering that so few of them take much interest in antiquities. There is not a more public-spirited body in the world than the much-abused landowners of England.

Those who have refused have generally done so on the grounds that they wish to remain responsible for their own monuments, and I think I may say, from my own observations, that there is very little damage to prehistoric monuments going on at the present time. Public opinion has done more than any Act of Parliament could do, and it appears to me that it is generally known throughout the country that any wilful damage to the monuments would be universally condemned.

But it is well to consider the operation of the Act, and how it may be improved. The provision which makes it illegal, ever after, to destroy the monuments that are now placed under the Act by their owners, and to enable magistrates to punish offenders summarily, appears to me excellent, and worthy to be retained. But there are defects to which it would be well to give attention. By the present Act, the Government are made responsible for all the monuments that are included, which entails expense; and as members of Parliament generally take very little interest in ancient monuments, and the great object of the Government must always be to curtail expenditure, additions to the list are not as a rule encouraged.

I last year obtained eleven new monuments, but I was told that this was too many, and that some must be omitted, so I selected three of the least important, and they have not been included. This, I think, is objectionable; the two provisions of the Act which I have mentioned should be applied as widely as possible. If the provision making Government responsible for the preservation of the whole of them is altered, there will be no inducement on the part of the authorities to reduce the number to be included. At present local archæologists wash their hands of the matter, thinking that there is a Government Inspector whose business it is to look after the monuments. This is a mistake; the proper function of the Inspector is simply to look after the monuments that are included, and to advise the Commissioners—not to obtain new monuments for the Act. I have done so because I was charged in a special manner with the organization and working of the Act on its first introduction, but it is beyond the proper functions of the Inspector. I have done it as a private individual who takes an interest in the subject, and any other private individual may do the same. Moreover, it is impossible for an Inspector to stand sentry over all the monuments that are put under the Act. The police are requested to look after them as well as they can, but damage must occasionally be done which local archæologists are in a better position to ascertain and to remedy, using the provisions of the Act for the purpose.

It may be that my position as a landowner, as Lord Stalbridge said in his letter asking me to take the appointment, may have had some effect in enabling me to persuade some of the other landowners, but you cannot insure always having a landowner for an Inspector, and it is desirable now to put the Act on a working footing. It is much to be wished that local Archæological Societies should be made to feel themselves responsible both for the inclusion of monuments under the Act, and their preservation afterwards; the Act arms them with full powers for the purpose if they think proper to use it.

At present no Archæological Society has rendered any assistance whatever, but Sir Herbert Maxwell, in Galloway, has not only offered his own monuments, he has persuaded his neighbours to do the same. What Sir Herbert Maxwell can do, others equally public-spirited can do also, if it is clearly understood that it rests with them to take action in the matter, and I think it should rest with them, because, being local, they can do more than a single Inspector charged with the supervision of the whole of the monuments of Great Britain. I think that the Government should continue to appropriate a small sum (it is now under £200 a year) to apply to such purposes as may be thought desirable, such as building sheds to preserve the monuments, but that they should not necessarily be held responsible for all the monuments placed under the Act, and that, the Bill being a permissive one, it should rest with the public to make use of it or not, as they may think proper. If there is no demand for the preservation of monuments, there is no reason why the country should be saddled with the expense of it. If there is a demand, let those who are interested use the law on the subject as they use any other to prosecute delinquents. I think, also, the provision that the new monuments before being included should rest forty days before Parliament might be advantageously abolished. The First Commissioner, with the practical knowledge of the Inspector, is fully competent to decide upon the monuments to be included. It is evident that, if it were desired to save any monument that might be threatened, the forty days would afford ample time to enable the destruction to be carried out before the Act could be applied. With these alterations I think the Act would take root in the country and produce better results. Of one thing, however, I feel certain: that, as long as the owner of a monument takes an interest in it, he is the best person that the public can look to for the preservation of it.

In conclusion it may perhaps interest the meeting if I say a few words upon the results of my recent excavations on the borders of Dorset and Wilts, upon which I have been at work for the last eight years, the detailed account of which is recorded in the two quarto volumes extending to 541 pages and 159 plates, the last of which is just completed.

I have excavated numerous barrows of the Bronze Age near Rushmore, about half-way between Salisbury and Blandford. Winkelbury Camp has been examined and sections cut through the ramparts; an Anglo-Saxon cemetery near it has been dug out, and two Romano-British villages thoroughly explored, the positions of which are shown on the map now exhibited on the walls, a reduced facsimile of which is given on p. 545.

In recording these excavations I have acted on the principle that views upon anthropological subjects are constantly on the change, as our imperfect knowledge of the early inhabitants of the country increases, and that when the records of excavations are confined to opinions and results, it is probable that those facts only which coincide with the theories current at the time receive the prominence they deserve.

The requirements of the future demand that everything should be recorded and tabulated in such a way as to be of easy access hereafter. I have therefore established a system of relic tables in which, without confusing the text and making it unreadable, every object, however small and apparently trivial, is inserted, and the great majority of them are figured in the plates.

It would occupy too much time to enter into details on the present occasion. The result has been to show by a computation from the bones of twenty-eight individuals, found buried in pits in the villages, that the Romanized Britons of this district were an exceedingly small race, having an average stature of not more than 5 feet 2·6 inches for the males, and 4 feet 10·9 inches for the females; that the tallest man was only 5 feet 7·8 inches, and he was an inch and a half taller than the next tallest man.

In head form, the great majority of twenty-six skeletons measured were mesaticephalic and mostly coffin-shaped, but three were hyper-dolichocephalic and two brachycephalic, which shows that the head form approached that of the Neolithic, long-barrow people, with a probable admixture of either Roman or Bronze Age types.

The stature is slightly less than that given by Thurnam for the long-barrow people of this district, but Dr. Garson informs me that, in a paper which he will read at this meeting, he will show that the height of the Romano-Britains whom I have discovered tallies as nearly as possible with that of some long-barrow bones found near Devizes. All are, of course, shorter than the skeletons of the Bronze Age, two of which I have found in the same locality, and which are of the usual tall stature and round-headed types of that people.

The Romano-Britons are also considerably shorter than the skeletons of the Anglo-Saxons found in the cemetery at Winkelbury, which are described in my second volume.

The problem, therefore, with respect to these Romanized Britons appears to be this: Are they the descendants of the long-barrow people, and do they owe their small stature to that circumstance, or is their small size to be attributed to their largest men having been drafted away into the Roman legions abroad?

Prof. Rolleston examined a number of skeletons from a cemetery at Frilford, which he believed to be Romanized Britons, and found that they were of large size, but in my address to the Royal Archæological Institute at Salisbury, last year, I expressed some doubt about the period of these skeletons, and in a paper since published by Dr. Beddoe I see that he rejects the evidence of their being Romano-Britons upon the same ground that I had doubted it, and he quotes Barnard Davies and Thurnam for the occurrence of other skeletons of these people of the same or nearly the same stature as those of the villages that I have explored.

We are therefore evidently beginning to accumulate reliable information about these people, whose physical peculiarities are less known to us than any other prehistoric, or rather non-historic, race that has contributed to the population of this country.

Thurnam shewed that the large-sized, round-headed Belgæ probably penetrated no further westward than the borders of the district I am speaking of, and that the bowl barrows and the long barrows of the Stone Age predominated to the westward of it.

Since the present volume of my excavations was in print, I have quite recently made another discovery of considerable interest bearing upon this question.

Bokerley Dyke is an ancient intrenchment which cuts across the old Roman road from Old Sarum to Badbury Rings. It is an earthwork of considerable magnitude, with a ditch on the north-east side of it. It appears to have originally occupied all the open downland spaces intervening between the ancient woods, which latter probably, by means of felled trees, afforded sufficient defence without earthworks. It extends with its dependencies and detached prolongations more or less all the way from White Sheet Hill, on the north-west, to Blagdon Hill, on the south-east, a distance of about nine miles. Its origin and

MAP SHEWING THE AREA FORMERLY OCCUPIED BY CRANBORNE CHASE, WITH THE ANTIQUITIES CONTAINED IN IT.

use have been frequently discussed by archæologists, but no one has hitherto assigned a right date to it. I have now cut two broad sections through it on either side of the Roman road, models of which are exhibited, with the result of proving that it is late Roman, or post-Roman, and is of the same date as the villages; Roman coins, to the amount of 500, of late date, extending to Constantinus and Gratianus, and pottery, having been found in both sections, all through the rampart, down to the old surface line. It appears that the dyke had been cut through ground occupied at an earlier date by the Romanized Britons, and that in forming the ditch they threw up the refuse from the habitations to form the bank, including the scattered coins and pottery. A human skeleton of similar character to those found in the villages was also discovered beneath the old surface line in one of the sections, the old surface line being clearly marked over it, showing that it had been buried there before the rampart was thrown over it. From this it appears probable that this dyke was thrown up to defend the Romano-British villages that are situated to the westward or rear of it, from an attack from the east, and that this must in all probability have been done at the time when the Saxon invaders were pressing upon them from the eastward.

This discovery throws a flood of light upon the history of this part of the country at that time, and shows that the Britons must have made a stout defence against their Anglo-Saxon conquerors, sufficient perhaps to account for the apparent predominance of British blood which has been noticed amongst the existing population of the district.

Wansdyke, which runs from a spot not far to the north of the Bokerley Dyke in the direction of Bath, has the same defensive attitude as Bokerley, and the examination of it, which it is proposed to make, will show whether or not it is of the same period.

The observations of Dr. Beddoe and other physical anthropologists upon the present population of the country show that the people of the South-West of England are, as a rule, shorter and darker than those to the eastward, and my own observations upon the people of this particular district will, when they are systematized, tend to define the area of this ethnical frontier more precisely. It would be a remarkable result if it should hereafter be shown that the physical changes observable in the distribution of the existing population are in any way coincident with these lines of defensive earthworks of the Roman or post-Roman age: and if it should be further shown that the same physical characteristics have persistently belonged to the people of this region ever since the time of the Neolithic folk of the long barrows, we shall find ourselves in the presence of anthropological deductions of some value in their bearing on the history of England. I purposely avoid speaking with confidence upon this point, feeling certain that the necessary evidence for deciding the question lies buried in the soil of the district, and will hereafter be unearthed. I shall resume the inquiry as soon as the harvest, if such it can be called this year, is over; but without bias, and with a mind prepared to throw over any preconceived hypothesis the moment it shows itself to be untenable.

SECTION A—MATHEMATICAL AND PHYSICAL SCIENCE.

Members of the Mathematical and the Mechanical Section had a meeting in the rooms of Section A for the special purpose of discussing the question of lightning-conductors. The chair was occupied by Prof. G. F. Fitzgerald, President of the Mathematical and Physical Science Section.

Mr. W. H. Preece, President of the Mechanical Section, opened the discussion, and said that if we wanted to know anything about atmospherical electricity, we had to go back to the works of Benjamin Franklin, 100 years ago. Up to 1870 there were absolutely no rules for the guidance of those who desired to erect lightning-conductors for the protection of buildings. In that year a great Conference was held on the subject, and the result of its deliberations was published in a book, and included a set of rules for the construction of conductors. We had since had great experience of them. He had under his supervision no fewer than 500,000 lightning-conductors. Some time ago a lectureship on atmospheric electricity was founded in memory of Dr. Mann, who experimented on the protection of buildings in South Africa. Prof. Oliver J. Lodge was selected as the lecturer, but, instead of cracking up the work of the Conference, he took the other line, and, if his statements were true, lightning-conductors would be of no use, and no buildings would be safe in a thunderstorm. Prof. Lodge had committed himself to fallacies which it was now his duty to bring before the meeting. The Professor assumed that a lightning-rod formed part of the flash. Well, it did not. Nobody had ever seen a flash of lightning strike a conductor. The function of a conductor was to prevent the possibility of the building being struck by the flash. If it should be struck, there was some defect in the construction of the conductor. Lightning did not go careering wildly about, but passed along a path prepared for it. There was another fallacy, viz. that a flash of lightning was instantaneous. There was no proof of that. We saw a flash of light, which indicated the path of the discharge, but how long the discharge lasted we did not know. There were invisible flashes of lightning, which was proved by the fact that persons had been killed under trees when there was no visible flash. He, however, came to that conclusion from the effect on telegraph-wires, where there were currents of sensible duration, showing that the flash was not instantaneous. The next part was the hardest to discuss. It was the assertion that lightning was oscillatory in its character; that it did not go direct from the cloud to the earth, but went flashing backwards and forwards with considerable frequency. This assertion was based more on mathematical reason than on absolute observation, and engineers had no great respect for mathematical development unless it were confirmed by absolute experiment. The facts against the theory were that electro-magnets were affected for a considerable duration of time by lightning-flashes. Iron and steel were affected, and he had heard letters of the alphabet signalled along the telegraph-wires by a flash—the letter R which needed three signs, C which needed four, and there was a case on record of G, which needed eight signs. Under those circumstances the flash could not be oscillatory unless the oscillations were very infrequent. A discharge from condensers or Leyden jars might be oscillatory, but they were dealing with flashes of lightning. While he was attacking Prof. Lodge in that way, he must say that no one had worked harder or more honestly in the matter. Prof. Lodge had made experiments, and they were correct, from which he deduced that the self-induction of copper was greater than that of iron. He also had repeated these experiments, but his deductions were just the opposite. There was no doubt the Professor was on the brink of a discovery. He had started a fresh hare, which electricians must follow up and kill. Self-induction was called up to explain all the phenomena which they did not understand, and he inclined to think it was very much what the Americans called a bug. In the telegraph science they had known it for many years, and called it electro-magnetic inertia. The next fallacy was that most conductors did not protect any area, but it was known from evidence that they did. He preferred to stand upon the experience of the past rather than upon Prof. Lodge's mathematical assumptions. There was a tendency to hasty generalization among mathematicians, but there could be no doubt that the experiments of Prof. Lodge and others were opening their minds to the true nature of electricity, and that they would in time be able to speak of the mechanical character of electricity. They wanted to know where the energy came from which was so destructive in a flash of lightning. Aqueous vapour condensed and falling as rain at the rate of 1 millimetre per acre per hour developed an energy of 600 horse-power per acre. There was the creation of the energy which only wanted further development to turn into a source of electrical energy. He felt convinced that the result of that discussion would be to establish the truth of the position taken up by the Lightning-Rod Conference, and would bring to the front what they were all anxious to see, the true theory of electricity shadowed forth by Prof. Fitzgerald in his opening address, and that would make this meeting an epoch in the history of electricity.

Prof. Oliver J. Lodge said he had no lightning-conductors under his supervision, and all his conclusions were formed from experiments, and if they were correct very few buildings were effectively and thoroughly protected at the present time; and, further, if his views were correct, lightning-rods would in the future cost very much less than now. The term electro-magnetic inertia seemed to imply that they knew more than they did, so he preferred self-induction until they attained to knowledge. Mr. Preece said that no properly-constructed rod ever failed, but in the report to the Conference there were a number of entire failures named. He had made some very careful experiments in which he provided alternative courses for an electric current, and he found that it required less electromotive force to send the current along a thin iron wire than along a thick copper one. According to Mr. Preece, the object of the conductor was to prevent a flash of lightning, but rods were struck and melted. The conductor had two functions to perform —to act as a point and prevent a flash if it could, and to carry off a flash when it could not help receiving one. The electric charge had some energy, and they could not hocus-pocus it out of existence. It might be better to let it dribble away slowly down a bad conductor than to let it rush headlong down a good one. The length of flash was a question for the consideration of meteorologists, and the duration of flashes was a point on which the same gentlemen might do good work. He had seen flashes which appeared to last two or three seconds, but he thought they must have been a succession of flashes. The fact that flashes deflected the compass-needle did not prove that they were not oscillatory, nor did it prove anything as to their duration. A momentary flash might produce the same effects. There was the question of a flash magnetizing a bar of steel. An oscillating current was able to do that; although Prof. Ewing used an oscillating current to demagnetize steel. The discharge of a Leyden jar caused an oscillating current. The charging was like lifting a pendulum rod suspended freely at one end. When the jar was discharged it was like releasing the pendulum; it must oscillate, and so must the electricity, and its oscillation would vary in accordance with the friction and other modifying causes. The greater the electro-magnetic inertia, the more certainly would there be oscillation. With regard to the protection of areas, the area which Mr. Preece imagined as protected was so small that they might give it him without discussion. There was, however, in his opinion no sure area of protection. Mr. Preece might have pressed him hard on the question of the conditions of a flash. He (the speaker) had assumed that the flash behaved as electricity did in an experiment. The cloud, however, was not like the tinfoil of a Leyden jar; it was made up of globules with spaces between them, and a discharge might be more like that of a spangled jar, or might be dribbled away a bit at a time, and not by great rushes. But they could not assume that it would always do so, and must prepare for the occurrence of a great rush. The true character of lightning must be discovered by observing lightning, and not by experiments in a laboratory. The spark of one induction-coil at a considerable distance would start another one sparking merely by its light. From that he came to the conclusion that when there was a very bright flash of lightning, it must involve very important consequences. There was no doubt that it would cause discharges all over the neighbouring area, and so he would say that areas of protection were misleading, and if a flash had that effect, they had better be without it if possible.

The Hon. Ralph Abercromby, who showed a number of photographs of lightning flashes, said there was no absolute evidence in the photographs of flashes of lightning following each other rapidly on exactly the same path. There was, however, distinct evidence of the tendency of lightning-flashes to occur parallel to each other. There seemed to be a tendency in lightning flashes to be ramified, to give off threads all round the main flash. Photography gave conclusive evidence that flashes were not so instantaneous as was generally supposed. It showed that the flash did not always jump from a cloud straight to the earth, but sometimes went meandering through the air and tying itself into knots, so that it could not be so instantaneous as was imagined. He was of opinion that lightning-clouds were generally more than 500 feet high, but lightning was rarely much higher than 10,000 feet high. By this he did not mean that lightning might jump 10,000 feet from the cloud to the earth; but that at an altitude of 10,000 feet on a mountain-side a thunderstorm was usually *below* the observer.

Lord Rayleigh said that, although some mathematicians were unpractical, yet it was to mathematics one must go to find the results of known causes under new circumstances. He had no special knowledge of lightning-conductors, but from his general acquaintance with electricity he should say that Prof. Lodge's experiments could hardly fail to have a most important practical application to lightning-conductors in the future. Mr. Preece spoke of the development of energy by the condensation of vapour into water, but the question was to find how some of that energy came to take the electrical form.

Sir W. Thomson said that mathematicians never predicted that the Atlantic cable could not be laid, but a celebrated engineer did so. He thought Prof. Lodge was in the American stage of inertia and Mr. Preece in the English stage. He believed that if Prof. Lodge proceeded with his experiments he would confirm his discovery that iron wire was a better conductor than copper. Self-induction was in the air, and they were talking of nothing else. He thought Mr. Abercromby's idea as to the duration was correct. It seemed to him probable that it was the sound of one spark which caused another rather than the light. There was the photograph giving three parallel flashes. It would be well if some experiments could be made to discover whether flashes occurring like that were simultaneous or followed one another, being started by the light or sound vibrations of the first. It was rather startling to find that a lightning-rod had protecting power over so small an area, and he would like to ask Mr. Preece whether copper had been experimentally proved to be better than iron. They could come to one conclusion from what they heard—namely, that houses made of sheet iron would be the safest possible places in a thunderstorm. The question of the effect of self-induction on statical discharges was a very important one. He suggested as a class experiment the discharge of a Leyden jar through a number of students (1) when they were arranged in zigzag rows, so as to have no self-induction in the path of the discharge; and (2) when they stood in a circle, so that the self-induction of the path was a maximum. The students should stand on insulating material. He thought the result of such an experiment would be to show that the students in the middle of the chain would feel the effect of the discharge far less in the second instance than in the first. With reference to the reports as to the occurrence of globular lightning, he believed them to be much exaggerated, and expressed an opinion that the whole effect might be a physiological optical delusion. Reiss experimented some forty years ago on the question of magnetism by jar discharges, and found that the direction of superficial magnetization sometimes was the one to be expected, sometimes the opposite one. He suggested new experiments as to the influence of the rate of oscillation on the result. The most efficient protection for gunpowder against lightning would be, he thought, to put it in a house whose exterior was entirely of iron and to put no lightning-rod on it.

Prof. Rowland observed that the conditions of Prof. Lodge's experiments were scarcely the same as those of actual lightning, and he pointed out that the length of the spark was no measure of the resistance of the conductor. Further, he showed some effects in Mr. Abercromby's photographs which were probably due to the astigmatism in the lens of the camera.

M. de Fonvielle, who spoke in French, observed that Sir William Thomson had said most eloquently that Mr. Preece was taking the English side of the question and Mr. Lodge the American side, but he must say that Sir William Thomson himself had taken the French side, and he had proposed a revolutionary system which consisted in the building of iron houses. He took the liberty, though being a Frenchman, to disagree with the great electrician, and to stand with Mr. Preece as an English conservative, with reference to lightning-conductors. Lord Rayleigh said that mathematicians and physicists should unite together, but he supposed that Lord Rayleigh would agree with him in remarking that Mr. Preece was realizing that alliance in a very remarkable manner, for on the one hand he dealt with a large number of experiments and observations of natural facts, and on the other hand he introduced statistics, or rather the calculation of probabilities, which was one of the highest branches of mathematics. The experiments made in laboratories were different from those which were presented by Nature only so far as they were conducted on very widely different scales. On the previous day, in that hall, M. Janssen had proved by his observations on the action of oxygen on the composition of the electric light that in many phenomena there was a coefficient behind. He congratulated them on the aid they were now receiving from photography. He should advise the meeting to delay its opinion for the time until the completion in Paris of the Eiffel Tower, which would be the most extraordinary lightning-conductor in existence, being 1000 feet high. He must, moreover, state that Paris was practically free from calamities produced by lightning. They had erected a sufficient number of lightning-rods, according to the principles so admirably advocated by Mr. Preece, and that was a strong evidence that Mr. Preece was altogether travelling in the right direction, quite irrespective of any mathematical or physical demonstration.

Prof. George Forbes said that Mr. Preece did not mean to say that mathematicians came to wrong conclusions when they had all the right data, but that they sometimes came to a conclusion without taking all the data into consideration. Prof. Lodge had come to say that if iron was not better than copper, it was at least as good; but they could not be quite prepared to accept that, because the experiments might be tried in instances more nearly approaching the natural conditions, and in that case it was quite possible that copper would be found to be the best.

Sir J. Douglass said that his experience of lighthouses protected by lightning-rods covered a space of forty years, and was comforting to the members of the Lightning-Rod Committee. He never knew a rod fulfilling the conditions he prescribed to fail in protecting the lighthouse and adjoining buildings.

Mr. J. Brown suggested the use of a revolving camera in taking photographs, in order to separate flashes, and thus see if each is single or not.

Mr. Sidney Walker said that anything which would cheapen lightning-conductors would be gladly welcomed. In the cases where damage had occurred, he believed that the result was due to a defect in the conductor. He pointed out that iron would not stand the weather so well as copper, and that, besides, it would be affected by the gases at the top of a factory or similar place.

Mr. G. J. Symons said he had investigated every accident by lightning of which he could hear, and had so got valuable experience. The conclusion left on his mind was that if people would erect conductors precisely in accordance with the rules laid down by the Conference,[1] and fulfilling all the conditions, they would be absolutely safe. Where accidents occurred to buildings with conductors, there was a reasonable explanation to be found. Prof. Lodge's experiments were laboratory experiments, and to get the real facts they must have something on a much larger scale, perhaps by a series of interrupted conductors on posts on the tops of some of those high hills where storms frequently occurred. With regard to protected areas, there were only two cases on record, and those doubtful, of anything being struck within a protected area.

[1] Report of the Lightning-Rod Conference (Spon, 1882).

Dr. Walker said he saw an obelisk on top of a hill struck. The top was knocked off, and the fluid came from the steps of the monument at fourteen different points, ploughing up the ground, and breaking rock at 100 feet distance.

Mr. Wood thought the black flash shown in one of the photographs was due to the reflection of one of the other flashes.

Lord Rayleigh said Stokes attributed that to the combination of gases in the path of the flash causing an opaque stratum.

Prof. Lodge said he could not understand why a conductor should have such a good earth. Why did not three points do at the bottom as well as at the top? If properly constructed conductors never failed, how was it that the hotel at Brussels was burnt, for that was considered protected in the most orthodox way? He would not say that conductors were of no use; they were of great use, but not absolutely certain. In his experiment he was bound to adopt the plan he did, because the experiments could not be done in any other way. It was only the outer surface of the conductor which conducted, and there was no particular good in the centre of a rod. A tube would do as well, and would be all the better if opened out into a flat bar, and yet better than that would be a strand of wires. Iron buildings, to be safe, must have perfect connections, for the smallest gap might give off a spark. That was the danger in houses supplied with gas; if the fluid travelled along the pipes and came to a gap, a spark and a fire might result.

Mr. Preece said the points between Prof. Lodge and himself were reduced to a very small compass indeed. He himself had always been a great advocate of iron on account of its cheapness. The use of copper caused needless expense in the erection of lightning-conductors. He believed every private house could be protected in accordance with the recommendations of the Conference for £1, if people would buy a coil of stranded iron wire a quarter of an inch in diameter, with the finial points, and have that put up.

The President summed up the discussion, and said the principal thing for them to pay attention to was that prevention was better than cure. There could be very little doubt that the presence of a considerable number of conductors afforded a great deal of protection to the area in which it existed, as was shown in the instance of Paris. It was desirable, if possible, that the whole country should be covered with conductors to prevent the discharge of flashes. There was no doubt that, though there might be room for improvement in the conductors, they had on the whole been right.

THE INTERNATIONAL GEOLOGICAL CONGRESS.[1]

II.

IN order to understand the present status of the Congress, and to forecast its probable future, we must briefly note the work done at the two preceding meetings, and compare that with the general results of the meeting just closed. At Bologna the greater part of the time was occupied with discussions upon the exact meanings to be attached to various geological terms, and upon the general principles which should guide us in geological classification. Certain rules were then laid down, which probably few authors have consistently followed, and which it is unlikely will be universally adopted. At Berlin the discussions turned more upon precise questions of classification, especially those relating to the sedimentary rocks; upon the lines by which various groups of strata should be marked off; and, in some cases, upon the names by which these groups should be known. This change of procedure was necessitated by the progress made with the international geological map of Europe; the material for such discussion on classification having been provided in the shape of Reports from various national Committees, of which that from England, presented by Prof. Hughes, was by far the most complete.

At the London meeting the classification of the Cambrian and Silurian strata was fully discussed; and two other questions, only lightly touched upon before, were here considered in some detail—the nature and origin of the crystalline schists, and the upper limit of the Tertiary system.

In Bologna numerous votes were taken, in Berlin several, but in London none. The English geologists were in a majority sufficiently large to carry any point upon which they were fairly well agreed, but no attempt was made to test this; and Prof. de Lapparent, in presenting a Report from the Committee appointed by the Council to consider the question of voting, paid a generous tribute to the English members for their self-restraint. There can be no doubt that the adoption of this Report marks an important epoch in the history of the Congress, and that resolutions hereafter voted will carry more weight than those which at present stand on its records. It recommended that members of the country in which the Congress meets should vote separately from the foreign geologists: if the votes of the two groups agree, the question will be taken as settled; if they disagree, the further consideration of the question will be postponed. The resolution further recommended that votes should not be taken on questions which are purely theoretical—such questions to be simply discussed, and various views obtained; and that decisions of the Congress should only refer to the more practical questions.

Two Commissions of the Congress have existed since the Bologna meeting—that on the Map of Europe, and that on Nomenclature and Classification. The work of the former is plainly marked out, and much has yet to be done. The other Commission has, however, in many respects served its purpose; it has obtained Reports from the various national Committees, most of which have been ably summarized by Prof. Dewalque. The future work of the Congress will partly lie in discussing these Reports, and in deciding such questions in general classification as may apply to wide districts, leaving minor points to be worked out by each country for itself. A Commission was therefore appointed with altered and somewhat wider powers; its functions will more fully shape themselves at the Congress in Philadelphia. As the future progress of the Geological Congress lies so much in the hands of this Commission, it may be desirable to record here the names of its members, which are to some extent the same as those already given (p. 519) for the Council of the London meeting, but there are some additions and changes:—Germany, Zittel; Australia, Liversidge; Austria, Neumayr; Belgium, Dewalque; Bulgaria, Zlatoski; Canada, R. Bell; Denmark, Johnstrup; Spain, Vilanova; United States, Hall; France, de Lapparent; Great Britain, Hughes; Hungary, Szabó; India, Blanford; Italy, Capellini; Mexico, Castillo; Norway, Kjerulf; Netherlands, Calker; Portugal, Delgado; Argentine Republic, Brackenbusch; Roumania, Stefanescu; Russia, Inostranzeff; Sweden, Torell; Switzerland, Renevier. Prof. Capellini was elected President of the Commission; and Prof. Dewalque, Secretary.

The Report upon the Map of Europe was presented to the Congress by Dr. W. Hauchecorne. This stated the progress which is being made. Four or five sheets of Central Europe will be ready for publication during the next two years, and it has been decided to publish the sheets as completed, each with its own title and index, instead of waiting for the completion of the whole of Europe, as was at first intended. A proof sheet (C iv.), containing a large part of Northern Germany, was exhibited; on this there are twenty-four different tints for the sedimentary formations, three for the Archæan, and nine for the eruptive rocks. The map is on the scale of 1 : 1,500,000, and will consist of forty-nine sheets. One colour is taken for each great group—Cretaceous, green; Jurassic, blue; &c. The subdivisions are shown by various modifications of these colours. As a rule, the lower subdivisions are shown by the darker tints, so that the map may be read with more facility than is usually the case with geo-

[1] Continued from p. 526.

logical maps. The map of the British Isles was handed in for publication at the closing meeting. Very little time was given to the map in the public sessions of the Congress, but the Map Commission had three long sittings, the results of which will be printed in the official Report. The most important points arrived at were the adoption of the term *Pleistocene* for the index of the map (the German term "*quartär*" to be bracketed with this); the separation of the modern deposits from the Pleistocene, and the mapping of the latter wherever practicable, the underlying formations (where known) to be distinguished by coloured lines; in modern eruptive rocks (those of volcanoes now active or only recently extinct) the stratified volcanic tuffs are to be distinguished from the cinders and the scoriæ.

M. Karpinski has been the representative of Russia on the Map Commission. On this occasion he was not present, his place being taken by MM. Nikitin and Tschernicheff. The latter submitted an important note on the crystalline schists of the Ural Mountains, which would have enlivened the discussion upon this question in the public meetings of the Congress. He states that the crystalline schists of the Urals contain limestones with a distinct hercynian fauna, and also that the schists pass horizontally into Devonian strata. It is probable that in cases of this kind (and similar cases elsewhere were referred to in the public discussion) the schists will be represented by the colour denoting their presumed age, whilst their present lithological character will be denoted by coloured lines. M. Nikitin raised a point which is important in many parts of Europe, but which is especially so in Russia—that is, the necessity of distinguishing *transition-beds*. He instanced the Volgian beds, which link the Jurassic with the Cretaceous; the Tartarian, between the Permian and the Trias; and others, spoken of by M. Nikitin as Permo-Carboniferous, which link the Permian to the Carboniferous. These transition-beds occupy immense areas in Russia, and cannot well be fitted into the existing classification.

The discussion on the crystalline schists occupied the whole of the sitting on Wednesday, and part of that on Friday. The material for this discussion had been provided by a collection of papers printed in advance and distributed at the opening. Translations from parts of this polyglot pamphlet have now appeared in NATURE. Essays in English were also contributed by five officers of the United States Geological Survey, with an introduction by Major Powell; and by Mr. Lawson, of the Geological Survey of Canada. One by Reusch, on Norway, also in English, was received too late for printing in the pamphlet, but it will appear in the full Report of the Congress.

This discussion derived additional value from the fine collection of rocks, maps, lectures, &c., illustrating this particular subject close at hand in the temporary Museum. The Geological Survey exhibited a large collection of rocks, maps, sections, &c., illustrating the North-West, the Central, and the Southern Highlands of Scotland; important collections of British rocks were also exhibited by Bonney, Blake, Hicks, Callaway, Cole, Hatch, Rutley, Wunsch, and others; foreign rocks were exhibited by Bell from Canada, Delgado from Portugal, Torell from Sweden, Reusch from Norway, Giordano and Mattirolo from Italy; whilst maps, drawings, models, &c., illustrating the discussion, were exhibited by Teall, Baltzer, Cadell, Ricketts, Lapworth, and others. Special mention should be made of the splendid collection exhibited by Heim, illustrating the deformation, crushing, &c., which the rocks of the Alps have undergone. All these exhibits are described in the Catalogue (54 pages with supplement of 4 pages). Several members of the Congress assisted in the arrangement of this Museum, but its success was chiefly due to the labours of Dr. Hinde, Mr. Teall, and Mr. Rudler.

In the foregoing notes we have not attempted to summarize the discussions. These were reported at some length in the *Times* and in other papers. We have preferred to devote the space at our disposal to a general survey of the meeting, and to note some points of importance which could not well be included in a formal report of daily proceedings. As already stated, the discussions may by some be held to have led to no definite result, inasmuch as no vote was taken and therefore no formal decision of the Congress can in future be appealed to. But the great value of such meetings lies in the opportunity afforded for personal discussion and the interchange of opinions, not only in the public sessions, but in the more easy and informal conversations over the exhibits in the Museum, in the corridors and reading-room, and at the friendly and social gatherings which made so pleasant a feature of the London meeting. We have no doubt that the general result of this meeting on geological opinion and progress will be at least as good as that of any which has gone before.

The London Congress was particularly fortunate in its place of meeting. Within the walls of the University of London there was ample accommodation for all the requirements of the Congress, whilst close at hand were the Jermyn Street Museum and the rooms of the Geological Society. Unfortunately the Honorary President, Prof. Huxley, was kept away by ill-health; Prof. Hughes, who has done so much for the Congress in England, was also unable to attend. The early death of M. Fontannes, who has so ably reported the proceedings of previous meetings, is a great loss to the Congress, and many fears were expressed that his place could not be adequately filled; but the labours of Messrs. Hulke and Foster in the Council, and of Barrois and Renard at the meetings, resulted in fuller reports than have appeared of any previous Congress.

REMARKS ON SOME OF THE MORE RECENT PUBLICATIONS DEALING WITH THE CRYSTALLINE SCHISTS.[1]

IN acceding to the invitation of the Geological Congress to contribute to the discussion of the crystalline schists, the author expresses his regret that his time has not allowed him to throw new light by fresh observation on the points of controversy. Other labours have for a long time completely occupied him; so that he has only been able to occasionally assist with advice a younger fellow-worker, Herr Emil Danzig, of Rochlitz, in his researches on the Saxon granulites. This work, which has but recently been brought to a close, and has been placed at the disposition of the members of the Congress, is recommended to the notice of those fellow-workers who are interested in these matters, for in it the granulite question has been completely treated and advanced another stage.

Prof. Lehmann still takes his stand on the results furnished him four years ago by his investigations on the old crystalline schists.

The, on the whole, favourable reception of those investigations assuredly indicates that the right path has been struck, and that an extension of our views on the crystalline schists has resulted from them. This is also proved by the fact that these views have also been successfully applied in other places. That in many cases the opinions advocated by the author have not been rendered quite correctly, cannot excite surprise. Such misconceptions were scarcely to be avoided.

Prof. Lehmann strenuously opposes the notion that his generalizations were made without due consideration, and draws attention to certain criticisms to which his work has been recently subjected.

As is well known, the controversy on the Saxon granulites turns on the question, whether their plainly developed parallel structure is to be regarded as true bedding in the sense of sedimentary deposition, or as of eruptive or plutonic origin. The same questions arise in the discussion of all other districts in which crystalline schists occur; the solution, however, will by no means always be the same. It is beyond doubt that a whole

[1] "Bemerkungen zu einigen neueren Arbeiten über Krystallinisch-schiefrige Gesteine," by Prof. J. Lehmann. Published by the International Geological Congress, London, 1888. (Abstracted from the German by Dr. F. H. Hatch.)

series of crystalline schists are of sedimentary origin, and it is a matter to be decided by detailed investigation which are to be considered as sedimentary and which as eruptive or plutonic. The results obtained by the author in the investigation of the Saxon "Granulitgebirge" and some adjacent districts do not therefore claim universal application.

The tentative interpretations given by him were arrived at by the close ob ervation of the field-relations of the rocks in question during a geological survey extending over several years; and it can now only be a question in how far the interpretation, which has been recognized with certainty as correct f r a series of phenomena, can be applied to other phenomena intimately related to them. The author admits that here and there he has gone somewhat too far in his tentative interpretation. It was scarcely possible, in so difficult a question as the "granulite question," which to-day has not yet reached its final limits, to go just so far that later experience should find nothing to modify.

But the description of the author's work by J. Roth (in a paper on "Zobtenite," read before the Berlin Akademie der Wissenschaften on June 23 of last year) as "a marvellous agglomeration of the most daring hypotheses" is scarcely justifiable.

In Prof. Lehmann's investigations on the crystalline schists it has, for the first time, been shown in the greatest detail that their present condition cannot be original, but must be one that has been influenced by the dynamic processes accompanying mountain-building. He is far from maintaining, however, that similar observations had not already been made; and he readily acknowledges that eminent investigators of the crystalline schists, such as Kjerulf and Michel-Lévy, had, at a much earlier period, made such observations. What is new is the mode and method in which the author utilizes his observations. Researches of this kind were sunk into oblivion: the theory of the sedimentary origin of the crystalline schists had become the ruling dogma; and the *Eozoon canadense* had also made its appearance in Europe.

Roth, in the paper referred to, maintains his old position, according to which the crystalline schists, including the phyllites, are plutonic and unaltered formations.

The evidence advanced by him to prove that the stratiform gabbros, which he terms zobtenite, cannot be numbered with the eruptive rocks is insufficient. The occasional observation of conformable relations with other crystalline schists is inadequate. This does not, however, hinder Roth from regarding it as proved that the Zobten rock cannot be eruptive. The isolated patches of the old rocks that crop out in Silesia are unfortunately extremely confused. The stratigraphical relations of these rocks, which are very highly metamorphosed, cannot be utilized to support either view, and no hope is to be entertained of more favourable exposures in the future.

Prof. Lehmann's views on the Saxon granulites have, in the main, been confirmed by the before-mentioned work of Herr E. Danzig. This work again shows how confused are the field-relations in the granulite-district, and that few exposures permit of an indisputable solution.

In the northern half of the Saxon district the granulite assumes a granular structure, and acquires a marked similarity to some "bedded" granites. These points have received especial attention from Herr E. Danzig. He comes to the conclusion that in many places no sharp line can be drawn between granulite and granite; further, that rocks, which belong undoubtedly to the granulites, present, like the granitic gneisses occurring in the granulite-complex and interbedded with mica-schists, the character of eruptive masses. They contain included fragments, and impregnate these as well as their immediate neighbourhood. The supposition formulated by Prof. Lehmann at the close of his researches in this district is thus confirmed—namely, that the Saxon granulite is a granite *massif*, which has been influenced in structure and composition by dynamic metamorphism.

This confirmation of his work induces the author to explain why he cannot accept the views advocated by E. Reyer in his newly-published work on "Theoretical Geology." Reyer holds the Saxon granulite-*massif* for "a mass of eruptive granite (*Massenerguss*), mantled over by 'tuffogenic' sediments (granulite), through which granite dykes are extruded from the central mass; while granite sheets (*Flankenergüsse*) are intercalated between its beds." Reyer might have gathered from the author's work that the Saxon granulites are, in the main, by no means highly metamorphosed: on the contrary, they deviate very little, in part not at all, from the original structure of eruptive granite rocks.

But apart from this, and without dwelling on the fact that we know absolutely nothing of the rocks underlying the Saxon granulites, the supposition that the alternation of mica-schists with granulite or granitic gneisses has been produced by an accumulation of successive lateral eruptions (*Flankenergüsse*) and precipitated sediments, cannot hold good.

The theoretical considerations of Reyer, the utility of which is gladly recognized by the author, and which in many cases can be supported by direct observation, must not be allowed to prejudice our judgment. The actual facts must first be established, and in so doing we do not encounter the streaky and platy structures which characterize the direction of movement in magmas. We see, in truth, something quite different. The "bedded" granite presents no zones of consolidation that follow closely the surrounding slates; we see rather an extraordinarily uniform mass of granite at first traversing, in a dyke-like manner, the slates, but afterwards insinuating itself between them, in both cases enclosing fragments of the traver ed rock. Where the granite was intruded as a dyke these fragments lie without order, but where it forms a sheet the flat pieces are, almost without exception, arranged parallel to the walls of the dyke. We are accustomed to regard granite, occurring as a dyke, as younger than the rock in which was formed the crack along which the molten rock ascended, without wishing to deny that it has existed, from the very beginning, deeply hidden in the bowels of the earth, and is therefore, in reality, older than the slates it traverses. But it has become customary to observe the convention; indeed, it is necessary to do so if we do not wish to be involved in universal chaos.

For the "bedded" granite it is no simple matter to prove that it is younger than its hanging wall. Attentive examination shows that the apparently conformable boundary has no such very conformable course; that, further, the apparently sedimentary beds are sometimes distinctly detached, and turn out to be loose masses; finally, a whole series of detailed phenomena show that wherever there have been dislocations, the granite has followed the opening and has impregnated the slates. How far such an impregnation can be assumed to have taken place is a matter for personal experience.

In the granite dykes the inclusions and the boundary surfaces of the slates present exactly the same phenomena; only in this case the fragments do not all present a parallel arrangement.

One would be driven to deny the possibility of strata or slate-masses being split parallel to their stratification or their bedding, if we were to deny that the "bedded" granites do not as much constitute a case of intrusion along cracks as do the obliquely-running granite dykes. Why should there not be, among such a number of granite dykes that run unconformably, some that have been formed by the in-filling of cracks (of seldom more than 400 metres width) that follow the divisional planes of stratification or cleavage? It is not to be supposed that these were cavities, the wide sweeping arches of which were supported by the rigidity of the lateral rock-masses: as fast as the slates were separated the granite forced in its way, and filled up the crack as soon as it was developed.

This separation along parallel divisional planes and intimate impregnation with eruptive material, which can be followed in the minutest details with the greatest clearness, arouse the question as to whether the same phenomena have assumed greater dimensions—dimensions that would still be trivial in comparison with the masses erupted. The author has described several exposures in the Saxon granulite district that render any other interpretation impossible.

Kjerulf, Michel-Lévy, and others have described very similar relations among eruptive masses. Michel-Lévy has quite recently given expression to his opinions in a "Note sur l'origine des terrains cristallins primitifs," and in a "Note sur les roches éruptives et cristallines des montagnes du Lyonnais." His statement to the effect that the au hor and a portion of the German school assume a development of heat by the plication of the earth's crust is, so far as the author is concerned, incorrect. On the contrary, he has shown that a c nv rsion of motion into heat has left no visible traces. He is quite at one with the French investigator as to the origin of the heat in the earth's crust.

The chief requisite in the discussion of the crystalline schists, is never to leave the solid ground of facts, and to pay particular attention to the collecting of these. If the statements of some authors are examined, it must awake astonishment to see with what positiveness statements are made, which, although of the greatest importance for the proper judgment of the genesis of rocks, do not correspond in the least to the facts, and by a little attention might easily have been avoided. The author has already had occasion to disprove the non-occurrence of fragments of clay-slate in the phyllite-gneiss of Goldkronach in the Fichtelgebirge, as maintained by Gümbel. A few hours' search sufficed to collect the clearest examples of abruptly fractured and injected enclosures From this occurrence Gümbel drew widely generalized conclusions. Whether it is enclosed fragments or concretions that are contained by a rock, is assuredly of the greatest importance for its proper explication.

The author then refers to the Ober-Mittweida conglomerate. Roth has described the pebble-like fragments enclosed in this rock, as concretions. The author cannot agree with him in this determination, and gives his reasons, which are mainly based on petrographical considerations, why he does not do so.

It will hardly be denied that dynamic processes involved in mountain-building, be the latter referred to whatever cause one will, have not only not been without influence on the structure of the rocks already in existence, but have also considerably influenced the distribution and the intrusion of the eruptive masses. The observations which the author has published on the gabbros of the Saxon granulite district, on banded granulites, on the conglomerates of the Saxon Erzgebirge that have been metamorphosed to mica-schists, and on Bavarian "Pfahlschiefer," have not been refuted, although their re-examination would not have been attended by any especial difficulties.

The author then concludes by expressing the wish, that, in the further study of the crystalline schists, metamorphosed sediments, which can, it is true, be altered to true mica-schists, but never to true gneisses of uniform structure, should be kept distinct from those mica-schists of which we do not know the origin, and from the true granitic gneisses. If this be not done, the false conclusion is inevitable that sediments pass through mica-schists into gneisses. Metamorphosed, gneiss-like sediments, in which allothigenic minerals like feldspar are associated with authigenic quartz and mica, and which are, to the author's mind incorrectly, often designated gneiss, should be described as gneissose greywacke, or as gneiss-greywacke. Some such divisional line must be drawn if we are to obtain any enlightenment on the structure and origin of districts composed of crystalline schists and massive rocks. Dubious schists should be represented by a neutral colour, and not lumped in with the gneisses. The designation gneiss is meaningless as long as the most diversified crystalline and semi-crystalline schists are included under it.

Gneisses, to the author's mind, are granites possessing a parallel structure, which is partly original, partly the result of a more or less intense pressure during, or subsequent to, consolidation. Whatever else may be said, gneisses and granites are things that belong to the same category, and the author cannot reconcile himself to their separation, with respect to origin.

It cannot be natural to arrange in separate petrological categories eruptive granites and non-eruptive granites (gneisses), eruptive diorites and non-eruptive diorites, eruptive gabbros and non-eruptive gabbros, and to treat them from distinct points of view. Plutonic rocks do not differ from the similarly composed eruptive rocks, because they have not left the place in which they were formed.

How the origin of the plutonic rocks is to be conceived—whether as the primordial terrestrial crust, or as produced by the melting down or *diagenesis* of sediments, the latter supposition involving logically a similar origin for a part, at least, of the eruptive rocks—are questions of which the answers are at present less pressing, and which, with certitude, we shall probably never solve.

THE STRATIGRAPHICAL SUCCESSION OF THE CAMBRIAN FAUNAS IN NORTH AMERICA.[1]

A REVIEW of the opinion of American geologists on the succession of the Cambrian faunas shows that all have followed the scheme published by Sir William Logan in 1855 (Geological Survey, Newfoundland), in placing the Paradoxides fauna at the base, and then in succession the Olenellus and Dicellocephalus or Olenus faunas.

[1] Abstract of remarks made by Chas. B. Walcott, of the United States Geological Survey, before the meeting of the International Geological Congress in London, in the course of discussion on the Cambrian System, on September 18, 1888.

The discovery of *Olenellus Kjerulfi* beneath the Paradoxides zone in Sweden led me to re-examine the section of Cambrian rocks in New York, and finally to go to Newfoundland. After long search, I found a complete unbroken section on Manuel's Brook, Conception Bay, that showed the following conformable series:—

	Feet.
a. Archæan Gneisses.	
1. Conglomerate resting unconformably upon *a* ...	35
2. Sandstone, shale, and impure limestone with *Olenellus Bröggeri*,[1] and sixteen species of the Olenellus fauna	25
3. Greenish argillaceous shale	40
4. Red " "	4
5. Limestone	2
6. Greenish argillaceous shales with an abundant Paradoxides fauna at summit	270
7. Dark argillaceous shales, *Paradoxides*, *Microdiscus punctatus*, *Agraulus*, *Conocoryphe*, &c., &c., near base	295
8. Alternating bands of shale and sandstone, with *Orthis* in great abundance	400
Total	1071

Dip of strata, 12° to 15° N. All strata unaltered and undisturbed.

The above section proves that in North America, as in Sweden, the Olenellus fauna is beneath the Paradoxides fauna. This changes the American scheme of classification of the Cambrian system, and places it in harmony with that of Europe. The Olenellus fauna in America includes 42 genera and 112 species, and I now recall 4 genera and 20 species from Europe not known in America, which give a fauna of 46 genera and 132 species beneath the Paradoxides zone.

The following table exhibits the succession of the terranes as now known in America:—

TABLE I.—*Lower Silurian* (*Ordovician*) *System.*

	Subdivision.	Terranes.	Faunas.
Cambrian System.	Upper Cambrian	Potsdam, Knox, Tonto, Belle Isle, &c.	Dicellocephalus or Olenus.
	Middle Cambrian	St. John, Avalan, Braintree.	Paradoxides.
	Lower Cambrian	Georgia, Prospect, Terra Nova.	Olenellus.

A comparison of typical sections of the Cambrian system gives the following:—

TABLE II.

Sweden.	Wales.	Newfoundland.	New York.	Rocky Mountains.
Olenus zone.	Olenus zone.	Olenus zone.	Olenus zone.	Olenus zone.
Paradoxides zone.	Paradoxides zone.	Paradoxides zone.	Represented by other genera than Paradoxides.	Represented by other genera than Paradoxides.
Olenellus zone.	Unknown.[2]	Olenellus zone.	Olenellus zone.	Olenellus zone

It affords me pleasure to recognize the work of the Swedish geologists, and to fully coincide with their results, and thus firmly establish on the two continents the true order of succession of the oldest known Palæozoic fauna.

[1] Name proposed for new species of Olenellus.
[2] After this paper was read, Prof. Lapworth showed me specimens of Olenellus like *O. Bröggeri*, from Shropshire.

NOTES.

WE regret to have to record the death of the well-known traveller, Mr. William Gifford Palgrave. He died in his sixty-third year at Montevideo, where he was British Minister. Mr. Palgrave will be remembered chiefly as the author of the famous "Narrative of a Year's Journey through Central and Eastern Arabia, 1862-63," one of the most brilliant and fascinating books of travel of modern times.

DR. CARNELLY, of University College, Dundee, has been appointed Professor of Chemistry at the University of Aberdeen, in the room of Dr. Brazier, who has resigned.

THE Emperor of Japan has conferred the Order of the Rising Sun, of the Fourth Class, on Mr. Thomas Alexander, Professor of Engineering, Trinity College, Dublin, for services in the Imperial University of Japan.

MR. EDGAR THURSTON, Superintendent of the Government Museum, Madras, expects to arrive in England early in October. We understand that Mr. Thurston has made some valuable collections of corals and other marine animals.

DR. LATHAM will deliver the Harveian oration at the Royal College of Physicians on Thursday, October 18, at 4 o'clock.

THE Exhibition held by the Photographic Society of Great Britain was opened on Monday at 5A Pall Mall East. It will remain open daily, and on Monday, Wednesday, and Saturday evenings, until November 14. Every Monday evening transparencies will be shown with the Society's optical lantern.

THE French Government has reorganized its system of war aërostation. Henceforward the activity of the director of this department will be chiefly concentrated on the manufacture of captive balloons for the several *corps d'armée* and fortifications.

WITHIN a month a new central station for the electric light will be opened at the Palais Royal, Paris, for the shops, the galleries, the Conseil d'État, the Cour des Comptes, the Théatre Français, and the Palais Royal. The building of the cave in which the engines are to be placed in the courtyard is almost finished.

ON Tuesday the seventh International Congress of Americanists was opened at Berlin, in the large hall of the Rathhaus, before a brilliant gathering of archæologists. The opening address was delivered by Herr von Gossler, Minister of Public Worship, who warmly welcomed his hearers in the name of the German Emperor and the Prussian Government, and referred to the distinguished services rendered by the brothers Humboldt in unfolding the secrets of the New World. The Congress will sit till Saturday.

AT the recent meeting of the American Association for the Advancement of Science, Dr. Daniel G. Brinton read an interesting and suggestive paper on the alleged Mongoloid affinities of the American race. He held that the asserted Mongolian or Mongoloid connection of the American race cannot be proved either by linguistics or by physical resemblances. Speaking of the typical, racial American culture, he maintained that it is as far as possible, in spirit and form, from the Mongolian. "Compare," said Dr. Brinton, "the rich theology of Mexico or Peru with the barren myths of China. The theory of government, the method of house-construction, the position of woman, the art of war, are all equally diverse, equally un-Mongolian. It is useless to bring up single art-products or devices, such as the calendar, and lay stress on certain similarities. The doctrine of the parallelism of human development explains far more satisfactorily all these coincidences. The sooner that Americanists generally, and especially those in Europe, recognize the absolute autochthony of native American culture, the more valuable will their studies become."

THE following changes have recently taken place in the editing of German botanical journals. The place of Prof. de Bary, as editor of the *Botanische Zeitung*, has been supplied by Prof. Graf zu Solms-Laubach, of Tübingen, who has recently succeeded the late Dr. Eichler in the Botanical Chair at Berlin; he will act in conjunction with the late Prof. de Bary's coadjutor, Dr. Wortmann. Dr. Kohl, of Marburg, has associated himself with Dr. Uhlworm in the editorship of the *Botanisches Centralblatt*, in the place of Dr. W. J. Behrens, who has been compelled to relinquish the editorship from the pressure of other engagements.

THE interesting and valuable reports on colonial fruit, which have been appearing in the *Kew Bulletin*, are continued in the October number. Much information is given as to fruit in Sierra Leone, the Gold Coast, Lagos, Natal, Malta, Cyprus, Ceylon, the Straits Settlements, and St. Helena.

THE late Mr. Samuel Miller, of Lynchburg, bequeathed to the University of Virginia 100,000 dollars, the income from which was to be expended for "the advancement of agriculture as a science and as a practical art by the instruction therein, and in the sciences connected therewith, of the youth of the country." A part of the income is to be used to maintain the work in agricultural chemistry already carried on at the University; but, according to *Science*, the larger portion of the income will be spent in promoting instruction and research in biology. A biological laboratory is being fitted up, and the equipment has been ordered. The instruction will be by lectures, with associated laboratory-work, and will cover general biology, zoology, comparative anatomy, and biology applied to agriculture. The Professor-elect is Mr. Albert H. Tuttle, recently Professor of Biology in the Ohio State University at Columbus.

AN interesting gas, allene, the isomer of allylene, the second member of the acetylene series of hydrocarbons, has been obtained in the pure state, and its constitution thoroughly investigated, by Messrs. Gustavson and Demjanoff, of Moscow. Very little, and that contradictory, has hitherto been published concerning this gaseous hydrocarbon, which differs so remarkably from ordinary allylene, and yet is represented by the same empirical formula, C_3H_4. The new method of obtaining it i very simple, consisting in the action of zinc dust upon an alcoholic solution of dibrom-propylene. Practically one starts with glyceryl tribromide, $C_3H_5Br_3$, allowing it to gradually drop from a stoppered funnel into a flask containing pieces of caustic potash, and connected with a condenser. The flask is heated in a paraffin bath to about 150° C., when the propylene dibromide distils over as an oil of acrolein-like odour. When the requisite quantity of the glyceryl tribromide has been added, the temperature is allowed to sink to 130°, and water run into the flask. On continuing the distillation the rest of the oil passes over in the steam. The dried and re-distilled oil is then used for the preparation of allene. It is allowed to slowly pass in drops into a second flask furnished with an upright condenser, and containing zinc dust and 80 per cent. alcohol. The flask is heated in a water-bath, and after about twenty drops of the dibromide have entered, the evolution of gas begins, and may be nicely regulated by the speed of dropping. The gas passes by a leading tube from the condenser, and may be stored over water in a gas-holder, being far less soluble than allylene. The gaseous allene thus obtained is colourless, has a peculiar smell, reminding one of its isomer, and burns with a smoky flame. Unlike allylene, however, it yields no precipitate with ammoniacal copper or silver solutions, but gives white precipitates with aqueous solutions of mercury salts. It combines rapidly, under considerable rise of temperature, with bromine, forming a colourless tetrabromide, $C_3H_4Br_4$, liquid at ordinary temperatures, with a camphor-like odour, but condensing to a crystalline mass at − 18°. In this respect, again, it differs from the tetrabromide of allylene, which remains liquid when surrounded by a freezing mixture. The constitution was finally proved to be $CH_2{=}C{=}CH_2$, as expected, the tetrabromide being, consequently, $CH_2Br{-}CBr_2{-}CH_2Br$; while allylene possesses the constitution $CH_3{-}C{\equiv}CH$, being, in fact, methyl acetylene, its tetrabromide being, therefore, $CH_3{-}CBr_2{-}CHBr_2$, a substance very different from the tetrabromide of allene.

INVITATIONS have been issued to each maritime nation to send one or more delegates to attend an International Maritime Conference to meet in Washington on April 17, 1889. The objects of the Conference will be to revise the regulations concerning vessels at sea, to adopt a uniform system of signals to indicate the direction in which vessels are moving in fog, snow, or thick weather, and at night, to convey warnings of approaching storms and other important information, and to formulate regulations for the prevention of collisions. The importance of the subject is so great that a full attendance of delegates is expected.

IN the *Archiv der naturwissenschaftl. Landesdurchforschung von Böhmen*, Band vi. No. 5, 1888, is a valuable memoir by Prof. Franz Klapálek under the title "Untersuchungen über die Fauna der Gewässer Böhmens, Part 1, Metamorphose der Trichopteren," in which the transformations of nearly twenty species of Bohemian caddis-flies are detailed, with illustrative figures and copious introductory general remarks on the internal and external anatomy of the larvæ and pupæ. The author states that the larvæ may be divided into two sections, which he terms "*raupenförmige*" and "campodeoid" respectively, and which correspond pretty nearly with the divisions "inæquipalpia" and "æquipalpia" employed by systematists for the perfect insects. Prof. Klapálek has been very successful in breeding these insects, a matter always attended with difficulty, more especially with those forms that inhabit rapid streams and torrents. A further series of observations will appear next year.

SOME interesting prehistoric remains have been discovered near Basingstoke. Six urns have been disinterred, and stone implements of very rude form have been found in the field in immediate relation with the vessels, although none have actually been discovered buried with the pottery. The site of the interments is a field adjoining Dummer Clump, a conspicuous landmark in the parish of Dummer, and near Kempshott Park, the seat of Sir Nelson Rycroft, who is the owner of the estate. A shepherd was pitching hurdles, when the bar came in contact with a large stone, which, on being removed, was found to have covered two very rudely-formed vessels, of which the under one was pronounced by Dr. S. Andrews, of Basingstoke, to contain human bones which had undergone incineration. Subsequently, another urn was removed, of a much coarser character, bearing a band round the base of the rim ornamented with sunken dots. All the vessels are hand-made and apparently fire-baked, and the larger ones have suffered some damage from the plough, which must have repeatedly passed over them.

THE new number of the *Internationales Archiv für Ethnographie* (Band i. Heft 5) will fully maintain the reputation of this excellent periodical. Among the contents are an article on arrows from Torres Straits, by Dr. M. Uhle; a note on a singular mask from Boissy Island, North-East New Guinea, and queries on the lizard in the folk-lore of Australasia, by Prof. H. H. Giglioli; and a paper on the chewing of the betel-nut, by F. Grabowsky. The coloured illustrations, as usual, are admirable.

FISHING is to be resumed this season at the Sild oyster-banks, on the coast of Jutland, which have been preserved for six years. The oysters are reported to be plentiful and in splendid condition.

THE additions to the Zoological Society's Gardens during the past week include a Grivet Monkey (*Cercopithecus griseo-viridis* ♂) from North-East Africa, presented by Lord Archibald Campbell; a Rhesus Monkey (*Macacus rhesus* ♀) from India, presented by Major Dudley Buckle, R.A.; a Bonnet Monkey (*Macacus sinicus* ♂) from India, presented by Mr. G. C. Gosling; two Sooty Mangabey Monkeys (*Cercocebus fuliginosus* ♀ ♀) from West Africa, presented by Mr. Edward Felton, R.E.; an Ocelot (*Felis pardalis* ♂) from Pernambuco, presented by Mr. E. Percy Bates; a Weka Rail (*Ocydromus australis*) from New Zealand, presented by Mr. H. Lindsay; a Rose-crested Cockatoo (*Cacatua moluccensis*) from Moluccas, presented by Miss Eve; a Puffin (*Fratercula arctica*) from Cornwall, presented by Mr. J. Muir Drew; a Common Snake (*Tropidonotus natrix*), a Common Slowworm (*Anguis fragilis*), British, presented by Mr. P. S. Hutchinson; a Common Viper (*Vipera berus*), British, presented by Mr. A. H. N. Smith; four European Tree Frogs (*Hyla arborea*), European, presented by Mr. Lionel A. Williams; two Grivet Monkeys (*Cercopithecus griseo viridis* ♂) from North-East Africa, deposited; a White-backed Trumpeter (*Psophia leucoptera*) from the Upper Amazons, received in exchange; two Collared Fruit Bats (*Cynonycteris collaris*), an Axis Deer (*Cervus axis* ♂), a Canadian Beaver (*Castor canadensis*), four Chilian Pintails (*Dafila spinicauda*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

THE SATELLITES OF MARS.—These faint objects have been successfully observed, during the late opposition, with the great telescope of the Lick Observatory. The building operations prevented the observations being carried on systematically, but measures of distance and position of one or both satellites were obtained on nine evenings between April 9 and April 28, and Phobos was seen as late as July 18, when the theoretical brightness of Mars was but one-tenth of what it was at the opposition of 1877, or one-fifth of what it will be at the coming opposition of 1890. A preliminary reduction of the observations gives the following corrections to the times of elongations as given by Mr. Marth in the *Monthly Notices* of the Royal Astronomical Society, and by the *American Nautical Almanac* respectively:—

	Marth.	American *N.A.*
	h.	h.
Phobos ...	+ 0·427 ...	− 0·33
Deimos ...	+ 0·020 ...	+ 0·35

Mr. Keeler, who made the observations, remarks (*Astr. Journ.*, No. 178) that, so far as his estimates of the brightness of the satellites go, they support Prof. Pickering's conclusion that Deimos is one half-magnitude brighter when on the eastern side of the planet than when on the western.

TOTAL LUNAR ECLIPSE OF JANUARY 28.—No. 4 of vol. xviii of the Annals of the Harvard College Observatory contains an account of the observations made there of the eclipse of the moon of January 28. The observations were of three classes—first, of the occultations of Dr. Döllen's list of stars; secondly, of the variation in the actinic brightness of the moon; and thirdly, the search, by means of photography, for a possible lunar satellite. In this second inquiry Mr. W. H. Pickering found that the photographic brightness of the full earth was 23·6 times as great as that of the full moon, equivalent to an albedo of 1·7 times that of the moon. The diminution in brightness ascribed to the moon during eclipse is most remarkable, Mr. Pickering giving the uneclipsed full moon as 1,400,000 times as bright as during the central phase, or about *twice* the ratio existing between the sun and full moon. In the search for the satellite a succession of photographs were taken, the telescope being made to follow the moon's motion as closely as possible, so that the stars were represented by short trails. A satellite would have left a trail inclined to the star trails and of a different length. The result of the search was negative, and as a satellite of the tenth magnitude, would have been registered on the plates, it appears probable that the moon has no satellite more than 200 metres in diameter, unless it was involved in the shadow of the earth during the eclipse, or was very dark, or was moving with the same speed amongst the stars as the moon, but in the opposite direction, in which case it would have been mistaken for a star.

PHOTOMETRIC OBSERVATIONS OF ASTEROIDS.—It has frequently been suggested that the asteroids, shining by reflected light, and subject, it might be assumed, only to variations the amount of which could be calculated for any required date, would prove specially useful as standards of brightness in the photometric observation of the fainter stars. Mr. Henry M. Parkhurst has carried out recently a series of observations on several of these bodies, which throws considerable light on their suitability for such a purpose. His method of observation was to note the time which the asteroid took to disappear after passing a transit-wire, the telescope being stationary, and the light of the asteroid or comparison-star suffering diminution either by a wedge or more frequently by a deflector—a piece of glass with nearly parallel sides, placed in the telescope tube, about one-seventh of the way from the focus, and covering half the field. The results of Mr. Parkhurst's observations, which embraced eighteen asteroids, and extended over nearly nine months—April to December 1887—are given in No. 3 of vol. xviii. of the *Annals of the Harvard College Observatory*, and show that the asteroids are not appreciably self-luminous, and that the sun undergoes no noteworthy fluctuations in light in periods of a few days; nor, as a comparison with observations made in some former years would indicate, in more lengthened periods. But they also show that the phase-correction is not covered by allowing simply for the decrease in the area illuminated—a further correction is needed, and one peculiar to each asteroid. In two cases, also, Harmonia and Iris, several of the observations stand out in strong contrast to the rest, and appear to indicate a variation due to axial rotation, the planet probably being irregular in shape, or its surface in reflecting power. No variation depending, as in the case of Saturn's ring, on the position of the asteroid in its orbit, and the relative position of the earth, has been noticed, but this inquiry has only been extended to the four asteroids first discovered. The mean error of an observation, when the special phase correction and probable variations due to rotation have been allowed for, appears to be less for an asteroid than for the fixed stars, the mean error of an observation of the solar illumination in the inquiry referred to above being given as 0·116m.

NEW CATALOGUE OF VARIABLE STARS.—Nos. 179 and 180 of *Gould's Astronomical Journal* contain a new catalogue of variable stars by Mr. S. C. Chandler. Mr. Chandler is not only a diligent observer of variable stars, the discoverer of several, and a zealous computer of the elements of their variations, but several years ago undertook an important and much-needed work, viz. the complete study of the bibliography of known and suspected variables. This catalogue coming from his hand, therefore, will be especially valuable, and the more welcome since it is thirteen years since Schönfeld published his second catalogue. Mr. Chandler puts it forward as merely a preliminary publication, a second more definitive being designed to follow as soon as the investigations now in hand shall have been completed. The present catalogue is no mere compilation. Almost every star in it visible from the latitude of Boston has been observed by Mr. Chandler, who has also gathered together and discussed every available published observation. The catalogue embraces 225 stars, and of these the variations of 160 are distinctly periodic; for 12 the periodic character is ill-defined, 14 are irregular, 12 are Novæ, and the remainder have been too little observed for the character of the variation to be properly known. Of the 160 periodic stars, the elements of 124 are the results of Mr. Chandler's own work, 22 are Schönfeld's, and 14 those of other computers after Mr. Chandler had carefully confirmed them. A point sure to lead eventually to an important advance in our knowledge of the cause of variation has received much attention from Mr. Chandler, viz. the systematic perturbations shown by so many of the periods, and a table is given of these inequalities for 26 stars. A useful novelty is introduced in the numeration of the stars of the catalogue, for, instead of giving them consecutive numbers, each is distinguished by a number equivalent to one-tenth of its R.A. for the mean equinox of 1900·0, expressed in seconds of time, thus securing that the numeration need not be disturbed by fresh discoveries.

MINOR PLANET NO. 275.—This object has been named Sapientia.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 *OCTOBER* 7–13.

(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 October 7

Sun rises, 6h. 13m.; souths, 11h. 47m. 40·2s.; sets, 17h. 22m.: right asc. on meridian, 12h. 53·9m.; decl. 5° 46′ S. Sidereal Time at Sunset, 18h. 29m.

Moon (at First Quarter October 12, 5h.) rises, 8h. 18m.; souths, 13h. 38m.; sets, 18h. 46m.: right asc. on meridian, 11h. 44·6m.; decl. 10° 51′ S.

Planet.	Rises.	Souths.	Sets.	Right asc. and declination on meridian.	
	h. m.	h. m.	h. m.	h. m.	° ′
Mercury..	8 47 ...	13 19 ...	17 51 ...	14 25·7 ...	17 24 S.
Venus ...	8 27 ...	13 17 ...	18 7 ...	14 23·4 ...	14 7 S.
Mars ...	12 18 ...	16 3 ...	19 48 ...	17 9·5 ...	24 33 S.
Jupiter ...	10 50 ...	15 2 ...	19 14 ...	16 9·2 ...	20 28 S.
Saturn ...	0 46 ...	8 16 ...	15 46 ...	9 21·8 ...	16 17 N.
Uranus ...	6 27 ...	11 59 ...	17 31 ...	13 5·2 ...	6 17 S.
Neptune..	19 10* ...	2 56 ...	10 42 ...	4 1·3 ...	18 54 N.

* Indicates that the rising is that of the preceding evening.

Occultations of Stars by the Moon (visible at Greenwich).

Oct.	Star.	Mag.	Disap.	Reap.	Corresponding angles from vertex to right for inverted image.
			h. m.	h. m.	° °
11 ...	B.A.C. 6524 ...	6½ ...	20 35 ...	21 17 ...	72 0
12 ...	B.A.C. 6889 ...	6 ...	19 53 ...	21 6 ...	110 311
13 ...	20 Capricorni ...	6 ...	19 5 ...	19 55 ...	153 240

Oct.	h.	
7 ...	4 ...	Venus in conjunction with and 5° 6′ south of the Moon.
7 ...	5 ...	Mercury in conjunction with and 8° 8′ south of the Moon.
8 ...	6 ...	Mercury at greatest elongation from the Sun 25° west.
9 ...	1 ...	Jupiter in conjunction with and 3° 33′ south of the Moon.
9 ...	22 ...	Mercury in conjunction with and 3° 9′ south of Venus.
10 ...	3 ...	Mars in conjunction with and 4° 38′ south of the Moon.
10 ...	13 ...	Uranus in conjunction with the Sun.

Saturn, October 7.—Outer major axis of outer ring = 38″·9: outer minor axis of outer ring = 9″·8: southern surface visible.

Variable Stars.

Star.	R.A.	Decl.		
	h. m.	° ′		h. m.
U Cephei	0 52·4 ...	81 16 N. ...	Oct. 6,	3 52 *m*
			,, 11,	3 32 *m*
Algol	3 0·9 ...	40 31 N. ...	,, 5,	1 8 *m*
			,, 7,	21 57 *m*
R Aurigæ	5 18·3 ...	53 28 N. ...	,, 7,	*m*
T Monocerotis ...	6 19·2 ...	7 9 N. ...	,, 10,	5 0 *m*
U Monocerotis ...	7 25·5 ...	9 33 S. ...	,, 12,	*m*
S Cancri	8 37·5 ...	19 26 N. ...	,, 11,	0 27 *m*
R Crateris	10 55·1 ...	17 43 S. ...	,, 7,	*m*
U Ophiuchi... ...	17 10·9 ...	1 20 N. ...	,, 11,	19 0 *m*
β Lyræ...	18 46·0 ...	33 14 N. ...	,, 7,	0 0 *M*
S Sagittarii	19 12·9 ...	19 14 S. ..	,, 11,	*M*
S Vulpeculæ ...	19 43·8 ...	27 1 N. ...	,, 11,	*m*
χ Cygni	19 46·3 ...	32 38 N. ...	,, 9,	*m*
η Aquilæ	19 46·8 ...	0 43 N. ...	,, 12,	21 0 *M*
R Sagittæ	20 9·0 ...	16 23 N. ...	,, 11,	*m*
T Vulpeculæ ...	20 46·7 ...	27 50 N. ...	,, 12,	2 0 *M*
Y Cygni	20 47·6 ...	34 14 N. ...	,, 8,	3 0 *m*
			,, 11,	3 0 *m*
δ Cephei	22 25·0 ...	57 51 N. ...	,, 7,	20 0 *M*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A.	Decl.	
Near η Persei	42 ...	55 N. ...	Slow.
,, θ Geminorum ...	102 ...	34 N. ...	Swift; streaks.
	135 ...	80 N. ...	Swift; streaks.
,, κ Cephei	305 ...	77 N. ...	Slow; faint.

GEOGRAPHICAL NOTES.

A TELEGRAM from Mr. Joseph Thomson, dated Mogador, September 19, reports that he has been successful beyond expectation in his exploration of the Atlas Mountains. He left Morocco city on August 27, and after being driven back from the Urika Valley to the south-east of the city, he proceeded eastwards, and succeeded in crossing the range southwards from Imintanut into the Sûs district. From Rezaya he ascended the main range to nearly 13,000 feet. Mr. Thomson intended to return to Hava for a few days, and afterwards to proceed northwards to Fez, Mequinez, and Tangier, returning home about the middle of December.

THE Report for 1887 of H.M.'s Special Commissioner for British New Guinea, contains information of considerable geographical interest. This is especially the case with the Report of Deputy-Commissioner Milman, who has charge of the western district, lying between the Dutch boundary and the Aird River. Mr. Milman refers to the discoloration of the sea about the coast between Talbot Island and the Fly River, due, doubtless, to the vast bodies of fresh water that empty into the sea from the Fly, Tait, Katoer, Mai-Kassa, and other rivers. The Fly River, as far as it has been ascended by Mr. Milman, is thickly populated with a purely agricultural and hunting people, living in large communities; while some houses in the villages are over 200 feet in length. As the river is ascended, traces of careful cultivation are seen here and there on the banks, the gardens or plantations being kept free from weeds, and planted with crotons and other bright-leaved shrubs between the bananas or other fruit-trees, besides being systematically irrigated by dykes cut at regular intervals, which, filling at high water, remain full as the water recedes. About 60 or 70 miles above Soomaioot several large creeks or rivers join the main river, but whether they are flowing into the river, or only form other mouths of this vast system, remains to be proved. The shores of the Fly River, as far as Mr. Milman ascended, are uniformly low, but owing to its great width he is inclined to think they are not subject to inundation. A tidal wave or bore, according to Mr. Milman, ascends the river, but only on the right bank, which accounts for previous visitors not having noticed it. A marauding tribe coming from the westwards have been in the habit of making attacks on the people in the neighbourhood of Sabai Island, but the exact locality they come from is a mystery. Their language and customs are entirely different from those of the Sabai Island people. They had probably never seen a white man until the Rev. E. B. Savage (who happened to be at Sabai when their lights were seen on the mainland) fearlessly visited their camp, and tried to hold some intercourse with them. He describes them as a much lighter race than the rest of the New Guinea natives, and as having long straight hair, while some of them have their nasal-bone pierced in three places, into which are introduced pieces of bone or shell. They appeared entirely unacquainted with fire-arms. Civilization has so far advanced at Port Moresby that a reading-room has been erected, in which the *Times* and other English journals are kept, a hotel has been opened, and a supply of water laid on by means of pipes to the native village.

A RUSSIAN scientific explorer, M. K. Nossilof, has recently returned to Archangelsk from Novaya Zemlya, where he spent a year, from the summer of 1887 to August 1888. He has brought with him rich botanical, zoological, and mineralogical collections, and means to return to the island soon, as he has resolved to devote five years to its exploration. M. Nossilof is reported to have discovered beds of iron, copper, coal, gold, and sulphur, some of which, he believes, could be profitably worked. Among other results obtained by him are many interesting observations on the animal, especially the bird, life of the island, thirteen months' meteorological observations, surveys covering 2500 square kilometres of land, observations on the ice-conditions of the east and west coasts, and 125 kilometres of coast survey. He has, moreover, discovered three new islands. During the winter and spring, M. Nossilof undertook excursions into the Kara Sea, and he hopes by-and-by to undertake a series of soundings as far as the River Yenissei. In the coming winter he intends to fix his station at the east end of Matotshkin Schar, and to establish there a second meteorological station, making excursions along the coast and into the interior.

ELECTRICAL NOTES.

THE Volta Prize of 50,000 francs has been awarded by the French Institute to M. Gramme for his labours in introducing and perfecting the continuous-current dynamo. The prize is given to the inventor who has formed a memorable epoch in the history of electricity. M. Gramme is a Belgian by birth, but a Parisian by residence. He is entirely a self-taught, self-made man. Although Gramme was anticipated by Pacinotti, his invention was entirely independent, and Pacinotti's was completely dormant, and would probably have remained hidden and unknown but for Gramme's success. No one will contend that the prize has not been richly deserved.

CONSIDERABLE attention has recently been drawn to some experiments by Chappuis and Maneuvrier, in Paris, on the decomposition of water by alternate currents. It is well to point out that the whole question was thoroughly threshed out by Sir W. Thomson in 1853, and his paper in the June number of the *Philosophical Magazine* of that year gives all that is necessary to know on the subject. Jamin, in 1882, showed how electrolysis could be performed by alternate currents by inserting an arc in circuit, the opposing E.M.F. of the arcs producing a partial rectification of the alternate currents. Mr. J. F. Kelley has just repeated the experiment in Newark, U.S.A.

MR. LOWRIE (B. A., 1888), showed how the insertion of an opposing E.M.F. in an alternating-current circuit enables electrolysis to be effected and how it could be utilized to measure the electrical energy consumed in electric light installations. If a decomposing cell of copper sulphate, and a constant E.M.F. such as a secondary cell, be inserted in the circuit, the current in one direction is assisted, while that in the reverse direction is opposed, and the cell is acted upon by the difference: an average current flowing, depositing copper at the same rate as if no alternate currents were present. 0·23544 gramme of copper is deposited per kilowatt-hour, or every gramme of copper deposited means 4·205 kilowatt-hours expended.

PROF. EWING (*Philosophical Magazine*, September 1888) has published, with additions, the paper read by him and Mr. Low at the Manchester B. A. meeting, on the influence of a plane of transverse section on the magnetic permeability of an iron bar. A joint between two portions of an iron core possesses distinct magnetic resistance even when the surfaces are true planes. Compression reduces this resistance in the rough faces and eliminates it when the faces are true planes. In all cases the resistance greatly diminished as the point of saturation was approached. A film of gold leaf interposed between the faces and compressed has only a very little injurious effect. Compression, however, reduces the permeability of the solid core for moderate magnetizing forces, though the contrary effect occurs when the magnetization is strong. Villari found the same reversal in the case of longitudinal pull, but in the opposite direction.

LORD RAYLEIGH (B. A., 1888) has been endeavouring to discover if an electric current flowing through an electrolyte causes the velocity of light to vary through the liquid. He experimented with dilute sulphuric acid. The result was negative within the range of the experiment, which was extremely delicate. In H_2SO_4 diluted, one ampere per square centimetre does not alter the velocity of light by one part in thirteen millions, or by 15 metres per second.

IT is estimated that in the United States there are 5351 electric light plants and stations working 192,500 arc and 1,925,000 glow lamps, and consuming 460,000 horse-power. There are thirty-four electric railways, 138 miles in length, run over by 223 motor cars using 4180 horse-power.

SIR WILLIAM THOMSON (B. A., 1888) dealt with the diffusion of rapidly alternating electric currents in the substance of homogeneous conductors. The surface is affected first, and the depth to which the disturbance penetrates depends on the frequency of the alternations. With a frequency of 150 per second a cylindrical copper conductor is said to be penetrated to a depth of 3 mm. Hence, if this be true, conductors for powerful alternating currents such as are used in the Gaulard and Gibbs system, should be tubes or flat bars with a thickness of 6 mm.

TROUVELOT has by photography obtained effects which lead to the conclusion that flashes of lightning may last several seconds. He gave his apparatus a slight horizontal displacement, and found a broad ribbon-shaped band on his plate.

NOTES ON METEORITES.[1]

IV.

Meteorites are Bodies which, like the Earth itself, revolve round the Sun.

WE have seen that the phenomena which accompany meteorites entering our air, whether they are soon burnt up and give rise only to the appearance of a shooting or falling star, or whether they are bulky enough to withstand the melting process till they reach the earth's surface, are similar. We are now in a position to discuss the origin of all these phenomena on the assumption that they have a common cause.

It is not so many years ago since the planetary spaces were supposed to be untenanted by anything more tangible than that mysterious fluid called ether. This notion is exactly represented by the French equivalent for those spaces, *le vide planétaire.* Hence, not to mention imagined supernatural causes—such as that, for instance, embodied in the tradition that Saint Lawrence, on the anniversary of his martyrdom (August 10), shed burning tears—the cause of the phenomenon was ascribed to atmospheric perturbations, exhalations of sulphur, *ignes fatui*, and so forth. An account of the August shower of 1857, even, published in the *Bulletin de l'Académie Royale de Belgique*, is accompanied by a minute record of rain, temperature, atmospheric electricity, &c.

Leaving out of consideration the opinions of the ancients, among whom Anaxagoras and Seneca may be especially mentioned, as being in favour of a cosmical origin, it may be pointed out that Kepler[2] regarded meteorites and shooting-stars as akin, and derived both from the ethereal regions.

Halley was the next to express an opinion that shooting-stars were of cosmical origin, but to Chladni belongs the credit of having broached the theory which modern observations have so abundantly justified. This theory was that space was full of the matter which, attracted by the earth, entered its atmosphere, accompanied by luminous effects only in some cases, and by actual falls of the matter in others.[3] The general acceptance of this view was retarded by Laplace and others, who saw a more probable origin for the phenomena by supposing meteorites to be masses shot out of lunar volcanoes. The first step in the demonstration of such an origin, which is now universally accepted, was made when Chladni,[4] in 1794, showed that no known terrestrial agency was capable of producing masses like the meteorites which had been seen to fall. At his and Lichtenbergh's suggestion, Brandes and Benzenberg in 1798 showed that, whatever they appear to do, shooting-stars never shoot upwards, but always downwards towards the earth. At the same time he showed the similarity of phenomena presented by fire-balls, shooting-stars, and the fall of meteorites, to which we have already called attention. He subsequently returned to and strengthened this view.[5]

"Should it be asked how such masses originated, or by what means they were brought into such an insulated position, this question would be the same as if it were asked how the planets originated. Whatever hypothesis we may form, we must either admit that the planets, if we except the many revolutions which they may have undergone, either on or near their surface, have always been since their first formation, and ever will be, the same; or that Nature, acting on created matter, possesses the power to produce worlds and whole systems, to destroy them, and from their materials to form new ones. For the latter opinion there are, indeed, more grounds than the former, as alternations of destruction and creation are exhibited by all organized and unorganized bodies on our earth; which gives us reason to suspect that Nature, to which greatness and smallness, considered in general, are merely relative terms, can produce more effects of the same kind on a larger scale.

"But many variations have been observed on distant bodies, which, in some measure, render the last opinion probable; for example, the appearing and total disappearing of certain stars, when they do not depend upon periodical changes. If we now admit that planetary bodies have started into existence, we cannot suppose that such an event can have otherwise taken place, than by conjecturing that either particles of matter, which were before dispersed throughout infinite space, in a more soft and chaotic condition, have united together in large masses, by the power of attraction; or that new planetary bodies have been formed from the fragments of much larger ones that have been broken to pieces, either perhaps by some external shock, or by an internal explosion. Let whichever of these hypotheses be the truest, it is not improbable, or at least contrary to nature, if we suppose that a large quantity of such material particles, either on account of their too great distance, or because prevented by a stronger movement in another direction, may not have united themselves to the larger accumulating mass of a new world; but have remained insulated, and, impelled by some shock, have continued their course through infinite space, until they approach so near to some planet as to be within the sphere of its attraction, and then by falling down to occasion the phenomena before mentioned.

"It is worthy of remark that iron is the principal component part of all the masses of this kind hitherto discovered; that it is found almost everywhere on the surface of the earth as a component part of many substances in the vegetable and animal kingdom; and that the effects of magnetism give us reason to conclude that there is a large provision of it in the interior parts of the earth. We may therefore conjecture that iron in general is the principal matter employed in the formation of new planetary bodies; and is still farther probable by this circumstance, that it is exclusively connected with the magnetic power, and also on account of their polarity may be necessary to these bodies. It is also probable, if the above theory be just, that other substances contained in such fallen masses, such as sulphur, siliceous earth, manganese, &c., may be peculiar, not to our globe alone, but may belong to the common materials employed in the formation of all planetary worlds."

This paper of Chladni's, it will be seen, dates from just before the beginning of the present century.

The subject was invested with a new interest in 1799, when the great Humboldt, who was then travelling in South America, saw an enormous quantity of shooting-stars covering the sky. In his long account of the shower in his "Personal Narrative," he states that, from the beginning of the phenomenon, there was not a space in the firmament equal in extent to three diameters of the moon that was not filled at every instant with bolides and falling stars; while he was locally informed that during a previous display in 1766 the inhabitants of Cumana had beheld the neighbouring volcano, Cayamba, veiled for an hour by a similar display.

In the next display, observed in the year 1833, 240,000 meteors were computed by Arago to have been visible above the horizon of Boston on the morning of November 13; while Mr. Baxendell, who observed the shower from the west coast of Mexico, states that "the number of meteors seen at once often equalled the apparent number of the fixed stars seen at a glance."

Olmsted, when he had witnessed the shower of 1833 (a shower heralded and followed by less brilliant displays in 1831–32 and 1834-35-36), and when, moreover, he had compared the phenomena with those recorded by Humboldt and Bonpland in 1799, announced the view which has since been so brilliantly confirmed—that the appearances are due to the passage of the earth through a *storm*, so to speak, of planetary bodies.

This was the first blow given to *le vide planétaire.* Space, instead of being empty, was full of bodies, some of them being congregated into rings, each body composing the ring revolving like a planet round the sun. In fact, these rings may be compared to *tangible orbits;* indeed, they almost realize the schoolboy's idea of an orbit, as a considerable part of the path is occupied by a string of little planets, while in the case of our earth's orbit, for instance, each point of the path is occupied in succession only.

Still Olmsted did not accept the view that the falling stars were of the same nature as meteorites.

Olmsted also noted that, however numerous the falling stars might be, or in whatever direction they appeared, or whatever the apparent lengths of their paths, the lines of motion of these paths, retraced along the sky, nearly all found a common focus of emanation or visual crater of projection among the fixed stars. This has since been called the radiant point.

The most salient fact, noticed even by those who did not see its significance, during the subsequent display in 1866, was that all the meteors seemed to come from the same region of the sky. Among all those seen by myself from 11 p.m. on Tuesday till 2 a.m. on Wednesday morning, *two only* were exceptions to the general direction. In fact, there was a region in which the

[1] Continued from p. 533.
[2] "Opera," ed. Fritsch, vol. vi. p. 157.
[3] "Ueber den Ursprung der von Pallas gefundenen Eisenmassen," p. 24.
[4] His paper on the Pallas iron is abstracted in *Phil. Mag.*, Tillock's Series, vol. ii., 1798.
[5] See *Phil. Mag.*, Tillock, vol. ii. p. 225, *et seq.*

meteors appeared trainless, and shone out for a moment like so many stars, because they were directly approaching us. Near this spot they were so numerous, and all so foreshortened, and for the most part faint, that the sky at times put on almost a phosphorescent appearance. As the eye travelled from this region the trains became longer, those being longest as a rule which first made their appearance overhead, or which rended westward. Now, if the paths of all had been projected backwards, they would have all intersected in one region, and that region the one in which the most foreshortened ones were seen.

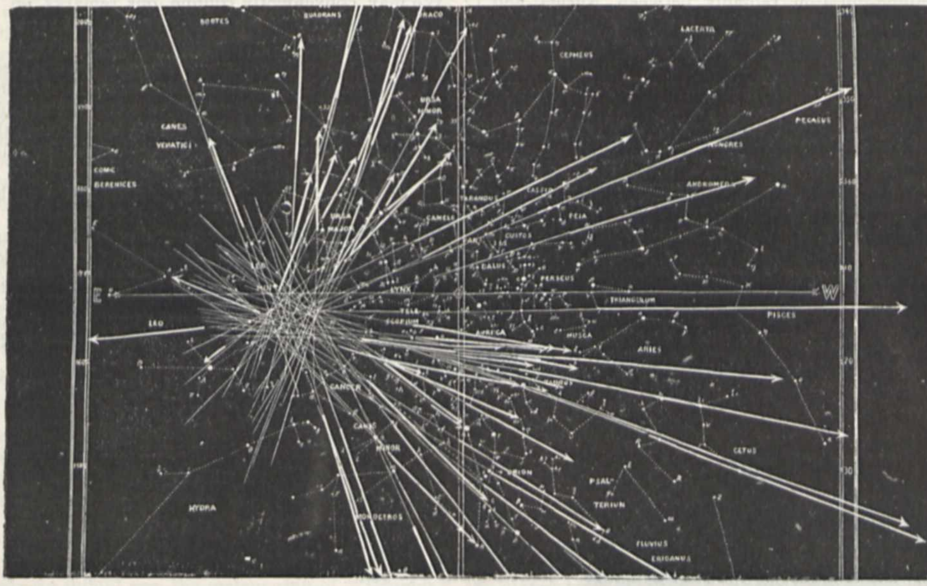


FIG. 7.—The radiant point of the November meteors.

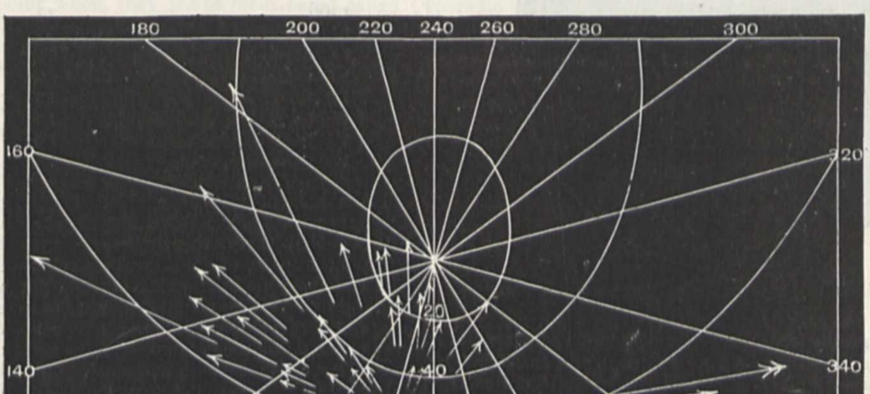


FIG. 8.—Radiant point of long duration (October–November), Denning.

So decidedly did this fact come out that there were moments in which the meteors belted the sky like the meridians on a terrestrial globe, the pole of the globe being represented by a point in the constellation Leo. In fact, they all seemed to *radiate* from that point, and *radiant point*, as we have seen, is precisely the name given to it by astronomers. *Vanishing point*, if the bull were permissible, is a term which would represent the fact rather than the appearance which is an effect of perspective; and hence we gather that the paths of the meteors are parallel, or nearly so, and that they come therefore from one point in the sky. The point from which they proceed in the case of the swarm we are now considering lies in the constellation Leo, situated in longitude 142° and latitude 8° 30′ N., according to Prof. Newton.

The radiants are generally of short duration, but Mr. Denning has shown that there are cases in which falling stars emanate from the same part of the sky for long periods of time.

One of these long-duration radiants between Auriga and Taurus is shown in the accompanying illustration (Fig. 8).

The next point, first brought to light by Olmsted, was that during a display the radiant point moves with the stars across the heavens. This is another strong argument in favour of the cosmical theory.

Meteors which are singly and occasionally observed, as we have seen, are called sporadic meteors, but in addition to these, which we may reckon to see every night, there are at certain times of the year very well known falls; so well known that we can say at once that on the 10th or 11th of next August more falling stars will be seen than are ordinarily visible. These are termed systematic meteors, and those to which we have just referred as appearing in November are of this class.

From 1833 to 1863 evidence was rapidly accumulated indicating that a very large proportion of the shooting-stars observed were not sporadic, but really systematic—that is to say, that at certain periods of the year meteors might be expected to diverge from their appearance in a particular part of the sky, and in greater numbers from that part than from elsewhere.

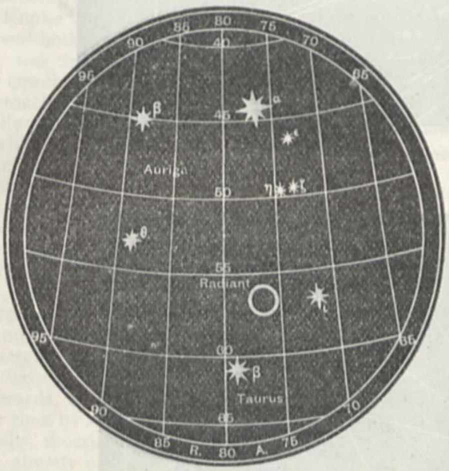


Fig. 9.—Position of the long-duration radiant among the stars.

During these years a considerable number of radiant points had been made out, and therefore the existence of a considerable number of streams or swarms had been suggested if not established. In 1863, Prof. H. A. Newton used these facts to strengthen the cosmical hypothesis.

The observations of Humboldt, modern observations, so to speak, were repeated, as we have seen, in 1833, on the same day (or one day later) of the same month on which Humboldt had made his observation in 1799, and again one day later in 1866 there was a recurrence of the same thing. Now these dates are separated by an equal interval of thirty-three years. The idea of periodicity was therefore suggested both for this and other displays, and gave rise to so great an interest in this question that an inquiry was set afoot as to whether falls had been seen before at previous intervals of thirty-three years, or whether it was a new thing seen first by Humboldt in 1799, or possibly by the Cumanese in 1766.

Prof. Newton took up the inquiry, and was soon able to show that the various chronicles of star-showers from the very earliest times, when properly discussed, indicated that the streams suggested by the observations since 1833 had really at variously-recurrent intervals since the beginning of astronomical observation given indications of their existence.[1] He especially indicated such cases of constant recurrences of showers in April, August, November, and December.

[1] *Silliman's Journal*, vol. xxxvi. p. 146, 1863.

The discussion of the dates of these showers in the early records showed a constant slow change of date in one direction or the other. This obviously demonstrated that the showers were independent of the tropical year—that is to say, of the earth's motion round the sun; and it is difficult to understand how a more definite proof of their cosmical origin could be afforded.

We may conveniently confine our remarks on this point to the inquiries relating to the "Leonid" swarm of meteorites which gives rise to the November display.

Newton and others found that we possess records, dating from A.D. 902, showing that about every thirty-three years since that time the heavens have been hung with gold. The Arab historian, Abu-l'Abbas ad-Dimashkî, chronicled the November star-shower of the year 1202 of our era in the following words, the while Chinese astronomers carefully watched the constellations in which the meteors appeared and vanished from the sight:—

"In the year 599, on the last day of Muharram, stars shot hither and thither, and flew one against another like a swarm of locusts; this phenomenon lasted until daybreak; people were thrown into consternation, and made importunate supplications to God the most High; there was never the like seen except on the coming out of the messenger of God—on whom be benediction and peace."

This table for the November display, from Prof. Newton, shows what the result of searching the old records was:—

Epochs of November Star-Showers.

Year.	Day on which the star-shower was seen.	Paris dates and hours.[1] d.	h.
902	October 13	12	17
{ 931	16	14	10
{ 934	14	13	17
1002	15	14	10
1101	17	16	17
1202	19	18	14
1366	23	22	17
1533	25	24	14
1602	28 [2]	27	10
1698	November 9	8	17
1799	12	11	21
{ 1832	13	12	16
{ 1833	13	12	22
1863-68	14	13	14

These ancient records enabled Prof. Newton to place the planetary nature of the November ring beyond all doubt.

It is evident that if this ring crosses our orbit in a certain definite point in space, our earth will always traverse it when it occupies the same definite point of its orbit with regard to the stars, provided the ring does not change its place. But our ordinary year, called the tropical year, is affected by the precession of the equinoxes, as it is measured from equinox to equinox, so that we do not measure it by the stars, but by an empirical point called the first point of the sign Aries, which is actually at the present moment in the constellation Pisces. If we refer the recorded star-showers to the sidereal year, or a fixed equinox, we should find an almost absolute identity in the dates of their appearance if there were no perturbation, but we shall see subsequently that there is perturbation, and this is a final demonstration of cosmical origin.

If there is a swarm of meteorites falling in any particular direction towards the plane of the ecliptic these meteorites will take little account of the precession of the equinoxes or the tropical year; the earth must take the meteorites as she finds them. The one great jump in the table was due to the alteration of the calendar, as there was a difference of twelve days between the old and new reckoning. Prof. Newton, Prof. Adams, and others have given a complete demonstration that from the year 902 a swarm of meteorites has been encountered by the earth every thirty-three years or thereabouts, and nearly in the same part of her orbit round the sun.

By a study of the position and *lie* of the earth in her orbit we can see from what part of space these meteors, these more numerous swarms, come. Suppose, for instance, that at one

[1] H. A. Newton, *Bul. Ac. R. Belg.*, xvii. No. 6.
[2] In many countries the change from old to new style was made in this interval commencing from 1582 in Spain, Portugal, and Italy.

part of the earth's orbit there is a stream of meteorites plunging down nearly vertically towards the ecliptic; the earth in passing through them would receive the greatest number of blows on its exterior atmosphere on the hemisphere above the plane of the ecliptic at the time, while the other hemisphere would be entirely sheltered, so that the direction of the fall would be capable of demonstration by a consideration of the earth's direction and the relation of its surface to the plane of the ecliptic at the time.

The observations indicate that these bodies are moving towards the plane of the ecliptic, from its northern side, into that part of it through which the earth passes in her annual journey in November; they, in fact, are moving round the sun in an orbit inclined at a not very large angle—17°—to the plane of the earth's orbit.

Similarly, we might observe the August ring rising from one of its nodes, situated in the point of the earth's orbit occupied by our planet on August 10, not at a slight angle like the November ring, but at an angle of 79° or 80°.

It is important to make this point quite clear.

Let us conceive the sun and earth to be half immersed in an infinite ocean which will represent to us the plane of the ecliptic, and let us further for greater simplicity assume that the earth's motion round the sun (in a direction contrary to the hands of a watch) is performed in a circular path with the sun at the centre; let us, moreover, suppose the earth's path, or orbit, to be marked by buoys, remembering that astronomers define the position of a heavenly body in the plane by stating its *longitude*—that is, its angular distance, reckoning from right to left, from a particular start-point, as seen from the sun; and its *latitude*—that is, its angular height above the plane as seen from the same body.

Now, if it were possible to buoy various points of the earth's orbit in the plane of the ecliptic in the convenient manner before suggested, we should see the meteor-ring of "Leonids" meeting the waves of our hypothetical ocean, at a slight angle (17°), at the point of the earth's orbit occupied by our planet on November 14, the point where they pierce them being called the *node*. Where the other node lies, where the meteorites cross the plane again, we do not exactly know; we only know that they do not cross our orbit; if they did, another star-shower would occur in May.

Let us inquire into this point a little more closely. Let us, in imagination, connect the earth and sun by a straight line; at any moment the direction of the earth's motion will be at right angles to that line (or a tangent to its orbit); therefore, as longitudes are reckoned, as we have seen, from right to left, the motion will be directed to a point 90° of longitude behind the sun. The sun's longitude at noon on November 14 was 232°, within a few minutes; 90° from this gives us 142°, which, as we have seen, is precisely the longitude of the radiant point. This, then, is proof positive enough that in longitude at least the meteoric hail was fairly directed against, and as fairly met by, the earth.

But it will be asked, If the radiant point is situated in latitude 8° 30′, how comes it that the inclination of the ring is stated to be 17°? should it not rather be 8° 30′? To this question we may reply by another: How comes it that, when we are hurrying through a shower, we always incline an umbrella at a less angle with the ground than that formed by the falling rain? The answer is the same in both cases. In the case of the meteorites, if our motion in one direction differs little from theirs, they appear to us to fall at an angle which is also almost precisely half of their real one.

Similar ancient records relating to star-showers seen in March and April, and July and August, showed that the earth's longitude was always the same when they were observed, if it was referred to a *fixed equinox*. The constant longitude for the star-showers anciently recorded to have taken place in March–April corresponds to April 20·1d., 1850, and for a like number seen in July–August, August 9·0d., 1850.

Forms and dimensions of the orbit of the August meteors, all of them very steeply inclined to the ecliptic, were calculated among the many combined observations and determinations of heights of those meteors made at German Observatories to conclude their longitudes, in the years following the great November showers of 1832–33, by the German astronomer, Erman. *But an exact value of their velocity was still wanting*; and from an approximate measure of the velocity of the "Perseids," obtained from observations of a fine meteor of the shower in America on August 10, 1861, Prof. H. A. Newton found elements of the ring, concluding it to be not far from circular in form, and nearly perpendicular in its plane to the ecliptic.

It will be seen that the longitude for the showers recorded in October–November advances along the ecliptic from a fixed equinox with a uniform motion of 52″ per annum. Such a motion as this must be due to planetary perturbation, and hence we are in presence of cosmical phenomena.

It is to an American astronomer, Prof. Newton, that we owe the first investigation into the constitution of the November ring.[1] He first considered the question whether the ring is of uniform density, and whether it lies merely near our orbit; the variation in the brilliancy of the showers being caused by the action of the planets and moon on the earth and ring—the greatest perturbation of the earth being 9000 miles each way—sometimes throwing us into the ring, sometimes causing us to pass it without meeting it. He has shown, however, that the ring cannot be of uniform density throughout, but that, on the other hand, in one part of it there is a clustering together of the little bodies of which it is composed—a few stragglers being scattered along the rest of its circuit.

From other considerations he showed that the meteors revolve round the sun in a direction opposed to the earth's motion, the most probable time of revolution being, according to his first view, 354·621 days, our own being accomplished in 365·256 days. This is the same as saying that the annual motion of the group is $1 + \frac{1}{33 \cdot 25}$ revolutions. Consequently, the centre of the group is brought, on this view, into contact with the earth once in every 133 years, but the earth passes very near the centre four times in this interval.

On this view the orbit of the swarm would be nearly circular.

With regard to the rings generally, Prof. Newton made out in 1865[2] (1) that all the sporadic shooting-stars cannot belong to a narrow ring which has a diameter approaching in size that of the earth; and (2) that a large portion of the meteorites, when they meet the earth, are travelling faster than it, or else that the sporadic meteors form a series of radiants at some distance from the ecliptic, and hence come from a series of rings considerably inclined to the plane of the ecliptic.

Further, he pointed out that the distribution of the orbits of the meteorites must be one or other of the following:—

(1) They may form rings passing near the earth's orbit at many points along its circuit (sporadic meteors may be outliers of such a ring).

(2) They may form a disk in the plane of the ecliptic.

(3) They may be distributed at random like the orbits of comets.

J. NORMAN LOCKYER.

(To be continued.)

[1] *Silliman's Journal*, Nos. 111 and 112.

[2] *Ibid.*, vol. xxxix.

SCIENTIFIC SERIALS.

American Journal of Science, September.—Cambrian fossils from Mount Stephens, North-West Territory of Canada, by Charles D. Walcott. The fossils here studied were first discovered last year by Otto J. Klotz, and partly described by Dr. C. Romiger. A comparison with specimens from the Middle Cambrian Terrane of Central Nevada shows that the two faunas are identical, and that consequently the Mount Stephens remains should be referred to about the horizon of the upper portion of the Middle Cambrian system. Other discoveries near the Kicking Horse Pass on the Canadian Pacific Railway seem to show that this fauna extends all along the western side of the great Keweenawan continental area from Southern Nevada far into British America.—History of changes in the Mount Loa craters (continued), by James D. Dana. Here are studied the relations of Kilauea to Mount Loa, arguments being advanced to establish the independent origin of the former, contrary to the author's earlier views on the subject. But his old conclusion is confirmed that volcanoes are not safety-valves, but are rather indexes of danger, pointing out the parts of the earth's crust that are most subject to earthquakes. A contrast is also drawn between volcanoes of the Mount Loa and Vesuvius types, the discharges of the former being almost exclusively outflows, those of the latter upthrows of cinders combined with lava-streams.—On the formation of deposits of oxides of manganese, by F. P. Dunnington. The main object of this paper is to show that manganese sulphate has probably taken a very important part in the formation of deposits of manganese ore.—Maxwell's theory of the viscosity of solids and certain features of its physical verification, by Carl Barus. These researches tend to show that Maxwell's theory is a version of Williamson's theory of etherification and of Clausius's theory of electrolysis. The transition made is from unstable groupings of atoms to unstable groupings of molecules. But while preserving minutely all the essentials of Maxwell's argument, the experiments here described go one step further, showing that viscosity is a phenomenon evoked by certain changes of molecular structure, the inherent nature of which is ultimately chemical.—On the origin of primary quartz in basalt, by Joseph P. Iddings. Here are described certain specimens of basalt occurring in the vicinity of the Rio Grande Cañon, which exhibit a remarkable number of porphyritic grains of quartz. A theory is proposed to account for the possible origin of this porphyritic quartz.—Mineralogical notes, by Geo. F. Kunz. Here are studied some specimens of phenacite and quartz pseudomorphs from Maine, a variety of transparent oligoclase and a cyanite from North Carolina, an apatite from New York, and an aragonite pseudomorph from Arizona.—An appendix of 42 pages contains a complete list of the late Asa Gray's writings, chronologically arranged and disposed in three categories: (1) scientific works and articles, 1834-83; (2) botanical notices and book reviews, 1841-87; (3) biographical sketches, obituaries, &c., 1842-88.

SOCIETIES AND ACADEMIES.

LONDON.

Entomological Society, September 5.—Dr. D. Sharp, President, in the chair.—Dr. Sharp mentioned that he had received, through Prof. Newton, a collection of Coleoptera from St. Kilda, consisting of *Carabus catenulatus* (1), *Nebria brevicollis* (12), *N. gyllenhalii* (3), *Calathus cisteloides* (20), *Pristonychus terricola* (1), *Pterostichus nigrita* (71), *Pt. niger* (31), *Amara aulica* (4), *Ocypus olens* (1). The species being nearly all large Geodephaga, he thought probably that many other Coleoptera inhabited the island. He remarked that these specimens showed no signs of depauperation, but were scarcely distinguishable from ordinary English specimens.—Mr. South exhibited a melanic *Aplecta nebulosa* from Rotherham, bred with five others of ordinary form, and an albino of the same species from Devonshire; a very curious dark variety of *Plusia gamma*; two dark varieties of *Eubolia limitata* from Durham; *Dicrorhampha consortana* from North Devon.—Mr. Champion exhibited *Harpalus cupreus*, *Leptusa testacea*, and *Cathormiocerus maritimus* from Sandown, Isle of Wight.—Mr. Elisha exhibited the following Microlepidoptera: *Œneana atricapitana, turionana, juliana, derasana, capreana, pomonana*, taken off *Sorbus aucuparia; sodaliana, zephyrana, trigeminana*; also *Schiffermulleriella horridella, alpella, fuscoaurella, therinella*, and *semidecandrella*, on *Cerastium tetrandrum*.—Mr. Jacoby exhibited three boxes of Coleoptera, collected partly by Mr. Fruhstroffer, containing some rare *Cetoniadæ*, *Faussidæ*, &c.—Mr. E. Saunders exhibited *Amblytylus delicatus*, Perr., a new British bug, taken at Woking.—Mr. Jacoby mentioned that he had taken the larva of *Vanessa cardui* on a narrow white-leaved plant in his garden.—Mr. Enock mentioned that out of a batch of two males and six females of the Hessian Fly kept together, all six females had laid fertile eggs, so that each male must have impregnated more than one female.

PARIS.

Academy of Sciences, September 24.—M. Des Cloizeaux in the chair.—Generalization of a theorem of Gauss, by M. J. Bertrand. This theorem is thus expressed: Whatever be the attracting body, the mean value of the potential at the different points of a sphere is equal to the relative potential at the centre of the sphere. The demonstration supposes the sphere to be exterior to the attracting body, and the present paper deals with the theorem when this condition is not fulfilled, and it is shown that by substituting for the full sphere a spherical surface the theorem still holds good.—Complement to the theory of overfalls, by M. J. Boussinesq. Various applications are given to the theory established in the previous paper (*Comptes rendus*, September 17, p. 513) regarding the influence exercised on the discharge by the velocity of the current at the overfall.—Observations of Brooks's comet (August 7), and of Barnard's comet (September 2), made with the 0·38 m. equatorial at the Observatory of Bordeaux, by MM. G. Rayet and Courty. The observations for Brooks's comet are for the period from September 5-17, those for Barnard's comet from September 11-17.—On the physiological action of *Hedwigia balsamifera*, by MM. E. Gaucher, Combemale, and Marestang. This plant, which has been classified and described by Descourtilz ("Flore des Antilles," iii. p. 263), belongs to the family of the Terebinthaceæ, and grows in the West Indies. The experiments on guinea-pigs and rabbits here described show that the alcoholic extract from the bark of stem and root is highly toxic, a dose of 0·161 gramme proving fatal. The aqueous extract is less toxic than the alcoholic, but both produce rapid and considerable lowering of the temperature, paralysis, and convulsions, spreading progressively from the lower part of the marrow to the rachidian bulb.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Lessons in Elementary Physics, new edition: Balfour Stewart (Macmillan).—Ungdomsskrifter, Första Serien, Första Häftet: Carl von Linnés (Stockholm).—The Frog, 3rd edition: A. Milnes Marshall (Cornish, Manchester).—Primer of Micro-Petrology: W. Mawer (London).—Memory: F. W. Edridge-Green (Baillière).—Mathematischen Theorien der Planeten-Bewegungen: Dr. O. Dziobek (Barth, Leipzig).—Examples in Physics: D. E. Jones (Macmillan).—A Text-book of Physiology, 5th edition, Part 1: M. Foster (Macmillan).—The Centre of the Central Sea: J. N. Emra (Kegan Paul).—Johannes Kepler und der Tellurisch-Kosmische Magnetismus: Dr. S. Günther (Wien).—Synopsis of the Vertebrate Fauna of the Puerco Series: E. D. Cope (Philadelphia).—Morphologisches Jahrbuch, 14 Band, 2 Heft: C. Gegenbaur (Leipzig).—Zeitschrift für Wissenschaftliche Zoologie, xlvii. Band, 1 Heft (Leipzig).—Geological Record for 1880-84: Topley and Sherborn (Taylor and Francis).—The Calendar of the University College of Wales, Aberystwyth, 1888-89 (Cornish, Manchester).—The Analyst's Laboratory Companion: A. E. Johnson (Churchill).—Memoirs and Proceedings of the Manchester Literary and Philosophical Society, 4th series, vol. 1 (Manchester).—Photography for All: W. J. Harrison (Iliffe).—Ornamental Waterfowl: Hon. Rose Hubbard (Simpkin).—Jahrbuch der Meteorologischen Beobachtungen der Wetterwarte der Magdeburgischen Zeitung, Jarhgang vi. 1887 (Magdeburg).—Proceedings and Transactions of the Royal Society of Canada for the Year 1887, vol. v. (Dawson, Montreal).—Catalogue of Variable Stars: S. C. Chandler (Lynn, Mass.)—Report on the Condition of Growing Crops, &c., August (Washington).—La Zoologia de Colón: J. I. de Armas (Habana).—Vierteljahrs-Wetter-Rundschau, Band i. Heft 3 and 4 (Mittler, Berlin).—Journal of Morphology, vol. ii. No. 1 (Ginn, Boston).—Mind, October (Williams and Norgate).—Journal of Anatomy and Physiology, October (Williams and Norgate).—The Geological Magazine, October (Trübner).

CONTENTS.

PAGE
Determinants 537
Our Book Shelf:—
Dawson: "The Geological History of Plants" . . . 538
Letters to the Editor:—
Prophetic Germs.—**Prof. E. Ray Lankester, F.R.S.** 539
A Shadow and Halo.—**E. W. P.** 540
Sonorous Sands.—**A. R. Hunt** 540
The Report of the Krakatão Committee of the Royal Society. I. 540
The British Association:—
Section H—Anthropology.—Opening Address by **Lieut.-General Pitt-Rivers, D.C.L., F.R.S., F.G.S., F.S.A.**, President of the Section. II. (*With Maps*) 542
Section A.—Mathematical and Physical Science . . . 546
The International Geological Congress. II. 548
Remarks on some of the more Recent Publications dealing with the Crystalline Schists. By **Prof. J. Lehmann** 549
The Stratigraphical Succession of the Cambrian Faunas in North America. By **Prof. Chas. B. Walcott** 551
Notes 552
Our Astronomical Column:—
The Satellites of Mars 553
Total Lunar Eclipse of January 28 553
Photometric Observations of Asteroids 554
New Catalogue of Variable Stars 554
Minor Planet No. 275 554
Astronomical Phenomena for the Week 1888 October 7-13 554
Geographical Notes 555
Electrical Notes 555
Notes on Meteorites. IV. (*Illustrated.*) By **J. Norman Lockyer, F.R.S.** 556
Scientific Serials 559
Societies and Academies 560
Books, Pamphlets, and Serials Received 560