THURSDAY, JANUARY 3, 1884

THERMAL CHEMISTRY

Thermochemische Untersuchungen. Von Julius Thomsen, Dr. Phil. et Med., &c. Volumes I., II., and III. (Leipzig: Johann Ambrosius Barth, 1882-83.)

A PAPER was published in this journal a short time ago calling attention to "The Backward State of Chemistry in England" (vol. xxviii. p. 613); the writer regrets that so little attention is paid to the chemistry of the carbon compounds, and that so much time is spent in our chemical schools in elementary and routine instruction. In the second of these regrets I can thoroughly sympathise; our so-called students of chemistry are becoming mere machines which perform, and generally perform badly, mechanical processes known as qualitative and quantitative analyses. We hear complaints from physical laboratories that practical physics is taught in an unsystematic manner; we sometimes have comparisons drawn between the desultory methods of teaching pursued in these laboratories and the orderly and systematic courses of practical chemistry conducted in the work-rooms of the sister science. But I am afraid it is rather the chemist who is to be pitied: his method is too methodical; it seems to succeed because it neglects the really scientific aspects of chemistry. Chemistry is a great branch of science; but what is the so-called practical chemistry of the schools or the examination? It is but a weary round of dull repetition; it consists of obtaining black precipitates, and yellow precipitates, and colourless precipitates, precipitates which are soluble and those which are insoluble; it occupies itself with filtering and washing, and drying, and burning, and weighing; it has little or no connection with the problems which belong to the science of chemistry. But when the author of the article to which reference has been made attributes the backward state of chemistry in England to the comparatively small amount of attention which is given to organic chemistry, I find myself unable to agree with him. I think we are apt to be dazzled by such things as the synthesis of indigo, or the artificial manufacture of alizarin: we forget to inquire whether the study of organic chemistry has in recent years added any great general principle to chemical science. The conception of the valency of elementary atoms is certainly an outcome of the study of the carbon compounds, or rather of the application of the atomic theory to this study; but have we not of late made too much of this conception? has it not rather stopped than aided inquiry? is it not time we had given up our "bonds," our "units of affinity," which are chiefly remarkable as being changeable almost at pleasure? Organic chemistry, as pursued in the German laboratories, it seems to me, has almost if not quite entered on the same path as that which has led qualitative and quantitative analysis to so sad a fall: it is in danger of ceasing to be a branch of science and of becoming an art of manufacture. Any student who goes through the course of preparation of organic compounds, systematised so well in the laboratories of the German universities and elsewhere, is ready to manufacture new compounds by the score; the difficulty consists in not making such compounds. There are whispers abroad that he who is not in the trade is regarded by the German professors as "no chemist."

I think the evil lies deeper: we are so anxious to act that we have no time to think. The chemist may gain a kind of reputation by making new compounds; the process requires no thought, no scientific training, no originality. It has also something to be said in its favour. Nature is so vast that we can scarcely hope to gain any accurate knowledge save by attacking the problems in detail. In chemistry, as in other branches of science, we must be content to gain "a series of small victories" over nature. But in fighting nature in detail we are apt to lose sight of general principles by the help of which alone can empiricism become science. I think that in chemistry, and more especially perhaps in organic chemistry, we are specialising too much: we are trying to solve large and complex problems by a series of small attacks all delivered from the same point. What then is the remedy? I would answer: Vary the points of attack; remember that the victory is to be gained only by boldness, and that it is emphatically worth gaining. Do not let chemistry remain the battlefield of the Philistines, but enliven it with the true spirit of science, with that spirit which will not believe that the universe is a "rubbish-heap of confused particulars," but will rather regard it as a vast organism in which while "everything is distinct yet [is] nothing defined into absolute independent singleness."

That the points from which the problems of chemistry may be attacked are many is witnessed by the book before us. Why is there no handbook of thermal chemistry in English? Will not some one at least translate Naumann's "Handbuch"? M. Thomsen has for years been known as one of the two great workers in the field of thermal chemistry; his contributions to this branch of science have been numerous and important; we cannot be too thankful that he has gathered these contributions together, and arranged and digested them in this series of volumes, which must remain as the groundwork of the science. Three volumes have appeared, and a fourth (to treat of organic compounds) is promised. Let me try to give some account of one or two points in Thomsen's work.

The notation of thermal chemistry is simple: Let r = the thermal value (stated in gram-units) of a chemical change; if the change consist in the formation of a definite quantity of a compound $X_a Y_b Z_c$—made up of a parts of X, b parts of Y, and c parts of Z—then

$$r = [X^a, Y^b, Z^c] + ;$$ [1]

if the same compound is produced in presence of a large quantity of water, then

$$r = [X^a, Y^b, Z^c, Aq] + ;$$

if the same substance already formed is dissolved in an unlimited quantity of water, then

$$r = [X^a Y^b Z^c, Aq] +.$$

The general expressions for the production and decomposition of a compound $X_a Y_b$ are

$$(1)\ X_a + Y_b = X_a Y_b + (X^a, Y^b);$$

and

$$(2)\ X_a Y_b = X_a + Y_b - (X^a, Y^b).$$

[1] Thomsen always writes the indices above the elementary symbols when these symbols occur in thermal equations

If the compounds X Y and Z V react to produce X Z and Y V then

$$r = [X, Z] + [Y, V] - [X, Y] - [Z, V].$$

These equations illustrate the methods by which the thermal value of a chemical change can be indirectly calculated. The total loss of energy by a chemical system in passing from a definite initial to a definite final state is independent of the intermediate states; assuming, as we may do for most purposes, that the total loss of energy is measured by the quantity of heat evolved, it follows that the total thermal change accompanying a chemical change depends only on the initial and final states of the system. Hence, if we have series of reactions beginning with the same materials in the same condition, and ending with the same products in the same condition, and if all the thermal changes in one series may be measured, and all except one in the other series may be measured, it follows that we can calculate the thermal value of the unknown member of the second series of changes. Thus, it is required to determine the thermal value of the synthesis of 46 grams of formic acid (CH_2O_2). Twelve grams of carbon, 2 of hydrogen, and 48 of oxygen combine to produce 44 grams of carbon dioxide and 18 grams of water: but the same quantities of the same materials might theoretically be combined to produce 46 grams of formic acid, and then from this, 44 grams of carbon dioxide + 18 grams of water would be produced. The following are the thermal values of the various parts of these two series of changes:—

$$[C, O^2] = 96{,}960 \text{ gram-units} + ; [H^2, O] = 68{,}360 + ;$$
$$[CH^2O^2, O] = 65{,}900 + ;$$

but

$$[C, O^2] + [H^2, O] = [C, H^2, O^2] + [CH^2O^2, O] = 165{,}320 +$$
$$\therefore [C, H^2, O^2] = [C, O^2] + [H^2, O] - [CH^2O^2, O] = 99{,}420 +.$$

Such calculations sometimes become very complex; corrections must frequently be introduced for quantities of heat evolved or absorbed during purely physical changes which form integral parts of the cycle of chemical change under investigation.

The thermal study and comparison of classes of chemical changes leads to the conclusion that a chemical change which is accompanied by considerable loss of energy to the changing system will generally occur, unless prevented by the action of forces external to the system. This generalisation, vague though it be, helps to explain many classes of chemical reactions, *e.g.* the action of concentrated and dilute solutions of hydriodic acid on sulphur, and on many hydroxyl-containing carbon compounds; and the action of sulphuretted hydrogen in precipitating certain metallic sulphides in the presence of acid, and others only form alkaline liquids.

Thomsen has devoted much time and care to the thermal investigation of the mutual actions of acids and bases: the greater part of his first volume is devoted to this inquiry. The "heat of neutralisation of an acid by a base" is defined as the number of gram-units of heat evolved on mixing equivalent quantities in grams of the acid and base in dilute aqueous solution, the products of the action being also soluble in water. Thomsen employs a solution of 2 NaOH (grams) in about 200 H_2O (grams) as the standard base: he measures the thermal values of the following reactions:—

[2NaOH Aq, 2HX Aq]	in the case of a	monobasic	acid.
[2NaOH Aq, H_2X Aq]	,, ,,	dibasic	,,
[2NaOH Aq, $\frac{2}{3}H_3X$ Aq]	,, ,,	tribasic	,,
[2NaOH Aq, $\frac{1}{2}H_4X$ Aq]	,, ,,	tetrabasic	,,

The commoner acids may be broadly divided into four groups, according to the values of the "heats of neutralisation." This value is for Group I. about 20,000 gram-units; II., about 25,000; III., about 27,000; and IV., about 30,000 gram-units. The study of heats of neutralisation has led Thomsen to the conception of the *avidity* of an acid, *i.e.* the striving of one acid to displace another from its combination with a base. Thus, when equivalent quantities of NaOH, HNO_3, and H_2SO_4 are mixed in dilute aqueous solutions, two-thirds of the NaOH combines with the HNO_3, and one third with the H_2SO_4; the *avidity* of HNO_3 for NaOH is said to be twice as great as that of H_2SO_4 for the same base. HNO_3 in aqueous solution is therefore a *stronger acid* than H_2SO_4.

Measurements of the heats of neutralisation of monobasic, dibasic, *n*-basic acids has led Thomsen to classify some of these acids in ways different from those generally adopted in the text-books. His results as regards dibasic and tribasic acids may be thus summarised:—

Dibasic Acids

Group I. Typical formula RH_2 *e.g.* $SiF_6 . H_2$
,, II. ,, ,, $R(OH)_2$ *e.g.* $SO_2(OH)_2$
,, III. ,, ,, $R(OH)H$. *e.g.* $SO_2(OH)H$.

Tribasic Acids

Group II. Typical formula $R(OH_3)_3$. *e.g.* $C_4H_5O_4(OH)_3$.
,, III. ,, ,, $HR(OH)H$. *e.g.* $HPO_2(OH)H$.

These examples will serve to show the suggestiveness of the results of thermal chemistry. Thomsen's three volumes teem with suggestions: his results throw light on such questions as are connoted by the expressions allotropy, molecular compounds, classification of elements and compounds, isomerism, and affinity.

It is in examining the subject of chemical affinity from the point of view of thermal chemistry that one becomes aware of the complexity of the problems included under this expression.

From the following numbers,

$$[H, Cl] = 22{,}000 + ; [H, Br] = 8440 + ; [H, I] = 6050 - ;$$

it might be concluded that the affinity of chlorine for hydrogen is much greater than that of bromine, and that the affinity of iodine for hydrogen is much less than that of bromine. But these thermal equations are not comparable; at ordinary temperatures chlorine is a gas, bromine a liquid, and iodine a solid; hence, on this ground alone, no precise conclusions can be drawn from the above data regarding the relative affinities for hydrogen of the three halogen elements. Again, looking at the numbers,

$$[C, O] = 28{,}600 + ; [C, O^2] = 97{,}000 +,$$

it might be said that when oxygen combines with carbon in quantities of 16 grams at a time, the union of the second parcel of 16 grams is attended with evolution of much more heat than accompanies the addition of the first parcel of 16 grams. But measurement of the heat of oxidation of carbon monoxide, [CO, O] = 68,400 +, at once negatives this conclusion, and rather points to the number 68,400 × 2 136,800 as representing the thermal value of the transaction, $C + O_2 = CO_2$, where C represents 12 grams of gaseous carbon.

In the ordinary chemical notation almost every chemical change is represented as much simpler than it really is; no indication is given of the fact that in most cases an *excess* of one or other of the reacting substances must be used. Thus the reaction usually written

$$AgCl + HI \text{ (gas)} = AgI + HCl$$

would more correctly represent the distribution of the reacting bodies were it written

$$xAgCl + x'HI = xAgI + x'HCl + (x-1)AgCl + (x-1)HI.$$

If it is assumed that in the thermal study of a chemical reaction allowance is made for all the purely physical changes which accompany the chemical change, for the influence of the masses of the reacting substances and for the possible formation and decomposition of molecular groups during the reaction, there yet remains the consideration that heat is lost or gained to the system in the decompositions and formations of elementary molecules, which decompositions and recompositions may form parts of the entire change under examination. Thus, take the comparatively simple reaction

$$2H_2O + Cl_4 = 4HCl + O_2;$$

expanded thermally we have

$$r = 4[H, Cl] + [O, O] - 2[H^2, O] + 2[Cl, Cl].$$

Even the apparently most simple case, the union of two elements, is more complex than at first sight appears. $[H^2, Cl^2] = 44{,}000 +$ simply tells that 2 grams of hydrogen combine with 71 grams of chlorine, and that 44,000 gram-units of heat are evolved. But if we wish to apply these data to questions concerning the affinity of chlorine for hydrogen, we must remember that affinity is the name given to the stress between atoms when regarded from the point of view of one kind of the reacting atoms. Hence, remembering that the molecules of hydrogen and chlorine are diatomic, we must amplify the equation $[H^2, Cl^2] = 44{,}000 +$, and write it thus—

$$r = 2[H, Cl] - [H, H] - [Cl, Cl] = 44{,}000 +;$$

but the value to be assigned to two of the terms in this equation are unknown. Until we are able to assign approximate values to the thermal changes accompanying the decompositions of elementary molecules and the combinations of elementary atoms, we shall not be in a position to apply thermal data to the subject of affinity, provided, that is, we use this term in its most precise meaning.

The statement of Berthelot in the "Essai de Mécanique Chimique," that the quantity of heat evolved in a reaction measures the sum of the physical and chemical changes which occur in that reaction, and furnishes a measure of the chemical affinities, is evidently untrue if we assign any precise meaning to the term "affinity." But if we use this term in a wide sense as summing up the various actions and reactions (other than those which are purely physical) which together constitute any given chemical change, then we may perhaps say that thermal measurements of comparable reactions are also relative measurements of the affinities of the reacting substances. It is in some such sense as this that the term "affinity" is used by Thomsen in his thermal researches on the relative affinities of the non-metallic elements (vol. ii.).

It is worthy of remark that Thomsen's arrangement of the commoner acids in order of relative affinities agrees very well with that given by Ostwald as the result of his investigations conducted on altogether different lines and by very different methods.

If thermal measurements of chemical changes really represent the sums of various partial changes, some of which have a positive and others a negative value, then it becomes doubtful whether any practical result is to be looked for from the application of Berthelot's *law of maximum work*, which runs thus:—

"Every chemical change, accomplished without the addition of energy from without the system, tends to the formation of that body or system of bodies the production of which is accompanied by evolution of the maximum quantity of heat."

Thomsen puts this "law" in a somewhat different form: he says, "Every simple or complex reaction of a purely chemical kind is accompanied by evolution of heat." Thomsen explains that by a purely chemical process he means one which is accomplished without addition of energy from sources external to the system, and consists in the "striving of atoms towards more stable equilibrium." But there are, I think, two principal objections to this statement. Actions "of a purely chemical kind," as thus defined, do not actually occur except as parts of cycles of reactions wherein are included changes not of a "purely chemical kind." And, secondly, we have at present no means of measuring the thermal values of those purely chemical actions—*i.e.* on Thomsen's view, atomic actions—but are obliged to include their values in the total value assigned to the complete cycle of operations which we term a chemical reaction.

Thomsen has it is true attempted to assign thermal values to the decomposition of the molecule of carbon into atoms and the recombination of atoms of carbon to form molecules. The pages of NATURE are scarcely suitable for a detailed discussion of Thomsen's methods; it seems to me, and I think to some others who have tried to follow Thomsen's arguments, both in the second volume of his "Untersuchungen" and also in the original papers in the *Berichte* and elsewhere, that these arguments really bristle with assumptions, and that a comparison of the results deduced by Thomsen with the actual calorimetric measurements obtained by himself and others is sufficient to throw grave doubt on the validity of those assumptions on which his arguments are based. One general result which appears to me to follow from Thomsen's investigation is that the time has come when we may with great advantage give up such expressions as "the carbon atom has four bonds," "such or such atoms are held by double links," and indeed the whole of that unscientific pseudo-dynamical nomenclature which has grown up around the vague and indefinable conception of *atomic bonds*.

There are many other points of interest in Thomsen's "Untersuchungen"; but I have said enough I trust to show the importance and the remarkable suggestiveness of these volumes; and also to establish the statement that the great advances of the future in chemistry are to be looked for, not so much in the domain of organic chemistry as in the application of the methods and generalisations of the science of matter and motion to the problems which we call chemical.

M. M. PATTISON MUIR

A SCIENTIFIC CATALOGUE

Bernard Quaritch's General Catalogue. Part II. Natural History and Science. Part III. Periodicals, Journals, and Transactions. (London: 1881-83.)

IN few instances that a political economist could hold up as an example is the function of the merchant in the processes of supply and demand so clearly and simply displayed as in that of Mr. Bernard Quaritch, the wealthy merchant in the book trade. He is especially a merchant-man seeking goodly pearls, whose great qualification must be that he knows the exact demand for, and the exact scarcity of, what is to be bought and sold. His catalogue does not aim at completeness as did the one which we noticed lately. Scarcely more than one-tenth of the titles carefully entered in Mr. Friedländer's lists are to be found here; but these make a collection, and a very large one, of books brought together by "natural" selection with the same good results in this case of intelligent working, as in the more automatic world around us. Many eminent men in various branches of science have first selected books bearing upon their own subjects, and then, on the dispersion of such libraries, Mr. Quaritch selects those works which have a higher value through their own superior merit, or the often doubtful though highly-prized recommendation of rarity. Accordingly Mr. Quaritch's catalogue is considerably like the sum total of British legislation. Each item of it was the supply of an existing want according to the best light of the time of its production. While circumstances, however, have changed and fresh laws have been devised to meet the changed circumstances, old laws have remained upon the statute book, and the existing code contains at the same time both inconsistent repetitions and grave deficiencies, and lacks both symmetry and completeness. While the catalogue of Mr. Friedländer shows the German love of both these good qualities and the scientific tastes of the compiler, that of Mr. Quaritch does not profess to be complete in any sense; it is a list of an immense stock of books brought together, as their former possessors ceased to require them, by a shrewd man of business who knew their market value. Hence in examining these bound up volumes which contain the many rich prizes of scientific literature constituting Part II. and Part III. of a new "General Catalogue," one is not surprised to find that a book like Agassiz's "Nomenclator Zoologicus" is to be found in four different places in one of them; that five copies of Owen's "Odontography" are offered, and a variety of copies of many others.

In Friedländer's catalogue we had to complain of too much classifying; not because classification is not of extreme value as a ready guide to the contents of a catalogue or library, but because many books refuse to fall under one head only, however discreet may be the arrangement. Mr. Bernard Quaritch's catalogue is just the reverse. In these volumes there is no attempt at either alphabetical or subject-divisions of the whole collection; different divisions are lists of books purchased at particular sales. A concise index makes up perhaps in the best way for this utter confusion of subjects. The table of contents, to which one would look first in trying to understand such a catalogue, is not printed in a way to clearly express the arrangement of those titles which *are* classified in subjects. A list of thirteen natural history headings follows "Egypt and North Africa" in exactly the same way as a nearly similar list of fourteen follows "The British Isles," but the former has nothing to do with Egypt as the latter had to do with the British Isles.

But Mr. Quaritch is the great connoisseur in a different class of books from the works which draw our attention in his catalogue. This class it would hardly come within our province to notice, were it not for the evidence given here, on the one hand, that costly books are purchased now as much as of old by the "patron" of literature, and on the other, that scientific works of original value and present scarcity are bought up by mere book collectors or bibliolaters, who would in many cases fret while one of their precious volumes was being turned over for consultation, lest it should end in a crack in its beautiful binding! Mr. Quaritch labours abundantly, and not without love, we think, for these purchasers. Here are a few of the feminine pomps and vanities with which he tickles the ears of bibliomaniacs:—"Grolier binding," "variegated leathers," "gold scroll tooling," "purple morocco super extra," "veau fauve," "veau marbré," "arms and cypher of ———," "vellum fly-leaves," "large paper," "tall copy," "magnificent specimen of bibliopegistic skill." Here is a titbit:—"First Aldine edition, very large, fine copy, in blue morocco, gold tooling, silk lining, vellum fly-leaves, gilt gaufré edges, by Bozerian."

A distinguishing feature in Mr. Quaritch's catalogues are the valuable notes appended to nearly all the most important of the works he offers. These notes as to the scarcity, completeness, market value, and often the history of the book testify to both the extent of his business and the minute accuracy of his knowledge of it. They are a mine of valuable information to any one whose business is in books, either commercially or as a librarian intrusted with the care and also the completion of important collections. In few cases will a book only professing to be a stock-list itself command a price in the market. Mr. Quaritch's catalogues command a high price, and the new edition of his general one, of which seven parts are now out, and which will probably not be completed for another year, if it should be the last which our veteran publishes, will doubtless remain for some time to come a standard work upon literature.

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. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Elevation and Subsidence

FOR several months past articles and letters have appeared in NATURE on the subject of subsidence and elevation of the earth's crust by addition and removal of weight. In this connection also much has been said in regard to the history of the idea. I wish therefore to draw attention to the fact that in 1859 I read a paper before the American Association for the Advancement of Science on the subject of the "Formation of Continents and Ocean Basins," in which, after giving the views of Herschell and Airy, I bring out this idea very prominently, and illustrate it by many diagrams. An abstract of this paper, by Sterry Hunt, was published in the *Canadian Naturalist*, vol. iv., 1859, p. 293, and reference to it will be found in the "Royal Society Catalogue," vol. iii. p. 919.

A very brief outline of the paper is as follows:—I make two assumptions: (1) an internal liquid with floating crust; (2) the crust of continental areas more conductive and therefore cooling and thickening more rapidly than that of oceanic areas.

It is evident that under these assumptions inequalities would commence first on the under surface of the crust by additions there, making convexities beneath the continental and concavities beneath the oceanic areas. But by *flotation* these inequalities on the under side next the liquid would be reproduced on the upper side next the atmosphere, and by this means alone continents would grow continually higher, and ocean beds deeper. Now add to these erosion. By cutting down continents and filling up the seas erosion would tend constantly to destroy these inequalities, while flotation would tend as constantly to reproduce them. Thus according to this view the continents rise partly by additions beneath and partly by removal above, and similarly the ocean beds sink partly by increased concavity beneath and partly by additions above. But evidently if unequal thickening should stop, flotation could only *partly* restore the inequalities destroyed by erosion.

Except the abstract above referred to, the paper was never published, and in February, 1865, it was destroyed, along with much else, by Sherman's army. My reason for not publishing more fully was that I soon became dissatisfied with it; for about that time the views of Hopkins and Pratt on the solidity of the earth began to attract attention, and I became convinced that dynamical geology must be reconstructed on a basis of a solid earth. But now that the idea of a sub-crust liquid or semi-liquid layer is becoming prominent (a condition which would not probably interfere with the substantial solidity of the earth in its astronomical relations), it seemed to me important that this long forgotten paper should be brought forward merely as a part of the history of the subject.

Now a few words on the subject of the communications referred to in the beginning of this letter. It seems to me that some of your correspondents have gone too far in regarding unloading by erosion as a cause of elevation. Evidently there must be some other and more fundamental cause, or erosion could not act. Evidently erosion can only partly restore an elevation produced by some other cause. Erosion is primarily an effect of elevation, only in this as in so many other cases the effect may react as a cause, to maintain the elevation. For example, the Colorado plateau region has been raised since Cretaceous times about 20,000 feet, but the maximum general erosion has been only about 12,000 feet. The erosion has been, therefore, the consequence, not the cause, of elevation, for it is impossible that the cause should lie so far behind the effect. I give this one example because it is on so large a scale, but every mountain range furnishes an example of great erosion as an effect of elevation produced by other causes. That loading and unloading the crust is a cause of subsidence and elevation there is little doubt, but that there are other and far more important causes is certain.

Berkeley, Cal., December 3 JOSEPH LECONTE

Red-deer Horns

IN continuation of my remarks on the eating of shed deer-horns by other deer, I have to add that six shed horns in various stages of erosion have been sent to me from Sutherlandshire. They each bear well defined teeth-marks on the gnawed portions, and this leaves little if any doubt that the popular belief that the horns are eaten by deer is founded on fact. The accompanying interesting letter from Mr. James Inglis, which gives the evidence of two experienced stalkers, both most intelligent and reliable men, is further confirmation of a curious though no doubt very natural habit of the deer, which finds in the lime-salts of the horn a necessary element of nutrition. You will observe that Inglis believes the deer use the molars in eating the bone, and this seems probable enough, as they apparently always begin at the points and eat towards the beam and burr, a method of proceeding by which they can bring portions of the horn within the action of the molars. J. FAYRER

December 27, 1883

". . . I send a few red-deer horns that have been partially gnawed by deer in the forest. I asked the stalkers to keep a look out and see if they could find any deer eating horns, and am glad to say that they have been able to put the matter beyond all doubt.

"Donald McRae saw with his glass a stag, in Dunrobin Glen, eating a horn; he went to the place where he saw him eating it, and found it partially eaten. I send it with the others. You will find a ticket on it to distinguish it from the rest.

"Duncan McPherson saw with his glass a hind, last week, eating a horn also; he did not find the horn, but he saw her (the hind), quite plainly, with it in her mouth, gnawing away at it near the point.

"Deer have no incisors in the upper jaw, but they have grinders or molars in both upper and lower jaws, formidable enough to eat any horn, and I have no doubt that it is with their molars that the horns are eaten.

"A shepherd in the parish of Lairg has a cow that eats all the bones she can find, and goes miles for them, and eats them up, shank bones and all; ribs are eaten easily, and seem to give no trouble whatever. "JAMES INGLIS

"December 24, 1883"

On the Absence of Earthworms from the Prairies of the Canadian North-West

NOT by any means the least remarkable of the very notable series of works which Mr. Darwin has given to the world is that which came last from his pen but a short time previous to his lamented death. Dealing, as it does, with effects which, when looked at in the detail, are exceedingly small and insignificant, but, when viewed in the aggregate, are shown to be of surprising importance, the "Vegetable Mould and Earthworms" must certainly rank as a most strikingly interesting work.

It is not my desire to call in question the conclusions at which Mr. Darwin has arrived with regard to the action of earthworms in cultivating the soil, but I wish to point out that in one extensive portion of the earth's surface, to which much attention has of late been directed on account of its agricultural capabilities, earthworms do not exist. I refer to the vast region commonly known as Manitoba and North-West Territories. My friend, Mr. E. E. T. Seton, of Carberry, Manitoba, was the first to point out to me that this enormous country must be regarded as forming an exception to Mr. Darwin's generalisations, on account of the total absence from it of every kind of earthworm, and, having lately returned from a visit to those regions, I can add my testimony to his in this particular, as well as in the matter of the amazing, innate fertility of the soil, which has been the wonder and remark of all travellers for years past, but which, in this case, obviously cannot be attributed to the action of worms, since these do not exist there. In addition to my own observations, I have the testimony of numbers of intelligent settlers, most of whom had been several years in the country, but all of whom unhesitatingly assured me that such a thing as an earthworm was unknown. Further, Mr. Leo Rogers, son of Mr. Thos. Rogers of Manchester, who has spent several years with the engineers of the Canadian Pacific Railway, has informed me that earthworms are unknown between Winnipeg and the Rockies. This being the case, it does not seem reasonable to suppose that they exist anywhere in the huge territory still further to the north, and comprising upwards of 3,000,000 square miles of land, or something like one third of the entire North American continent, and which may therefore be regarded as forming an exception to Mr. Darwin's statement (p. 120), that "Worms are found in all parts of the world, and some of the genera have an enormous range. They inhabit the most isolated islands; they abound in Iceland, and are known to exist in the West Indies, St. Helena, Madagascar, New Caledonia, and Tahiti. In the Antarctic regions worms from Kerguelen Land have been described by Ray Lankester, and I have found them in the Falkland Islands. How they reach such isolated spots is at present quite unknown." In connection with the statement (p. 121) that "Worms throw up plenty of castings in the United States," it may be pointed out that the boundary line (the 49th parallel) is to some extent a natural one, from which the rivers run both north and south. Further, I have been assured by friends, and have also seen with my own eyes, that earthworms abound at Toronto and in other parts of Ontario. This being the case, an interesting inquiry arises as to the cause of the absence of worms from the North-West, and I can only suggest two probable reasons—the great cold of winter and the prevalence of prairie fires in spring and autumn. Personally I favour the latter, though both causes may in part be answerable. If worms abound in Iceland (65° N. lat.), in Kerguelen Land (50° S. lat.), and in Toronto (43·4° N. lat., mean winter temperature 27¼° F.), why should they not also occur at Winnipeg (50° N. lat.)? Certainly the mean winter temperature is very low, being about 8° F., and the mean minimum for eleven years − 40½° F. I made special inquiries as to the depth to which the soil in Manitoba becomes frozen in winter. This is often as much as five or six feet, but only, I believe, in the more exposed places, and certainly as a rule it is thawed again in the spring. I do not think this would render the ground uninhabitable by worms when they are able to exist in Iceland. Mr. Darwin says nothing as to the effect of frost on worms except (p. 26) that "worms are sensitive to a low temperature, as may be inferred from their not coming out of their burrows during a frost"; but he states (p. 110) that they are easily able to descend three or four or even seven or eight feet below the surface. It would be interesting to ascertain whether worms inhabit equally cold portions of the Old World.

But the agency which I believe has caused the absence of earthworms from the North-West is, as already stated, the prairie fires which annually sweep over enormous portions of the country, totally consuming the grass, and converting it into a black ash. This, it might well be imagined, would for months together completely deprive any worms that formerly existed of that variety of decaying vegetable matter that composes their food; and assuming that fires have annually passed over large portions of the prairies for scores of generations (as seems in every way probable), it appears to me only reasonable to suppose that this cause would effectually have exterminated the worms from the country or have prevented them occupying it. It is my belief (as I shall elsewhere state more fully) that the very fertile, fine, black, powdery, and almost soot-like soil from one to three feet thick, even the open, treeless nature of the prairies themselves, and the absence from their surface, so far as my observation goes, of every single species of mollusk, while many species abound in all the ponds, lakes, and streams, are all in a large degree, if not entirely, due to the action of the fire. If this view ultimately turns out to be correct, it will be further seen that the very means which has deprived the soil of the North-West of that natural cultivation which the soils of most other countries enjoy has, at the same time, liberally supplied it with a manure resulting from the charred ashes of the grass which is annually burned. My friend, Mr. T. Rogers, who has taken much interest in the absence of worms from the North-West, and is inclined to attribute it rather to frost than to fire, though he suggests that the "alkali" may possibly have had something to do with it, has already brought the subject before the Literary and Scientific Society of Manchester, where he seems to have met with a good deal of incredulity.

As another evidence of the absence of worms, the numerous, large, Glacial boulders that strew the prairies around Brandon and elsewhere may be cited. These, had worms existed, would doubtless have long ago been lowered beneath the surface, as also the skulls and other bones of buffaloes, which so abound on the prairies, and most of which have evidently lain there a long while. Nevertheless some of these have been buried in the course of time, as one gentleman told me that he had sometimes turned them up from a depth of two or three inches beneath the surface when ploughing. Their burial may have been accomplished by the wind drifting soil over them, or by the working of gophers. Of these peculiar little animals two species are very abundant on the prairies, where they make extensive burrows, which it seems possible may to some extent accomplish the natural cultivation of the soil in the way worms are accustomed to do it elsewhere. Some more suggestive remarks on this point may be found in a paper by Mr. Seton, published in the Report of the Manitoba Department of Agriculture for 1882, and which may be studied with advantage. ROBT. MILLER CHRISTY

Chignal St. James, near Chelmsford, December 20, 1883

Magnetic Dip in South China and Formosa

WHILE engaged on a meteorological mission in China I availed myself of the opportunity to make the following determinations of the magnetic dip. The observations in Hong Kong were made at the public gardens, the Observatory being not yet ready. On October 10 I observed at the British Consulate; on November 3 at the English Presbyterian Missions Compound, Swatow. In Amoy I observed at the residence of the Commissioner I.M. Customs, in Takow (Formosa) at the Custom House, and at the South Cape (Formosa), near the magnificent fortified lighthouse. It is to be feared that the observations on the coast of China are slightly vitiated from local attraction, the rocks consisting of ferruginous granite. Southern Formosa is built up of coral, raised in places to a great height, no doubt through volcanic action. Slight earthquakes are of common occurrence in Formosa, whereas along the coast of China they are rare and of no importance except to the seismologist.

Place	Date	Local M.T. h. m.	Dip. ° ′
Hong Kong ...	1883, Nov. 5 ...	5 9 p.m. ...	32 17
,, ...	,, ,, 9 ...	5 4 ,, ...	32 19
Swatow	,, Oct. 10 ...	5 24 ,, ...	34 23
,, ...	,, Nov. 3 ...	11 25 a.m. ...	34 17
Amoy	,, Oct. 14 ...	3 50 p.m. ...	36 45
,, ...	,, ,, 16 ...	5 10 ,, ...	36 50
Takow	,, ,, 24 ...	2 45 ,, ...	32 54
South Cape ...	,, ,, 27 ...	4 0 ,, ...	31 24
,, ...	,, ,, 28 ...	4 30 ,, ...	31 27·5
,, ...	,, ,, 29 ...	3 20 ,, ...	31 24·5

W. DOBERCK,
Government Astronomer

Hong Kong, Nov. 10

THE ORIGIN OF CORAL REEFS

REGARDING this interesting geological problem, which has recently been discussed in NATURE, we are enabled through the kindness of Mr. Murray, of the *Challenger* Commission, to publish a letter which has been addressed to him by Dr. Guppy from the Pacific. The importance of this communication will be recognised in the confirmation it supplies of the inference that coral reefs start upon a platform of limestone composed of the remains of foraminifera, &c., and are themselves of no great thickness. Dr. Guppy will no doubt continue his researches, and we may hope to obtain from him precise data regarding the average thickness of the coral rock, the lithological difference between it and the underlying limestone, the structure of the limestone, whether any succession of organisms can be detected in it, and whether at any point the underlying volcanic rock can be seen which would afford a measurement of the thickness of the calcareous deposits. The effects of denudation and their relation to height above the sea will no doubt also receive his attention.

"*Shortlands Islands, Solomon Group*,
"*August* 7, 1883

"During the twelve months I have spent in this group of islands—serving as surgeon on board H.M. surveying-ship *Lark*—I have been much interested in and have devoted considerable attention to the raised coral formations in various islands; and as my observations may be of service towards confirming the views which you have advanced with reference to coral islands and reefs, I will state briefly the results of my observations.

"Excluding the large continental islands, I will refer for the sake of brevity to the numerous small islands of this archipelago, those of volcanic, and those of calcareous formations. Confining myself to the islands of calcareous formation, I will pass over the numerous small islands which are entirely composed of coral detritus, sand, and shells, and have been formed by the materials thrown up by the waves at the present sea-level; and will restrict my remarks to a very common type of islands in this group, with gently sloping and rounded profile, having an elevation varying perhaps between 100 and 1100 or 1200 feet, and composed in bulk of *an impure earthy or argillaceous limestone*, usually bedded, and almost always *foraminiferous*, now and then rich in other pelagic organisms, such as *Pteropods*. On this rock *rests the* *coral limestone*, which forms but a comparatively thin crust, and has been altogether removed from most of the higher regions by sub-aërial agencies. However, I have observed the raised coral rock still preserved at considerable heights above the sea, and in two localities at elevations of 900 feet.

"Amongst the sub-group known as the Shortland Islands, I came upon beds of this *impure calcareous rock* (beneath the raised coral rock) *abounding in Pteropods*, mostly *Hyalæa*, and large foraminiferous tests, mingled with shells, some of them of shallow water habit.

"I am, &c., "H. B. GUPPY"

A FORGOTTEN EVOLUTIONIST

A BOOK has lately come into my hands a few words about which may possibly interest some of the readers of NATURE. Its title is "Histoire Naturelle des Fraisiers"; the author was A. N. Duchesne, and it was published at Paris in 1766. It must be, I suspect, an uncommon book, for there is no copy in the library of the Royal Gardens at Kew. And this library, comprising as it does the contributions of many collectors who allowed little to escape them, is remarkably complete; Mr. Daydon Jackson has in fact found in it more than a thousand publications the titles of which are not to be met with in the last edition of Pritzel's well-known "Thesaurus."

The scarceness of a botanical book is not perhaps in itself a matter of any great moment, and I bought the book out of a provincial sale catalogue without expecting it to be particularly interesting, though I knew Duchesne's name as an authority on the cultivated forms of the strawberry. I very soon, however, came to the conclusion on looking over it that it was a very remarkable production indeed, and in a scientific sense at least a century in advance of its time.

Duchesne's book is in fact the record of a purely biological study of a small group of plants. The significance of work of this sort has only been thoroughly recognised since the publication of the "Origin of Species." Just as with C. K. Sprengel, whose book was also written in the last century (1793), the world has had to roll on far into another hundred years before it was ready to do justice to this kind of research. There is a curious incongruousness between the freshness and modernness of the ideas and the faded type and musty paper in which they are embalmed.

Duchesne plunges at once into the business of his book in the first line of the preface with a straightforward simplicity not unworthy of Mr. Darwin. I will attempt a translation of the first paragraph :—

"The wish to see if it were possible to raise from seed a plant which scarcely ever produces any has led me by a happy chance to the production of a new race, which made its appearance at Versailles in 1761. This circumstance induced me to more closely devote myself to the study of strawberries, and led me to another discovery. I found that they are not all truly hermaphrodite; forms exist, in fact, which are sexually differentiated.[1] And I have succeeded in the past year, 1765, in fertilising, by means of one set of plants, individuals of another sort, which are cultivated as a matter of curiosity, and are constantly sterile. One, amongst others, has produced fruits of great beauty; M. le Marquis de Marigny has obtained for me the honour of having this submitted to the king, and it is to be raised in the Versailles Gardens by my method. This unexpected success has still more redoubled my ardour to make further observations."

The race so produced, which Duchesne called *Le Fraisier de Versailles*, or *Fragaria monophylla*, is undoubtedly a very curious plant. All its leaves are permanently unifoliate; *i.e.* instead of bearing three leaflets, as is ordinarily the case with strawberries, the petioles bear but one. Duchesne observes that this is also the case with the first leaves of all seedling strawberries. *Fragaria monophylla* may be therefore regarded as a form which always retains the juvenile, and never arrives at the adult, foliage, and this peculiarity remains constant in subsequent generations. The effect of crossing, as a potent stimulus to variation, could not but have powerfully impressed Duchesne in so striking a case as this, and further observations seemed to have led him to account for the common characters which otherwise diverging forms exhibited as best accounted for by a common ancestral origin. The study of geographically separated species, however, necessarily led him to see that something more than crossing was needed to account for variation in every case. In discussing *Fragaria virginiana*, a native of North America, which is the origin of the race of Scarlets, Duchesne speculates as to its derivation from the wild *F. vesca* of Europe, and attributes the divergences from this type to the effect of North American soil and climate.

His work on Strawberries, where he was dealing mainly with races, led him to speculate with regard to the higher groups of species, genera, and orders. His results seem to me, for the time, so extraordinarily bold, and therefore historically so interesting, that I quote the first portion of the Recapitulation, pp. 219-221, entire, in the original French :—

"J'ai déjà dit, à l'occasion du Fraisier-ananas, qu'il étoit très-difficile de ranger en ligne droite les diverses Races d'une même Espèce, de manière qu'on pût passer de l'une a l'autre par gradations de nuance. Cela est peut-être aussi impossible, que de ranger en ligne droite les Espèces, les Genres, et les Familles; par la raison que chaque Race, comme chaque Espèce, chaque Genre, ou chaque Famille, a des rapports de ressemblance avec plusieurs autres.

"L'ordre Généalogique est donc le seul que la nature indique, le seul qui satisfasse pleinement l'esprit; tout autre est arbitraire et vide d'idées. J'ai eu soin, à chacune des Races de Fraisiers, d'indiquer ce qui m'a paru vraisemblable à cet égard; mais je n'ose me flatter d'avoir toujours rencontré juste. Il faudroit, pour le bien faire, avoir des connaissances certaines et précises du pays natal de chaque Fraisier, ou bien, du tems où il a été élevé de graine, et de quel autre Fraisier provenoit cette graine; j'ai fait voir combien on manquoit encore de lumières sur tout cela.

"C'est par cette raison que je me suis permis de donner mes conjectures; en voici les résultats; la forme d'Arbre généalogique les rendra encore plus sensibles, et en fera mieux saisir l'ensemble."

It is certainly startling to come upon a phylogeny of the most modern type in a book more than a century old.

It was not till after I had gratified myself with a study of Duchesne's remarkable speculations that it flashed across my mind that attention had already recently been called to them; and I found, in fact, that Prof. Alphonse de Candolle, in a short paper put together with the felicitous erudition of which he seems to possess so inexhaustible a store, had already, in May of last year,[1] stated most of the points on which I have dwelt above. And he mentions that, on the occasion of a visit to Mr. Darwin in 1880 he told him of the existence of the book, which he describes, justly enough, as "a very curious work, older than that of Lamarck, but to which no one had ever referred except for points of secondary interest."

I know little about Duchesne himself. De Candolle says that he was a horticulturist and Professor of Natural History, and that his knowledge was as varied as it was

[1] This must be one of the first observations of the tendency of plants with hermaphrodite flowers to pass into the diœcious state. The fact is now well established. (See Darwin's "Forms of Flowers," pp. 278-309.)

[1] "Darwin considéré au point de vue des causes de son succès," &c., *Archives des Sciences*, May, 1882.

sound. No one, nevertheless, ever seems to have paid the smallest attention to his evolutionary theories. Even Silvestre, who pronounced his *éloge* at a public meeting of the Société Royale d'Agriculture in 1827, abstains from the slightest reference to them.

While in his experiments and his mode of drawing conclusions from them Duchesne strongly recalls the method of Mr. Darwin, the parallel cannot be carried further. In so far as he obtained a glimpse at the modern doctrine of evolution it was in the form afterwards formulated by Lamarck. Of the part played by the struggle for existence in the matter I find no trace in his writings. W. T. THISELTON DYER

TEACHING ANIMALS TO CONVERSE

MR. DARWIN'S notes on Instinct, recently published by my friend Mr. Romanes, have again called our attention to the interesting subject of instinct in animals.

Miss Martineau once remarked that, considering how long we have lived in close association with animals, it is astonishing how little we know about them, and especially about their mental condition. This applies with especial force to our domestic animals, and above all of course to dogs.

I believe that it arises very much from the fact that hitherto we have tried to teach animals rather than to learn from them,—to convey our ideas to them, rather than to devise any language, or code of signals, by means of which they might communicate theirs to us. No doubt the former process is interesting and instructive, but it does not carry us very far.

Under these circumstances it has occurred to me whether some such system as that followed with deaf-mutes, especially by Dr. Howe with Laura Bridgman, might not prove very instructive if adapted to the case of dogs.

Accordingly I prepared some pieces of stout cardboard, and printed on each in legible letters a word such as "Food," "Bone," "Out," &c. The head master of one of the deaf and dumb schools kindly agreed to assist me. We each began with a terrier puppy, but neither of us obtained any satisfactory results. My dog indeed was lost before I had had him long. I then began training a black poodle, "Van" by name, kindly given me by my friend Mr. Nickalls. I commenced by giving the dog food in a saucer, over which I laid the card on which was the word "Food," placing also by the side an empty saucer, covered by a plain card.

"Van" soon learnt to distinguish between the two, and the next stage was to teach him to bring me the card; this he now does, and hands it to me quite prettily, and I then give him a bone, or a little food, or take him out, according to the card brought. He still brings sometimes a plain card, in which case I point out his error, and he then takes it back and changes it. This however does not often happen. Yesterday morning, for instance, "Van" brought me the card with "Food" on it, nine times in succession, selecting it from among other plain cards, though I changed the relative position every time.

No one who sees him can doubt that he understands the act of bringing the card with the word "Food" on it as a request for something to eat, and that he distinguishes between it and a plain card. I also believe that he distinguishes for instance between the card with the word "Food" on it and the card with "Out" on it.

This then seems to open up a method, which may be carried much further, for it is obvious that the cards may be multiplied, and the dog thus enabled to communicate freely with us. I have as yet, I know, made only a very small beginning, and hope to carry the experiment much further, but my object in sending this communication is twofold. In the first place I trust that some of the readers of NATURE may be able and willing to suggest extensions and improvements of the idea.

Secondly, my spare time is small and liable to many interruptions; animals also we know differ greatly from one another. Now many of your readers have favourite dogs, and I would express a hope that some of them may be disposed to study them in the manner indicated.

The observations, even though negative, would be interesting; but I confess I hope that some positive results might follow, which would enable us to obtain a more correct insight into the minds of animals than we have yet acquired. JOHN LUBBOCK

High Elms, Down, Kent, December 20, 1883

THE FRENCH DEEP-SEA EXPEDITION OF 1883

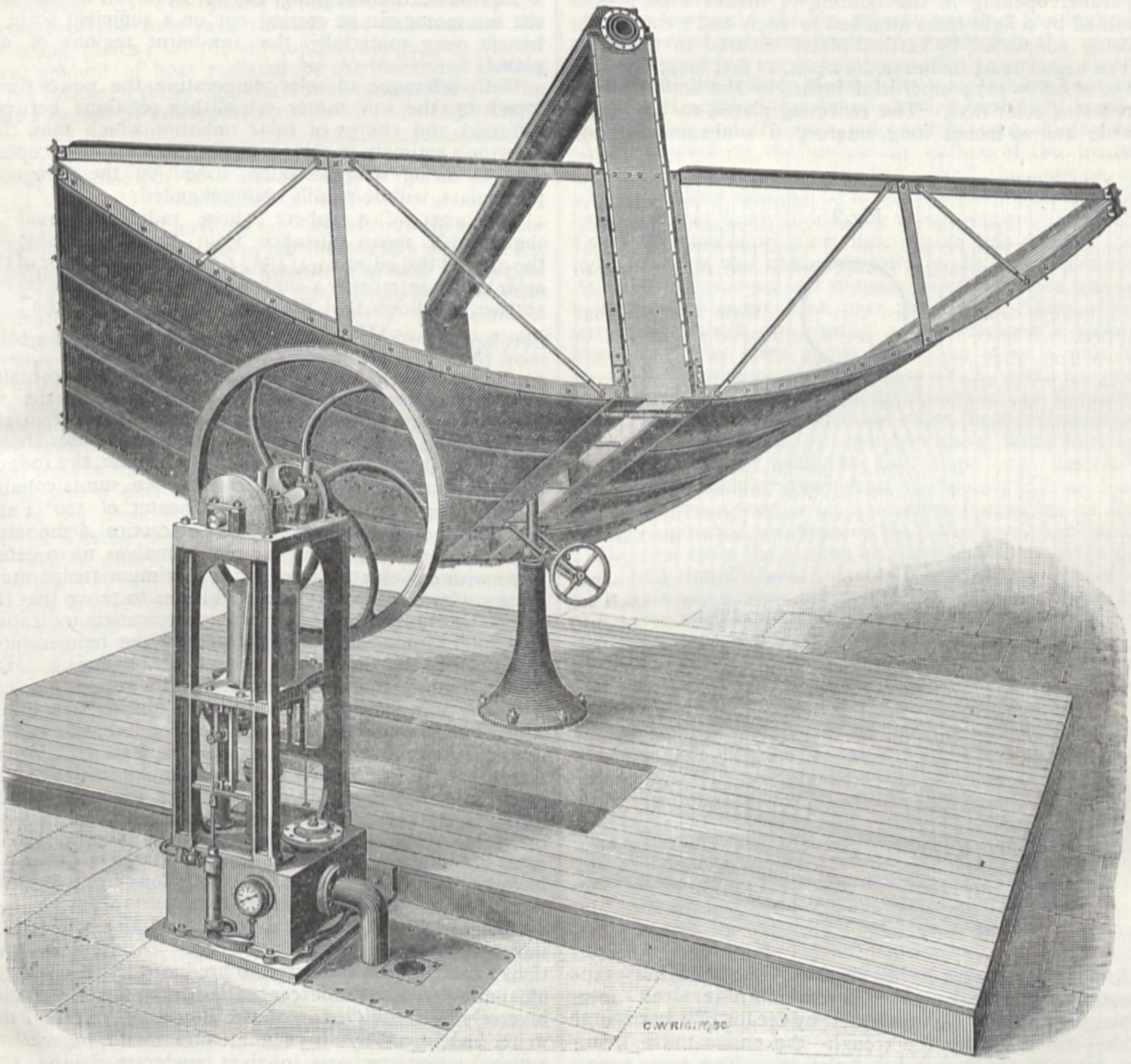
I HAVE just returned from a very short visit to Paris, made for the purpose of inspecting the Mollusca which were procured during last summer's deep-sea expedition in the French Government steamer *Talisman*. The expedition was under the scientific charge of Prof. Alphonse Milne-Edwards. For the opportunity of this inspection I was indebted to the kindness of my friend Dr. Paul Fischer, whose reputation as a conchologist is so well known.

The course of the expedition was along the Atlantic coasts of Spain, Marocco, Sahara, Senegal, Cape Verde Isles, the Canaries, and Azores; and the time occupied was three months. More full and accurate particulars will very shortly be given by Prof. A. Milne-Edwards to the Academy of Sciences, and be published in their *Comptes Rendus*. The collection will be exhibited next month to public view. The greatest depth explored was about 2200 fathoms. The trawl was mostly used. Life was plentiful everywhere. As was the case in the *Porcupine*, *Challenger*, and other expeditions of the same kind, many animals (especially Crustacea) at the greatest depths were highly and brightly coloured, some of them having large eyes, and others being blind or eyeless. There was an abundance of hitherto unknown forms (genera and species) in every department of zoology—fishes, Mollusca, Polyzoa, Crustacea, Annelids, Echinoderms, Polyps, Corals, Foraminifera, and Sponges Among the Mollusca were some remarkable cases of the wide distribution of species in respect of space as well as of depth. For instance, boreal shells, such as *Fusus islandicus* and *F. berniciensis*, which inhabit northern seas at moderate depths, viz. 50 to 80 fathoms, were found living off the coast of Marocco, and the latter species even below the tropic of Capricorn, at depths of from 450 to 2200 fathoms. *Lima excavata*, considered a peculiarly Norwegian species, was likewise obtained off the Moorish coast, of a very large size; it was recorded by Prof. Seguenza as a Pliocene fossil of Sicily and Calabria, under the name of *Lima gigantea*. In the *Porcupine* Expedition of 1870 fragments were dredged off Cape St. Vincent; and in the *Challenger* Expedition this fine species was obtained from 10 to 175 fathoms off Western Patagonia and Japan. A bivalve (*Scrobicularia longicallus*), which in northern seas inhabits moderate depths, was procured in many places by the *Talisman*, at depths varying from 350 to 1429 fathoms. It occurred living in the deepest dredgings of the *Porcupine* Expedition of 1869, off the coast of Brittany, at a depth of 2435 fathoms. Many Mollusca (*e.g. Pecten vitreus, Limopsis minuta, Dentalium agile, Trochus ottoi, Columbella haliæeti*, and *Scaphander punctostriatus*) seem to inhabit the depths of the North Atlantic in every part, from one side to the other. The smaller shells in the *Talisman* collection have not yet been picked out. The Marquis de Folin will, with his usual care and industry, undertake that part of the work, which will occupy some time; he has requested me to examine and name those species which are known to me. I understand that another deep-sea expedition will be made by our enterprising neighbours next summer, being the fourth in consecutive years.[1]

December 21, 1883 J. GWYN JEFFREYS

THE SUN MOTOR AND THE SUN'S TEMPERATURE

THE annexed illustration (Fig. 1) represents a perspective view of a sun motor constructed by the writer, and put in operation last summer. This mechanical device for utilising the sun's radiant heat is the result of experiments conducted during a series of twenty years; a succession of experimental machines of similar general design, but varying in detail, having been built during that period. The leading feature of the sun motor is that of concentrating the radiant heat by means of a rectangular trough having a curved bottom lined on the inside with polished plates so arranged that they reflect the sun's rays towards a cylindrical heater placed longitudinally above the trough. This heater, it is scarcely necessary to state, contains the acting medium, steam or air, employed to transfer the solar energy to the motor; the transfer being effected by



Ericsson's Sun Motor, erected at New York, 1883.

means of cylinders provided with pistons and valves resembling those of motive engines of the ordinary type. Practical engineers as well as scientists have demonstrated that solar energy cannot be rendered available for producing motive power, in consequence of the feebleness of solar radiation. The great cost of large reflectors and the difficulty of producing accurate curvature on a large scale, besides the great amount of labour called for in preventing the polished surface from becoming tarnished, are objections which have been supposed to render direct solar energy practically useless for producing mechanical power.

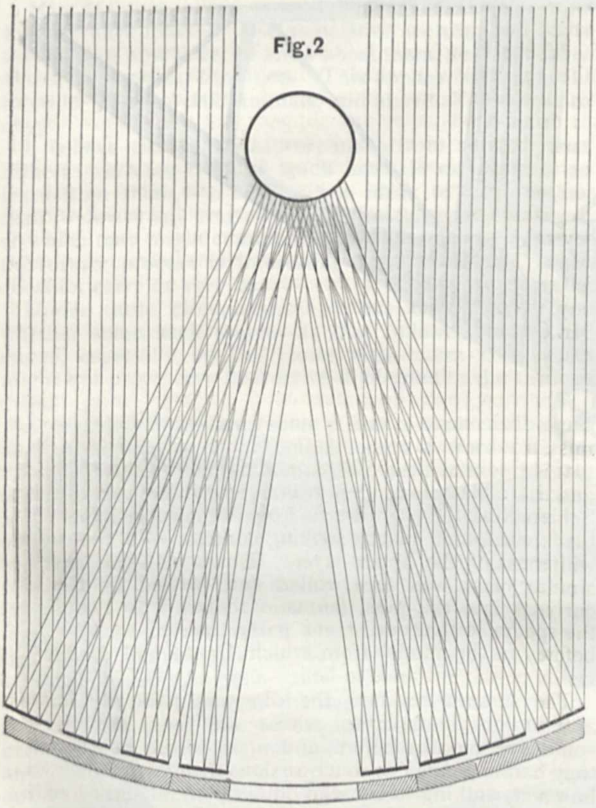
The device under consideration overcomes the stated objections by very simple means, as will be seen by the following description:—The bottom of the rectangular trough consists of straight wooden staves, supported by iron ribs of parabolic curvature secured to the sides of the trough. On these staves the reflecting plates, consisting

[1] P.S.—In NATURE of December 20 (p. 172), I overlooked the misprint of India for Sweden.—J. G. J.

of flat window glass silvered on the under side, are fastened. It will be readily understood that the method thus adopted for concentrating the radiant heat does not call for a structure of great accuracy, provided the wooden staves are secured to the iron ribs in such a position that the silvered plates attached to the same reflect the solar rays towards the heater. Fig. 2 represents a transverse section of the latter, part of the bottom of the trough, and sections of the reflecting plates; the direct and reflected solar rays being indicated by vertical and diagonal lines.

Referring to the illustration, it will be seen that the trough, 11 feet long, and 16 feet broad, including a parallel opening in the bottom, 12 inches wide, is sustained by a light truss attached to each end; the heater being supported by vertical plates secured to the truss. The heater is $6\frac{1}{4}$ inches in diameter, 11 feet long, exposing $130 \times 9{\cdot}8 = 1274$ superficial inches to the action of the reflected solar rays. The reflecting plates, each 3 inches wide and 26 inches long, intercept a sunbeam of $130 \times 180 = 23{,}400$ square inches section. The trough is supported by a central pivot, round which it revolves. The change of inclination is effected by means of a horizontal axle—concealed by the trough—the entire mass being so accurately balanced that a pull of five pounds applied at the extremity enables a person to change the inclination or cause the whole to revolve. A single revolution of the motive engine develops more power than needed to turn the trough, and regulate its inclination so as to face the sun, during a day's operation.

Fig. 2



The motor shown by the illustration is a steam-engine, the working cylinder being 6 inches in diameter, with 8 inches stroke. The piston rod, passing through the bottom of the cylinder, operates a force-pump of 5 inches diameter. By means of an ordinary cross-head secured to the piston-rod below the steam cylinder, and by ordinary connecting rods, motion is imparted to a crank shaft and fly-wheel, applied at the top of the engine frame; the object of this arrangement being that of showing the capability of the engine to work either pumps or mills. It should be noticed that the flexible steam-pipe employed to convey the steam to the engine, as well as the steam chamber attached to the upper end of the heater, have been excluded in the illustration. The average speed of the engine during the trials last summer was 120 turns per minute, the absolute pressure on the working piston being 35 lbs. per square inch. The steam was worked expansively in the ratio of 1 to 3, with a nearly perfect vacuum kept up in the condenser inclosed in the pedestal which supports the engine frame.

In view of the foregoing, experts need not be told that the sun motor can be carried out on a sufficient scale to benefit very materially the sun-burnt regions of our planet.

With reference to solar temperature, the power developed by the sun motor establishes relations between diffusion and energy of solar radiation which show that Newton's estimate of solar temperature must be accepted. The following demonstration, based on the foregoing particulars, will be readily comprehended.

The area of a sphere whose radius is equal to the earth's mean distance from the sun being to the area of the latter as $214{\cdot}5^2 : 1$, while the reflector of the solar motor intercepts a sunbeam of 23,400 square inches section, it follows that the reflector will receive the heat developed by $\frac{23400}{214{\cdot}5^2} = 0{\cdot}508$ square inch of the solar surface. Hence, as the heater of the motor contains 1274 square inches, we establish the fact that the reflected solar rays acting on the same are *diffused* in the ratio of $1274 : 0{\cdot}508 = 2507 : 1$. Practice has now shown that, notwithstanding this extreme diffusion, the radiant energy transmitted to the reflector by the sun is capable of imparting a temperature to the heater of 520° Fahr. above that of the atmosphere. The practical demonstration thus furnished by the sun motor enables us to determine with sufficient exactness the minimum temperature of the solar surface. It also enables us to prove that the calculations made by certain French scientists indicating that solar temperature does not exceed the temperatures produced in the laboratory are wholly erroneous. Had Pouillet known that solar radiation, after suffering a *two-thousand-five-hundred-and-seven-fold* diffusion, retains a radiant energy of 520° Fahr., he would not have asserted that the temperature of the solar surface is 1760° C. Accepting Newton's law that "the temperature is as the density of the rays," the temperature imparted to the heater of the sun motor proves that the temperature of the solar surface cannot be less than $520° \times 2507 = 1{,}303{,}640°$ Fahr. Let us bear in mind that, while attempts have been made to establish a much lower temperature than Newton's estimate, no demonstration whatever has yet been produced tending to *prove* that the said law is unsound. On the contrary, the most careful investigations show that the temperature produced by radiant heat emanating from incandescent spherical bodies diminishes inversely as the *diffusion* of the heat rays. Again, the writer has proved by his vacuum-actinometer, inclosed in a vessel maintained at a constant temperature during the observations, that for equal zenith distance the intensity of solar radiation at midsummer is 5°·88 Fahr. less than during the winter solstice. This diminution of the sun's radiant heat in aphelion, it will be found, corresponds within 0°·40 of the temperature which Newton's law demands. It is proposed to discuss this branch of the subject more fully on a future occasion.

The operation of the sun motor, it will be well to add, furnishes another proof in support of Newton's assumption that the energy increases as the *density* of the rays. The foregoing explanation concerning the reflection of the rays (see Fig. 2), shows that no augmentation of temperature takes place during their transmission from the reflector to the heater. Yet we find that an increase of the number of reflecting plates increases proportionably the power of the motor. Considering that the parallelism of the rays absolutely prevents augmentation of temperature during the transmission, it will be asked: What causes the observed increase of mechanical power? Obviously, the energy produced by the increased *density* of the rays acting on the heater. The truth of the Newtonian doctrine, that the energy increases as the density of the rays, has thus been verified by a practical test which cannot be questioned. It is scarcely necessary to observe that our computation of temperature—1,303,640° Fahr.—does not show maximum solar intensity, the following points, besides atmospheric absorption, not having been considered:—(1) The diminution of energy attending the passage of the heat rays through the substance of the reflecting plates; (2) the diminution consequent on the great amount of heat radiated by the blackened surface of the heater; (3) the diminution of temperature in the heater caused by convection. J. ERICSSON

A CHRISTMAS VISIT TO BEN NEVIS OBSERVATORY

ALTHOUGH I have no tale of perilous adventure or hair-breadth escape to tell the readers of NATURE, yet I think that they will be interested to hear of the progress that is being made in the first British attempt at the cultivation of high-level meteorology. This interest will be all the greater that the hearty encouragement and support that the Ben Nevis experiment has received from all parts of the United Kingdom has given it the character of a national undertaking.

As most of the readers of NATURE doubtless know, the observatory is at present in the experimental stage. A good road to the top with bridges and waterways has been made, and a part of the building erected sufficient to shelter the observers. It was judged wise to build as little as possible, until experience should have taught us the peculiar difficulties to be contended with in the somewhat novel circumstances presented by the summit of Ben Nevis in winter time. For, although several high level meteorological observatories, and indeed many other human habitations, already exist at much greater heights above the sea, yet there is probably no spot at present inhabited all the year round that presents climatic vicissitudes so remarkable. When winter is over, the directors will have a full report, with practical suggestions from the superintendent, Mr. Omond, to guide them in their further operations. Still it was thought well that some of the governing body should see with their own eyes the state of the observatory, and the work of the observers during the cold season. Accordingly two of them (Mr. John Murray and myself) made a visit of inspection on December 26th, of which I propose to give a few particulars.

Accompanied by Mr. Maclean, the contractor for the road and observatory buildings, we started from Fort William about 9.30 on Wednesday morning. At first the sky was dark and gloomy, and it was thought that Ben Nevis was to give a specimen of his worst weather. It was not cold however; in fact it was oppressively warm during the first thousand feet of the ascent from the farm of Achantie where the new road begins. This, coupled with the fact that the pony which one of the party rode up the first 2500 feet of the hill somewhat forced the pace, made it a little uncomfortable for the two pedestrians. The newly made road, loosened by the frost, and sodden by the rain and melting snow, was in places very heavy. Up as far as the little lake (Loch an Meall aut Suidhe), however, the roadway had suffered no substantial damage, except that the fall of a large stone had carried away a small piece of the margin; and all the bridges and waterways were found in excellent condition. This is very satisfactory, for the snow has already been down to Fort William; and recently a very rapid thaw has carried it so completely away, that on the 26th very little was met with under 3000 feet. The test has thus been tolerably severe and yet up to 2600 feet or so the road on the 26th was in far better condition than it was on the day of the opening ceremony. About the altitude just mentioned, a part of the road had been badly ploughed up by a spate of water from the melting snow; higher up still, the damage seemed to be less, but it was not so easy to judge, as the roadway was there gradually lost in the overlying snow.

As the party rose in height, the temperature of the air and the ardour of the pony alike fell, and then the walkers were left to the full enjoyment of their climb. During the latter part of the first 3000 feet, the mist had been so thick that the pony and its rider could scarcely be discerned a few yards off; but several hundred feet higher, after the road had been finally lost sight of in the snow, and all the party were on foot, we suddenly emerged about noon from the gloom of the mist into the brightest of daylight. Overhead the sky was blue, a fresh light breeze was blowing, and the reflected sunlight was shining in silvery masses on the undulating surface of the frozen snow. We soon reached Buchan's Well, the position of which had been markéd by a wooden pole; but the well itself was completely hidden by a deep snow-drift, which filled the hollow in which it lies. From this spot to the top, the ascent was made almost straight over the snow. At times it was steep and slippery, but the surface was so hard that we rarely sank over the ankles. Two of us were rough shod, one having a few cricketer's spikes screwed to the soles of his boots, the other a pair of *steigeisen* (climbing irons), the use of which he had learned several years ago during some excursions in the Tyrolese Alps. Mr. Maclean, who had not taken these precautions, fell once or twice, but fortunately without being hurt in any way. When near the last slope we descried Mr. Omond hacking away most assiduously with an ice-axe to prepare a way for us, a needless precaution as far as the rough-shod members of the party were concerned.

The view from the plateau on the summit was magnificent. All round there floated a billowy ocean of white mist, from which rose masses of the same, piled up in places like mountain ranges, and through which rose here and there black mountain peaks (prominent among these Schiehallion). Away towards Fort William was stretched a black curtain of mist in striking contrast with the snow-whiteness of the upper layer. Down in Glen Nevis a similar mass was seen, rolled and twisted by the air-currents into the most fantastic shapes. So grand was the spectacle that one of our party insisted that we had before us the model from which Dante had drawn his vision of the entrance to hell.

The summit reached, the directors naturally looked around for the building, whose site they had chosen some five months before, and upon whose construction they had expended so much anxious thought. There was, however, nothing to be seen but two small dark-looking stumps rising a little over the surrounding snow-flat, and alongside of these a little mound of snow. The stumps turned out to be the chimney and ventilator on the roof of the observatory, and the mound was a portico built by the observers with blocks of frozen snow to protect a snow staircase which had been carried down the side of the house to the doorway. After descending under the translucent canopy and stumbling for a little in the unfamiliar darkness of the passage, we entered the main room of the observatory, which for the present serves as sitting-room, kitchen, and office combined. Here we found the table laid for our lunch; and very soon we were comforting ourselves with hot coffee, cabin biscuits, and excellent Danish butter from the stores of the establishment. The whole ascent had occupied a little over three hours and a half.

The little room in which we sat contained the American stove which heats the whole observatory, and on which the snow melting and all the cooking is done by John Duncan, the second assistant observer and housemaid. On one of the walls is the combined sideboard and crockery and instrument cupboard; against another stands a small bench with a vice; and on a third is the telegraph instrument, Mr. Omond's desk and book-case, and the drawers in which are kept the records of the observatory. Out of the sitting-room open the three bedrooms for the observers, which resemble very closely the cabins on board a ship; indeed the whole establishment has an intensely nautical air about it, and the visitor steadies himself instinctively now and then, and wonders that the roll never comes.

The rest of the building is occupied with a coal and oil store, and a storeroom in which are kept the cabin biscuits, dried potatoes, tinned soups, meat, and vegetables, lime juice, and medicine chest; which Mr. Omond calculates will support the three observers till June.

The afternoon and evening we spent in watching the observers at work, in dining (which we did very comfortably off the Christmas cheer, viz. roast turkey and plum pudding, provided for the inhabitants of Ben Nevis by a thoughtful friend), and in eager discussion of plans for the present and future work of the Observatory. The routine of the observatory at present consists in hourly observations of the barometer, protected thermometers, dry and wet bulb and maximum and minimum, wind-direction and pressure, rain, snow, sleet or hail, mist, fog or haze, clouds lower and upper, amount, species, and direction, sunshine recorder, miscellaneous, thunder, lightning, haloes, auroræ, meteors, &c., nature and precise time of occurrence of. The self-registering barograph and thermograph now added to the collection of instruments are working very well, and will be invaluable for the record of sudden changes. The protected thermometers and the thermograph are attached to a ladder fixed in the snow. As the level of the snow rises and falls, they are moved from step to step, so as to keep them as nearly as possible to the regulation distance of four feet from the surface. A measurement from the top of the ladder to the surface gives the depth of the snow, which at present varies from six to ten feet at different parts of the summit of the mountain.

Any detailed account of the winter climate of Ben Nevis would be premature and out of place in this notice; but Mr. Buchan has kindly furnished me with an analysis of the meteorological phenomena on Christmas and the following day which were in several respects remarkable.

At 1 A.M. of Christmas day, temperature was 37° 0 from which it steadily fell to 31°·5 at 11 A.M., the air all the time being quite saturated and loaded with dark, gloomy mist, with a barometer steadily rising. The wind was moderate from north-west till 3 A.M., when it changed to west-south-west. About noon the mist pall cleared away and the sun shone out with great splendour. From this hour to midnight, the following most remarkable observations were made (see table).

	Barom.	Therm.		Humidity.	
	Inches.	Dry.	Wet.	Calculated.	Air Hygrometer.
Noon	25·822	33°·0	31°·9	88	—
1 P.M.	·834	36°·9	33°·4	70	86
2 ,,	·824	37° 6	33°·1	66	76
3 ,,	·823	40°·9	31°·8	45	64
4 ,,	·819	37°·7	30°·8	50	71
5 ,,	·818	39°·8	31°·8	47	60
6 ,,	·817	40°·0	32°·8	50	56
7 ,,	·811	39°·3	32°·2	51	57
8 ,,	·813	38°·4	32°·1	53	64
9 ,,	·812	32°·8	31°·7	87	77
10 ,,	·813	38°·8	34°·7	69	63
11 ,,	·804	37°·0	36°·1	92	65
Midnight	·809	38°·9	38°·5	97	62

Except a few cirrus clouds which appeared about one, three, four, and ten o'clock, the sky was cloudless throughout, and during the evening the stars sparkled with unwonted brightness in the dark blue sky.

These remarkable atmospheric conditions were strictly confined to the higher region of Ben Nevis. Fog or cloud covered the lower hills and filled the valleys all the afternoon; it rose sometimes as high as the "plateau of storms," but was mostly below 3000 feet on Ben Nevis, and during the time no other hill showed itself through the sea of cloud. The sunset of the 25th, as well as the sunrise of the 26th, was very beautiful. On the 26th pressure remained high and steady, wind south-westerly, sky generally clear, and temperature and humidity equally remarkable as on the preceding day. Indeed at 2 P.M. the relative humidity, which was lower than could be calculated from Glaisher's Tables, was only 34. At 3 P.M. temperature had fallen 6°·9, humidity risen to 96°, and a light fog prevailed for the next four hours, the wind having shifted from south-west to west-north-west. About 7 P.M. the sky again cleared, temperature steady, rose from 28°·0 to 36°·0 at midnight, and a humidity as low as 67 was observed. The great significance of these observations on Ben Nevis will be more apparent when compared with the anticyclone which overspread so large a part of north-western Europe at the time, to which, being situated on its west side, we owed the mild weather of Christmas, 1883.

In addition to the hourly observations, the observers have had for some time back to conduct a constant warfare with the rapidly-accumulating snow. Every now and then all hands had to be turned out to clear the doors and windows of the observatory; and it sometimes happened that, when they went out for this purpose, the snow drifted in so rapidly that it was almost impossible to shut the door again. The device of the snow staircase got over the difficulty to a large extent as regards the door, and it is proposed to build tubes with short lengths of rectangular wooden framework, passing from the windows up to the surface of the snow. At the upper end of these will be placed, at night or during heavy snowfalls, light canvas doors, which can be afterwards removed and additional lengths of framework added according to necessity. The chimney will be lengthened in a similar way by means of iron tubes, which have been sent up for the purpose. In this way the difficulties of the present winter will be met. For the future it is proposed to get over the difficulty of the accumulating snow by building an observing tower at some little distance from the living-rooms. In this tower there will be several stories with doors to the four cardinal points of the compass, so that the observers may use for exit and entrance that story which is nearest the snow level, and that door which happens to be on the lee-side of the tower. In the ground-floor of this tower it is proposed to place a seismometer and self-registering magnetic instruments. On the roof will be placed an anemometer for measuring the direction and strength of the wind. It is proposed so to arrange this instrument that its indications can be read inside the tower. This appears to be essential, for during the storm on the 12th ult. it was found impossible to go outside the observatory, so that wind observations are wanting in the daily sheet on that very interesting occasion. The observing-tower will be connected with the rest of the buildings by a covered way of some length fitted with doors to cut off the hot air; and in all probability the accommodation of the observatory will be increased by the addition of an office, or experimenting room, and one or more small bedrooms for the use of inspectors or others on temporary business, and for the convenience of scientific men who may wish to make a visit to the observatory for the purposes of scientific research.

For reasons sufficiently explained, the staff has scarcely had time as yet to go beyond the mere routine of observations above mentioned; but none of the valuable suggestions which Mr. Omond and the directors have received have been lost sight of. A beginning has already been made in the collection of meteoric dust; in fact Mr. Murray carried down with him a portion of the residue obtained by melting considerable quantities of surface snow. This is now being examined, and we shall doubtless hear by and by whether it is all of purely local, or partly of volcanic or cosmic origin.

It is intended, as soon as proper arrangements can be made, and the concurrence of the Post Office authorities obtained, to commence a series of simultaneous observations on earth currents along the cable from the summit of Ben Nevis to Fort William, and along a telegraph line from Fort William to some other station not far above sea-level. By means of this horizontal and vertical exploration we hope to obtain some interesting data (either positive or negative) regarding the origin of the variations of terrestrial magnetism, auroræ, &c. The cable will also be turned to account for observations on atmospheric electricity. These plans are mentioned partly to show that the directors are fully alive to the manifold uses to be made of their stronghold upon Ben Nevis, partly to incite scientific men generally to favour us with their suggestions for the full utilisation of the observatory, not only for meteorology, but for physical science in general.

It would take too long to dwell at length on all the interesting casual observations recorded in Mr. Omond's log, a detailed account of which will probably be given hereafter by Mr. Omond himself. It may be interesting, however, to allude to the frequently occurring phenomenon which he calls "Glories." The shadow of the head or hands of the observer is frequently seen on the clouds in the valley to the north-east surrounded by a halo of colour. The phenomenon appears to be akin to, or identical with, the mist phantom so well known under the name of the "Brocken Spectre." The occurrence of this phenomenon is by no means so rare in this country as many suppose. The writer of this notice saw it to perfection three years ago in Skye. A party of four or five of us were standing on Sgur-na-Panachtich, one of the Cuchullin peaks; we were looking down on the dark rock basin of Coruisk, in which was floating a cloud of mist. The sun was low behind us; and, projected on the mist, we saw what appeared to be gigantic dark shadows of ourselves completely outlined with a glory of rainbow colours. Each could see his own spectre best, but also those of his neighbours more or less distinctly. The figures imitated every motion we made, and, when we whirled our alpenstocks over our heads, the antics of the phantoms were most weird and awe-inspiring.

We spent the night of the 26th at the Observatory. During the first watch, that is, up to about one o'clock in the morning, we sat up, and went out with the observer when he made his hourly observations. The air felt quite mild, although the temperature was about the freezing point; the sky was perfectly clear, and the stars shone brilliantly. Mr. Omond brought out his telescope, and we lay down on the snow and examined Jupiter and his satellites, filled our eyes with the beauties of the Pleiades, and exhausted our little stocks of astronomical knowledge by naming such constellations as we happened to know.

The staff had insisted on providing each of us with a bed; we thus had good opportunity of testing their sleeping accommodation, which turned out to be excellent. Next morning we rose to see the sun rise, and were richly rewarded. About eight o'clock a ribbon of bright crimson appeared behind Schiehallion, which deloped a gorgeous succession of tints ending in copper colour and brick red, under the gradually rising sun; to right and left appeared the peculiar green colours so marked in the recent remarkable sunsets, to which the Ben Nevis sunrise showed a great resemblance. The greater part of the horizon was clear, and we had a view of the surrounding mountains seldom, if ever, equalled in summer time for beauty of colour. Ben More, the range of Glencoe, the Perthshire Hills, the whole length of the Caledonian Canal, the Cuchullin Hills, could all be seen with perfect distinctness. The white snow on the black-blue hilltops, and the bright red of the withered heather and bracken lower down, afforded contrasts of colour to be seen at no other season. Some of the hillsides shone in the sunlight like bronze. Others glowed like the richest velvet, and the valleys were filled with the subtle blue haze that gives such a charm to the scenery of the west of Scotland.

We naturally congratulated Mr. Omond on the weather he enjoyed on Ben Nevis; but it appeared that the treat was as great for him as for us. Since he began his seclusion on November 11, there had been just three fine days —the day on which he went up, Christmas day, and the day following, all the rest of the time the most he had seen was an occasional glimpse of a snow-covered mountain-peak through a hole in the mist. Our good fortune had been great; and, although it might have suited the main purpose of our visit better to have been detained by mist and sleet, or to have seen the observatory in the process of being buried in a snow-drift, we resigned ourselves with a very good grace to what the Fates had sent us.

After sharing the regulation breakfast of tinned mutton and coffee, we went out once more to see the observers at work. We then had an opportunity of seeing the precautions which they find it necessary to take in tempestuous weather when they have occasion to go near the edge of the narrow plateau on which they live. For sanitary reasons it is necessary to carry all the refuse of the observatory to a considerable distance, where it is thrown over a cliff. In winter, when this cliff is covered with a treacherous cornice of slippery snow, and the wind blows so hard that the head of a meat tin thrown to windward is often carried right back to leeward of the mountain, the footing at the edge is anything but secure. On such occasions two of the observers go abreast with the pail of rubbish between them, and each is roped to one who goes behind with an ice-axe to steady him in case of accident.

By 11 o'clock the barometer had begun to fall, and the humidity of the air had greatly increased. Mr. Omond therefore warned us that, unless we were prepared to incur the risk of detention, we had better depart. Accordingly we packed up our trophies, consisting of the residue above mentioned, pregnant with the potentiality of cosmic and volcanic dust, a bundle of Mr. Omond's daily sheets, and a little shrew that had been killed on the previous evening, the first of a colony of these animals who, with several weasels, had taken up their abode in the outer dry stone wall of the observatory. As might be expected, animal life is very scarce in winter on the top of Ben Nevis. No deer or ptarmigan had been seen, only the tracks of foxes, which abound in certain parts of the hill. The only living things we had seen in the snow-covered part of the hill were large numbers of a dipterous fly, which we found every now and then crawling on the surface of the snow.

Having bidden farewell to Mr. Omond and his companions, and wished them good luck and a continuance of their present good health and spirits during the rest of the winter, we commenced our descent at 11.30. The bottom was reached, after several halts to enjoy the magnificent view, in about the same time as it had taken us to ascend.

In such weather as we had the ascent of Ben Nevis is decidedly more pleasant and less fatiguing than in summer. It is well, however, to warn the readers of NATURE that our case was exceptional, and that under adverse circumstances such an enterprise is likely to be both unpleasant and dangerous. G. CHRYSTAL

THE REMARKABLE SUNSETS

INFORMATION with regard to these beautiful phenomena and their cause is rapidly being collected, and at the same time the opinions of those who have given most attention to them are being stated, both here and on the Continent. Among the latter we may refer to a memoir presented by Prof. Forel to the Société Vaudoise des Sciences Naturelles, on the 19th of December. At the beginning of the displays in Switzerland, M. Forel ascribed them to those causes which produce the ordinary after-glow so beautifully visible in mountainous countries, and at first he considered that the meteorological conditions were such as to favour this view. Further inquiry, however, he now states has made this hypothesis absolutely untenable. One of his arguments is that the glows which first appeared in November and then decreased to 3rd December, regained a maximum on the 24th and 25th. Now from the 22nd to 26th December, Switzerland was the centre of a maximum of atmospheric pressure, the barometer being higher there than in any of the surrounding countries. Exactly the opposite held in November, and this confirms him in the idea that meteorological factors alone do not suffice to explain the glows. He also describes the dates and tracts of the chromatic phenomena observed, and considers that their origination in Krakatoa is a simple and sufficient explanation. *La Nature* for the 29th ult. contains an interesting communication from M. Van Sandick, an Engineer des Ponts et Chaussées, at Pedang, who was an eye witness of the later stage of the eruption. He was on board the *Governor-General Loudin*, and was close to Krakatoa on August 26th. His communication is accompanied by a very detailed map, showing the changes which have supervened not only in the Straits themselves, but also on the neighbouring coasts of Java and Sumatra, but we shall return to this important letter.

The new observatory on the summit of Ben Nevis has been utilised for the collection of snow, with a view of determining whether or not it contains any dust particles. This has been forwarded to Mr. John Murray of the *Challenger* Commission by Mr. Omond the superintendent of the observatory. We may hope to hear soon whether the results are positive or negative on this special point of inquiry. We have to call attention to the important letter of Mr. Macpherson published below. We learn from the *Weekly British Colonist*, published at Victoria, British Columbia, that the sunsets made their appearance there on November 27th. Long after sunset the light in the sky became more fervent in colour, till at last the waters in the harbour and straits borrowed the splendid crimson. Darting and rapidly moving blood-red rays of light were seen shooting far into the sky, suggesting an aurora. A letter from St. Raphael, on the shores of the bay of San Francisco, dated December 4th, refers to the magnificent sunrises and sunsets. The date of their commencement is not stated. From Kiakhta, on the Mongolian frontier, we learn that the glows there began on December 11th, and terminated on the 25th.

The glows were seen some time before November 6th at Kalim Pong, twenty miles north-west of Darjeeling.

We have received the following further communications on this subject:—

THE body of evidence now brought in from all parts of the world must, I think, by this time have convinced Mr. Piazzi Smyth that the late sunrises and sunsets do need some explanation, more particular than he was willing to give them. With your leave I should like to point out from my own observations and those of others that, "given a clear sky" and the other conditions put by Mr. Smyth, the sunrises and sunsets of other days, however bright and beautiful, have *not* given any such effects as were witnessed, to take an instance, here on Sunday night, December 16th. I shall speak chiefly of the sunsets.

(1.) *These sunsets differ from others, first in their time and their place or quarter.* Sunset proper is, I suppose, the few minutes between the first dipping and the last disappearance of the sun's disk below the true horizen; the pageant or phenomena we call sunset, however, includes a great deal that goes on before and after this. The remarkable and specific features of the late sunsets have not been before or at sunset proper; they have been after-glows, and have lasted long, very long, after. To take instances from your number of the 13th ult., Mr. F. A. R. Russell notices that on November 28th, the sun having set at 3.55, one after-glow lasted till 5.10, and was then succeeded by another "reaching high above the horizon." The day before he mentions the after-glow as lasting to 5.20. On the 29th a "foreglow" is reported as seen in London from 5.30 to 7.30, that is more than two hours before sunrise, which was at 7.43. On December 1st, sunset being at 3.53, Mr. Russell observed an after-glow till 5.35; on December 4th the first dawn at 6.5, the sun rising at 7.50; the next day dawn at the same time, sunrise 7.51; that evening, sunset being at 3.50, he observed not a glow only but "spokes of rays from the glowing bank" at 4.45, that is to say, sunbeams, visible in the shape of sunbeams, 55 minutes after sunset. Mr. Johnston-Lavis speaks of the after-glow at Naples as *at a maximum* an hour after sunset. Here at Stonyhurst on December 16th, the sun having set at 3.49, the glow was observed till 5.50. Now winter dawns and after-glows do not last from an hour to two hours, and still less so day after day, as these have done. The recent sunrises and sunsets then differ from others in duration.

They differ also in the quarter of the heavens where they are seen. The after-glows are not low lingering slips of light skirting the horizon, but high up in the sky, sometimes in the zenith.

I have further remarked that the deepest of the after-glow is in the south, whereas the sun below the horizon is then northing. I see that other observers take notice of the same.

(2) *They differ in their periodic action or behaviour.* The flushes of crimson and other colours after ordinary sunsets are irregular, not the same nor at the same time for two days together; for they depend upon the accidental shapes and sizes and densities of the cloud-banks or vapour-banks the sun is entering or freeing himself from, which vary and can never be alike from day to day. But these glows or flushes are noticed to be periodic before sunrise and after sunset. Mr. Russell furnishes exact estimates of the intervals of time, which he finds to be the same day after day.

(3) *They differ in the nature of the glow, which is both intense and lustreless*, and that both in the sky and on the earth. The glow is intense, this is what strikes every one; it has prolonged the daylight, and optically changed the season; it bathes the whole sky, it is mistaken for the reflection of a great fire; at the sundown itself and southwards from that on December 4, I took a note of it as more like inflamed flesh than the lucid reds of ordinary sunsets. On the same evening the fields facing west glowed as if overlaid with yellow wax.

But it is also lustreless. A bright sunset lines the clouds so that their brims look like gold, brass, bronze, or steel. It fetches out those dazzling flecks and spangles which people call fish-scales. It gives to a mackerel or dappled cloudrack the appearance of quilted crimson silk, or a ploughed field glazed with crimson ice. These effects may have been seen in the late sunsets, but they are not the specific after-glow; that is, without gloss or lustre.

The two things together, that is intensity of light and want of lustre, give to objects on the earth the peculiar illumination which may be seen in studios and other well-like rooms, and which itself affects the practice of painters and may be seen in their works, notably Rembrandt's, disguising or feebly showing the outlines and distinctions of things, but fetching out white surfaces and coloured stuffs with a rich and inward and seemingly self-luminous glow.

(4) *They differ in the regularity of their colouring.* Four colours in particular have been noticeable in these after-glows, and in a fixed order of time and place—orange, lowest and nearest the sundown; above this, and broader, green; above this, broader still, a variable red, ending in being crimson; above this a faint lilac. The lilac disappears; the green deepens, spreads, and encroaches on the orange; and the red deepens, spreads, and encroaches on the green, till at last one red, varying downwards from crimson to scarlet or orange fills the west and south. The four colours I have named are mentioned in Lieut. G. N. Bittleston's letter from Umballa: "The sun goes down as usual and it gets nearly dark, and then a bright red and yellow and green and purple blaze comes in the sky and makes it lighter again." I suppose the yellow here spoken of to be an orange yellow, and the purple to be what I have above called lilac.

Ordinary sunsets have not this order; this, so to say, fixed and limited palette. The green in particular, is low down when it appears. There is often a trace of olive between the sundown and the higher blue sky, but it never develops, that I remember, into a fresh green.

(5) *They differ in the colours themselves, which are impure and not of the spectrum.* The first orange and the last crimson flush are perhaps pure, or nearly so, but the two most remarkable glows, the green and the red, are not. The green is between an apple-green or pea-green (which are pure greens) and an olive (which is a tertiary colour): it is vivid and beautiful, but not pure. The red is very impure, and not evenly laid on. On the 4th it appeared brown, like a strong light behind tortoiseshell, or Derbyshire alabaster. It has been well compared to the colour of incandescent iron. Sometimes it appears like a mixture of chalk with sand and muddy earths. The pigments for it would be ochre and Indian red.

Now the yellows, oranges, crimsons, purples, and greens of bright sunsets are beautifully pure. Tertiary colours may of course also be found in certain cases and places.

(6) *They differ in the texture of the coloured surfaces*, which are neither distinct cloud of recognised make nor yet translucent mediums. Mr. Russell's observations should here be read. I have further noticed streamers, fine ribbing or mackerelling, and other more curious textures, the colour varying with the texture.

In ordinary sunsets the yellows and greens and the lower reds look like glass, or coloured liquids, as pure as the blue. Other colours, or these in other parts, are distinct flushes or illuminations of cloud or landscape.

I subjoin an account of the sunset of the 16th, which was here very remarkable, from my own observations and those of one of the observatory staff.

A bright glow had been round the sun all day and became more remarkable towards sunset. It then had a silvery or steely look, with soft radiating streamers and little colour; its shape was mainly elliptical, the slightly longer axis being vertical; the size about 20° from the sun each way. There was a pale gold colour, brightening and fading by turns for ten minutes as the sun went down. After the sunset the horizon was, by 4.10, lined a long way by a glowing tawny light, not very pure in colour and distinctly textured in hummocks, bodies like a shoal of dolphins, or in what are called gadroons, or as the Japanese conventionally represent waves. The glowing vapour above this was as yet colourless; then this took a beautiful olive or celadon green, not so vivid as the previous day's, and delicately fluted; the green belt was broader than the orange, and pressed down on and contracted it. Above the green in turn appeared a red glow, broader and burlier in make; it was softly brindled, and in the ribs or bars the colour was rosier, in the channels where the blue of the sky shone through it was a mallow colour. Above this was a vague lilac. The red was first noticed 45° above the horizon, and spokes or beams could be seen in it, compared by one beholder to a man's open hand. By 4.45 the red had driven out the green, and, fusing with the remains of the orange, reached the horizon. By that time the east, which had a rose tinge, became of a duller red, compared to sand: according to my observation, the ground of the sky in the east was green or else tawny, and the crimson only in the clouds. A great sheet of heavy dark cloud, with a reefed or puckered make, drew off the west in the course of the pageant: the edge of this and the smaller pellets of clond that filed across the bright field of the sundown caught a livid green. At 5 the red in the west was fainter, at 5.20 it became notably rosier and livelier; but it was never of a pure rose. A faint dusky blush was left as late as 5.30, or later. While these changes were going on in the sky, the landscape of Ribblesdale glowed with a frowning brown.

The two following observations seem to have to do with the same phenomena and their causes. For some weeks past on fine bright days, when the sun has been behind a big cloud and has sent up (perspectively speaking) the dark crown or paling of beams of shadow in such cases commonly to be seen, I have remarked, upon the ground of the sky, sometimes an amber, sometimes a soft rose colour, instead of the usual darkening of the blue. Also on moonlight nights, and particularly on December 14, a sort of brown or muddy cast, never before witnessed, has been seen by more than one observer, in the sky. GERARD HOPKINS

Stonyhurst College, December 21, 1883

THE remarkable phenomena after sunset which, according to NATURE, were seen in the second half of November in England, Italy, at the Cape, and a little earlier in many parts of Asia, could be observed almost all over Austria and Germany. I saw them myself in an especially distinct appearance here on November 22 and 29. Soon after sunset on November 22 (at 4.30 p.m.), a crimson glow was seen in the direction of south-west, and while everybody was supposing that some large printworks lying in that direction were on fire, the glow was getting more intense, and at 5 p.m. the whole of the western sky assumed a bluish purple hue which rose up to the zenith while the sun was sinking lower, so that the glow could be attributed only to an atmospheric phenomenon. About an hour after sunset the colour of the sky was almost violet, with which the phenomenon disappeared.

According to German papers, a phenomenon of this kind and intensity was never before observed in Central Europe. Dr. Assmann, director of the Meteorological Observatory, Madgeburg, attempts to explain these phenomena by the reflection of sunlight from the upper strata of our atmosphere, highly saturated with aqueous vapour, owing to its comparatively high temperature. The phenomenon could not be attributed to electrical causes, as at that time not the slightest magnetic disturbance could be observed at the Prague Observatory. In the spectrum of this light uncommonly strong "rain bands" were seen. As the sun was about $18\frac{3}{4}°$ below the horizon when the phenomena began (before sunrise) or ceased (after sunset), the reflection was calculated to have taken place at a height of about fifty English miles.

Does it not strike you that the glow was observed at earlier periods the more we advance towards the east—the source of the late Java eruptions? B. BRAUNER

Bohemian University, Prague, December 18, 1883

THE late splendid sunsets which have so vividly attracted the attention of men of science and of the general public were so remarkable and of so long a duration in the clear atmosphere of the Castilian tableland, where sunsets are usually dull, that they have not failed to impress observers with the notion that they were due to other causes than those of common atmospheric refraction and reflection.

When the phenomena had already lasted four or five days, I read Mr. Symons' letter, published in the *Times* of the 1st inst., and I thought that possibly evidence might be obtained towards the confirmation of this theory if the sediment of fresh-fallen snow was thoroughly investigated; for if the dust of Krakatoa was really reflecting in the higher regions of the atmosphere the sun's rays, some of it must necessarily be descending towards the earth.

Luckily on the 7th of this month, and when the phenomenon was at its height, and had already lasted for about eight days, there was a fall of snow at Madrid, of which I naturally profited, submitting it to a thorough investigation, the results of which, I think, will throw some light on so remarkable a phenomenon.

The snow analysed was obtained from what had fallen on some zinc plates before the exposed windows to the north of my house, which is situated at the extreme north end of the town, where there are no buildings facing it, and also from what my friend Dr. Francisco Quisoga gathered from the windows of his house, situated about a mile to the south-east of mine; and in both the same substances were found.

The snow yielded about a litre of water, which, when the sediment had collected, was decanted, and the solid part dried at a temperature below that of boiling water. The dry powder was then tested for magnetism and it was found to be extremely magnetic. It was then incinerated on platinum foil to a bright red heat so as to destroy organic substances, and the remaining dust was then submitted to microscopical investigation. The greater part of it is made up of what probably is the natural dust of the atmosphere of Madrid; of particles of mica, generally brown, and similar to that of the Guadarrama range, and in various states of decomposition, splinters of quartz and felspar, the greater part of it orthoclase, some small fragments of tourmaline, magnetic iron, and fragments of diatoms. Besides these mineral substances, which may probably be traced to the rocks forming the vicinity of the capital, some others were found for the presence of which it is difficult to account. The most remarkable are small particles of a foliated mineral of a yellowish colour, perceptibly dichroic, and which between crossed Nichols is extinguished when the cleavage traces are parallel to the principal section of the polarising Nichol; the interference colours being of bright blue, and red, and yellow colours. Treated by boiling hydrochloric acid for twenty minutes, not a trace of action was perceived. These characters are all referable to a rhombic pyroxene, and judging from its dichroism this substance may be taken for a hypersthene, which has besides a most striking resemblance to volcanic hypersthene. In addition to this mineral, small particles are found which appear to be referable to common pyroxene of a yellowish colour, of active action in polarised light, and the extinction not taking place parallel to what seem to be the edges of the prism. Besides these minerals some corpuscles are found of hardly any action on polarised light, and sometimes full of globular concretions and other kinds of microliths, which, if seen in products of a volcanic region, I would not hesitate in considering of volcanic origin.

These are the principal substances which an investigation of the sediment of the snow which fell in Madrid on the 7th inst. have revealed, and though I am far from asserting that what appears to be foreign to the atmosphere of this part of the world is referable to the dust of Krakatoa, if further analyses in other parts of the world should show these same substances floating in the atmosphere, there would be powerful reasons for inferring that the gorgeous sunsets of the past months have been brought about in consequence of that stupendous display of the volcanic forces of our globe.

It is already a remarkable coincidence that hypersthene should have been found both by MM. Daubrée and Renard in their respective analyses of the ashes collected in the vicinity of Krakatoa. JOSEPH MACPHERSON

Madrid, December 22, 1883

COMPLYING with the request contained in your "Notes" of December 13 (p. 157), I would say that the appearances, already fully described by so many of your correspondents, commenced here on December 1. On that day I made an entry in my note-book as follows:—"Perfectly calm at sunset, with a light haze of a rose tint rolling away from overhead towards the west-south-west horizon. The colours of the sky were a very pale green, red, gold, and pink; and, as the light faded away, the south-west was one mass of deep rich red. The crescent moon (a little over eighteen days old) in the refractor was of a pale green colour, and the bright limb seemed to extend to an extraordinary distance round the dark body. Barometer falling."

Again: "December 2.—Sky clouded over by 1 p.m. Sunset, as seen between breaks in the clouds, was again of a deep rich red. Barometer steady."

"December 3.—Rainy and very dull. Barometer steady."

"December 4.—Sunset, as seen through the clouds along the horizon, was again of a deep red colour, gradually shading off into a pale rose tint towards the zenith. The moon, Fomalhaut, and Vega seemed to float in a pale rose sea; whilst thin fleecy clouds as they drifted across the moon's face were of a beautiful pale green. This appearance—as did that on the 1st—lasted for about an hour and a quarter after sunset; the rest of the sky being covered with clouds, some faintly reflecting the various tints. Barometer falling."

I should not omit to mention that the sunrises were also, more or less, of similar character. Since the 4th we have had very bad weather; gales from both north and south, heavy rains, and snow. Yet the sky, when occasionally glimpsed at sunset, seems to bear traces of the same appearances. W. E. J.

Constantinople, December 21, 1883

IN addition to the remarkable sunsets which have led to such a large amount of correspondence in NATURE and elsewhere, there is another and possibly a related phenomenon to which my attention has been directed during the last few weeks. From country friends I learn that the nights, in the absence of the moon, and even when cloudy, have been remarkably light for the time of year. I cannot profess to have witnessed this phenomenon myself, living as I do in the midst of London, where the perpetual glare of gas renders any satisfactory estimate of the atmospheric luminosity quite hopeless. It would be interesting, however, to learn whether other observers more favourably located have noticed this effect. It occurred to me that the phenomenon might perhaps be connected with the volcanic dust theory of the sunsets, being, in fact, a result of the slight phosphorescence of this dust. Whether the latter exhibits any degree of phosphorescence could be readily determined by those who are fortunate enough to possess a specimen, by means of Becquerel's phosphoroscope.

R. MELDOLA

21, John Street, Bedford Row, W.C., Dec. 31, 1883

IN corroboration of what Messrs. Beyerinck and Van Dam noticed at Wageningen in connection with the late storm, I write to tell you that on the morning of December 12, after the heavy rain which accompanied the gale had ceased, the windows of my house, which is isolated and exposed, were covered with a grayish sediment, just as your correspondents describe it. It will be interesting, now that attention has been drawn to the fact, to know if the phenomenon, the result no doubt of dust brought down by the rain, has been observed elsewhere.

F. M. BURTON

Highfield, Gainsborough, December 24, 1883

I SUBMIT to you two slides of dust from windows, deposited during the storm of December 12. When the contained salt crystals are dissolved by adding distilled water, the appearance much resembles that recorded in NATURE of December 20. The material, scraped from windows cleaned just before the storm, where the original drop-marks are still unaltered, was put on the cleaned slides, and a drop of distilled water added. Should my surmise be confirmed, and any of your readers desire to have specimen slides, I would forward a limited number on receipt of sixpence each to cover postage and trouble. Descriptions I have received from America, either in letters or newspaper cuttings, show an identical sequence of appearances. At Poughkeepsie, on the Hudson, the fire engines were called out on the morning of November 27, and "this spectacle has been witnessed every clear evening for several days past, generally between a quarter past five and six o'clock." A letter from Dorset, Vermont, November 29, describes "a very unusual exhibition in the skies for the past three or four evenings. It has been clear, and the colouring intense, from flame to a delicate pink, and the clouds off at a distance would look light green. . . . It gave an impression of an intense fire the other side of the West Mountains, and colouring the entire sky."

J. EDMUND CLARK

York, December 22, 1883

THE accompanying extract may be of service to you. Sapporo is in the northernmost island of Japan (Yeso), in lat. 43° N., and long. (*circa*) 141° E. As the telegraph ramifies through all parts of Japan, it is improbable that any considerable local eruption would have taken place to account for the phenomenon without news of it having also reached the *Official Gazette*.

ROBERT BEADON

11, Lee Park, Lee, Kent, December 14, 1883

Extract from *Japan Weekly Mail* (published in Yokohama) of October 20, 1883. (The *Official Gazette* is the Government gazette published in Japanese.)—"The *Official Gazette* states that, since the 13th inst., a constant haze has pervaded the atmosphere of Sapporo, and that the sun and moon are of a blood-red colour. Clouds of ashes fall continuously. The phenomenon is ascribed to some volcanic eruption."

NOTES

PROF. OWEN has received the honour of K.C.B. as an acknowledgment of his eminent services for sixty years to science and the public interests.

PROF. W. H. MACKINTOSH has been elected to the Professorship of Comparative Anatomy in Trinity College, Dublin, vice Prof. Macalister, F.R.S., who resigned on his appointment to the Anatomy Chair at Cambridge.

BY the death of the well-known mathematician, the Rev. W. Roberts, M.A., the Rev. Richard Townsend, M.A., F.R.S., becomes a Senior Fellow of Trinity College, Dublin, thereby vacating the Professorship of Natural Philosophy held by him since 1870.

THE vacancy in the Professorship of Geology and Mineralogy in the University of Dublin has been filled by the election of Prof. Sollas, of University College, Bristol. This appointment will give great satisfaction, and will afford Mr. Sollas large opportunities for palæontological research; the large collections of fossil plants and vertebrates in the museum in Dublin remaining to this day almost unknown.

THE Swedish Government intend to establish a botanico-physiological station in the north of Sweden for the study of the flora and the diseases of the crops in that part of the country.

THE Finnish Government have ordered a steamer to be specially built in Sweden for the scientific researches about to be prosecuted in the Baltic.

M. HOUZEAU, who was only recently appointed director of the Brussels Observatory, has resigned his post, and it is reported that M. de Konkolly of Gzalla Observatory, Hungary, will succeed him.

PROF. MAURICE LEVY has been nominated member of the Paris Academy of Sciences in the Section of Mechanics.

THE Prince of Wales, as President of the Society of Arts, has transmitted to Lady Siemens the resolution passed after the death of Sir William Siemens, by the Council of that Society, and in doing so has expressed his own appreciation of Sir William Siemens's labours.

SCIENCE had quite a field-day in Perth on December 20, when the Natural History Society of the Fair City formally opened its museum. Prof. J. Geikie of Edinburgh, who was for some time president of the Society, opened the proceeding with an address in which he pointed out what such a local museum should be. Other speakers followed, and from the 20th to the 23rd was an almost continuous *conversazione*, in which exhibitions, demonstrations, and lectures were given. The electric light played a prominent part, and the objects brought together for the instruction and enjoyment of the many visitors represented all departments of science. The enterprise of the Perthshire Society is exceptional, and they have reason to be proud of their museum, reading, lecture, and other rooms, all of which, we have no doubt, will be put to excellent practical uses.

THE meteorological observations taken during October, 1883, at St. Ignatius' College, Malta, by the Rev. James Scoles, S.J., have been received. For the month the means were—pressure, 30·283 inches; temperature, 67°·98; daily range, 10°·2; elastic force of vapour, 0·498 inch, and humidity, 76; rainfall, 2·67 inches, and days of rain, 12; velocity of wind per hour, 8¼ miles; sky, a third covered with cloud; temperature of sea, 72°·0, with a monthly range of 4°·0; and thunderstorms and other electrical phenomena on the 4th, 10th, 11th, 12th, 13th, 15th, 26th, and 30th. Atmospheric pressure was thus fully a fourth of an inch below the mean, temperature 3°·4 lower than usual, and rainfall about half an inch less. This Society has peculiar facilities for prosecuting meteorological and other researches through its widely scattered seminaries and colleges, and we have the greatest pleasure in noting the increasing readiness with which its services are given to science.

MR. H. H. JOHNSTON will give a discourse on "Kilimanjaro, the snow-clad Mountain of Equatorial Africa," at the Royal Institution, on Friday evening, January 25. Prof. Bonney's discourse on "The Building of the Alps," announced for that evening will be given on April 4.

HERR STEINEGER has been so fortunate as to secure eleven crania and numerous bones of the extinct sea-cow, *Rhytina stelleri*, which have been forwarded to the Smithsonian Institution at Washington.

A SPLENDID meteor was seen at Frankfort-on-the-Maine on December 8 at 6.45 a.m. It moved from west to east, and illuminated the whole neighbourhood.

A *Times* correspondent writes from Iceland that reports of a volcanic eruption in the interior were current last year, and were founded on peculiar appearances of the sky, and especially on the observation from some of the remote inland farms of columns of smoke or vapour rising in the far distance. Nothing definite has, however, been ascertained as to these phenomena. An unusually large number of scientific men, geologists, botanists, and philologists, chiefly German and Swedish, have this year visited Iceland and investigated its structure, flora, and language; and at present Prof. Sophus Tromholt, well known in scientific circles by his researches as to the aurora borealis, is pursuing these investigations here, and intends to remain all the winter, as, from the clearness of the atmosphere and the frequency and brilliancy of the aurora, Iceland is exceedingly well suited for his observations.

THE extensive collections of American Coleoptera made by the late Dr. J. L. LeConte, containing an immense number of original types, become the property of the Museum of Comparative Zoology of Cambridge, Mass.

THE French Société des Électriciens has completed its arrangements, and has been divided into six sections:—Theoretical electricity, M. Marie Davy president; Dynamo-electrical machinery, transmission of force to a distance, distribution of energy, M. Tresca president; Electric lighting, M. Du Moncel president; Telegraphy and telephony, M. Blavier president; Electro-chemistry and electrotherapy, M. Jamin president.

WHEN Arago was director of the Observatory of Paris, the dotation of this establishment was less than 4000*l*. a year. This sum was greatly increased when Leverrier was appointed by Napoleon III., and before his death it had reached 10,000*l*. Now the sum allotted is about 16,000*l*., although the meteorological department has been set apart as a special service.

THE Italian Geographical Society awards its great gold medal to Count Pietro Antonelli, in consideration of the important results of his last journey to Shoa.

FROM advanced sheets of the *Proceedings of the Anthropological Society of Washington*, Col. F. A. Seely of the United States Patent Office, we learn from *Science*, publishes a pamphlet entitled "An Inquiry into the Origin of Invention." The author is accustomed, day by day, as new claims for patents come before him, to eliminate the successive steps in the classes of machinery until he reaches the fundamental idea. This is the plan pursued in tracing backward the whole subject of invention to its sources in the mind of primitive man. The subject is illustrated, first, by the story of the steam-engine, and then by the examination of the bow and arrow and other implements of the lower races. The author rejects Prof. Gaudry's Dryopithecus, and affirms, "Obviously, archæology can find no trace of a remoter age than that of stone; but I mistrust that the thoughtful anthropologist will accept the evidence of earlier ages, one of which, taking one of its perishable materials as the type of all, we may call the age of wood. Still farther back must lie an age, as indefinite in duration as any, when man existed in his rudest condition, without arts of any kind, except such as he employed in common with lower animals; and this is the true primitive period."

WE have received the report for the years 1880 and 1881 of the administration of the artistic and scientific collections in the Royal Museums of Dresden. The Zoological and Anthropological Museum was visited by 61,129 persons in 1880, and by 65,455 in 1881. An index to Reichenbach's ornithological works has been prepared by the director, Dr. A. B. Meyer, who has also issued an important work on the picture-writings of the Eastern Archipelago and Pacific Islands. The staff of this museum now consists of the Director, Th. Kirsch, curator, L. Römer and J. C. G. Wilhelm, first and second conservators, C. A. Kippe, preparer of specimens, a scientific assistant, and two attendants. The zoological and anthropological collections were enriched in the years 1880 and 1881 by 2242 specimens of the higher animals, and 17,753 of insects, by 237 anthropological and 1351 ethnographic objects, including 61 crania and 56 photographs and drawings of human types from various quarters. The library attached to this department was increased by 332 works, including donations from the British Museum, Smithsonian, and other sources. The systematic catalogue of the fishes was completed in three volumes, with alphabetical index of the 294 genera, 726 species, and 2901 specimens contained in the collection. The nests, to the number of 800, were also rearranged and catalogued, and progress was made with the catalogues of the birds (from No. 1688 to 2948) and insects (Hymenoptera concluded, Diptera thoroughly revised, of Coleoptera three families arranged and catalogued).

MESSRS. BAILLIÈRE AND CO. of Paris have issued the first number of a new scientific weekly, *Science et Nature*, profusely illustrated.

M. ÉD. MAILLY has brought out, in two volumes, a "Histoire de l'Académie Impérial et Royal de Bruxelles," from which so much good work has emanated. The history abounds in interest. F. Hayez of Brussels is the publisher.

SPAIN does seem to be progressing in the right direction. We have the second volume of Mr. F. Gillman's very useful and carefully compiled "Enciclopedia-Popular Illustrada" (Madrid), with a large atlas of plates. Also the first number of *La Industria Ibérica*, a weekly paper devoted to the industry and science of the whole peninsula, well printed, and, to judge from the first number, judiciously edited.

MESSRS. CHARLES GRIFFIN AND CO. announce the following scientific publications as forthcoming:—"A Manual of Geology," by Robert Etheridge, F.R.S., and Prof. H. G. Seeley, F.R.S.; "A Manual of Chemistry," by Prof. Dupré, F.R.S., and Dr. H. Wilson Hake; "A Manual of Botany: the Morphology, Physiology, and Classification of Plants, for the Use of Students," by Prof. W. R. M'Nab; "A Pocket-book of Electrical Rules and Tables, for the Use of Electricians and Engineers," by John Munro, C.E., and Andrew Jamieson, C.E., F.R.S.E.

THE additions to the Zoological Society's Gardens during the past week include a Rhesus Monkey (*Macacus rhesus*) from India, presented by Miss P. Crabtree; a Campbell's Monkey (*Cercopithecus campbelli*) from West Africa, presented by Mr. Walter van Weede; an Alligator (*Alligator mississippiensis*) from the Mississippi, presented by Mr. Thick; a Ring-tailed Coati (*Nasua rufa*) from South America, deposited.

PHYSICAL NOTES

M. E. REYNIER has described, in *l'Électricien*, a research made by him on the maxima and minima of electromotive force of certain batteries in which polarisation takes place. These he calls "single-electrolyte" batteries, instead of "single-fluid" batteries, following a suggestion of the late M. Niaudet. The difference consists in the relative size of the electrodes. For example, in the case of a zinc-copper cell containing a single electrolytic fluid, the maximum cell is made with a kathode of sheet-copper folded and curved, presenting 300 times as much surface as the thin copper rod which serves as anode, whilst in the minimum cell the proportion is reversed, so that the polarisation at the surface of the copper attains at once its maximum value. The value of the E.M.F. of the cells when filled with dilute sulphuric acid, and having the zinc amalgamated, was 1·072 volts maximum, and 0·272 volts minimum. Many other electrolytes were examined by M. Reynier. The electromotive force was measured upon a galvanometer of high resistance.

M. REYNIER has suggested a modification of his maximum cell to serve as a standard of electromotive force—namely, a cell having a very large copper electrode, and a very small amalgamated zinc electrode, immersed in a solution of sea-salt. According to M. Reynier, this battery has an E.M.F. of 0·82 volts, and maintains this value within 1 per cent. even when the circuit was loosed for two hours through a resistance of 820 ohms. M. Reynier prefers this combination to one containing sulphate of zinc in solution, because of the liability of the latter salt to contain free acid.

M. HENRI BECQUEREL has been pursuing his researches upon the infra-red rays of the spectrum. For the investigation of this region there are four methods, the first of them involving the use of a line-thermopile and a rock-salt prism; the second, Abney's photographic method; the third, Langley's method, with bolometer and a reflecting diffraction grating; the fourth, that of Becquerel, depending upon the discovery that the infra-red rays have the effect of extinguishing the glow of a phosphorescent body exposed previously to ultra-violet rays. M. Becquerel finds that water, for example, gives in the region to which this method is applicable three well marked absorption-bands, having wave-lengths respectively of 930, 1080, and 1230.

THE newest result of Becquerel's researches is worth more than passing mention. He finds that there exist in this wholly invisible region of the spectrum bright-line spectra—equally invisible, of course—just as in the visible parts of the spectrum, observable in the radiations of hot vapours. Thus, incandescent sodium vapour prints upon the previously "insolated" phosphorescent substance two well-marked lines (wave-lengths 819 and 1098), corresponding to two bright lines hitherto unknown. The extent of the region which is capable of being explored by this novel process is from wave-length 760 to 1300, or exceeding in extent that of the whole of the visible and ultra-violet rays.

AN interesting experiment is described in the *Zeitschrift des elektrotechnischen Vereins*, in Vienna, by Prof. von Waltenhofen, made by means of Noe's thermo-electric generators. If a current from a voltaic battery has been sent for a few moments through one of these generators, it is capable of yielding a discharge like a secondary battery. This effect is so far a mere repetition of a well-known experiment of Peltier, and is due to the change of temperature at the junction, called the Peltier effect. But von Waltenhofen observes that the effects are different according to the sense of the charging current. In one case, with increasing charging currents the discharge currents also increased, and were always in the opposite sense to that of the charging current. But when the charging current was reversed, it was found that with increasing charging currents the discharge currents at first increase, then attain a maximum, then decrease to zero, then actually recommence in the converse sense, namely, in the same sense as that of the charging current. Prof. von Waltenhofen is disposed to attribute this anamalous result to the lack of symmetry in the disposition of the alternate solderings of the generators, and to their alternately unequal resistance causing alternately unequal developments of heat due to resistance.

IN proof of the law of proportion between the thickness of a square vibrating plate and its pitch, Dr. Elsas gives the following neat experiment. Let three plates be cut from the same sheet of material, of the same size and form. Cement two of these together so as to produce a plate of double thickness. Then, on exciting the single plate and the double plate by communicating to them respectively the vibrations of two tuning forks whose pitches are as 1 : 2, the plates will be excited in identical manners, as will be seen by dusting sand upon them, the clang-figures being identical.

LORD RAYLEIGH has reprinted for private circulation in pamphlet form several of his most valuable optical papers, including those on the manufacture, reproduction by photography, and theory, of diffraction-gratings, and those on colour-mixtures.

LORD RAYLEIGH has also reprinted some of his papers on electricity and on absolute pitch, from NATURE and from the Reports of the British Association, in a convenient pamphlet form.

THE question whether condensation of steam is a cause of electrification has been examined afresh by S. Kalischer in the Physical Laboratory at Berlin. According to the views of Faraday, this is a cause of electrification, and upon the alleged phenomenon Prof. Spring has founded a theory of the origin of thunderstorms. Landerer thought he had heard sounds in the telephone due to condensation of moisture on the line wires. Kalischer has in vain repeated the experiment. He has also examined, by means of the quadrant electrometer, whether any such electrification could be observed from the deposit of moisture upon the surface of a vessel containing ice or some artificial cooling mixture. The whole of the results were negative.

AMONGST the many recent suggestions for primary batteries is one due to MM. Lalande and Chaperon, in which oxide of copper is used as a depolarising agent. The oxide, in powder, is placed in or on a sheet of copper or iron. The positive element is zinc, and the exciting liquid caustic potash. A zincate of potash is formed by the solution of the zinc. The cell is absolutely inactive when the circuit is open. When closed, the current is remarkably constant. According to Hospitalier, the electromotive force is 0·98 volt. It must of course be closed from the air, to prevent absorption of carbonic acid by the potash. The reduced copper is reoxidised by simple exposure to the air.

IN a series of studies on the copper voltameter, published in the *Repertorium der Physik* by Dr. H. Hammerl, the following conclusions are formulated:—1. The material condition of the surface of the electrode, that is to say, whether it is covered with a bright copper film or not, has no influence on the amount of the deposit. 2. The changes of concentration of the copper solution, brought about in the voltameter by the current itself, cannot be sufficiently prevented by stirring. 3. Heating the fluid to boiling causes the deposit to come down almost completely in the state of cuprous oxide: it is partially oxidised even at temperatures between 40° and 60° C. 4. The greatest permissible strength of current, for which the deposit may be safely assumed to be a measure of the current, is about 7 amperes per square decimetre of the cathode surface.

THE EVIDENCE FOR EVOLUTION IN THE HISTORY OF THE EXTINCT MAMMALIA[1]

THE subject to which I wish to call your attention this morning requires neither preface nor apology, as it is one with the discussion of which you are perfectly familiar. My object in bringing it before the general session of the Association was in view of the fact that you were all familiar with it in a general way, and that it probably interests the members of sections which do not pursue the special branch to which it refers, as well as those which do; also, since it has been brought before us in various public addresses for many years during the meetings of this Association, I thought it might be well to be introduced at this meeting of this Association, in order that we might not omit to have all the sides of this interesting question presented.

The interests which are involved in it are large: they are chiefly, however, of a mental and metaphysical character; they do not refer so much to industrial and practical interests, nor do they involve questions of applied science. They involve, however, questions of opinion, questions of belief, questions which affect human happiness, I venture to say, even more than questions of applied science; certainly, which affect the happiness of the higher grades of men and women more than food or clothing, because they relate to the states of our mind, explaining as they do the reasons of our relations to our fellow-beings and to all things by which we are surrounded, and the general system of the forces by which we are surrounded. So it has always appeared to me: hence I have selected the department of biology, and have taken a great interest in this aspect of it.

[1] A lecture by Prof. E. D. Cope of Philadelphia, given in genera session before the American Association for Advancement of Science at Minneapolis, August 20, 1883. Stenographically reported for *Science*.

The doctrine of evolution, as taught by the biologists of to-day, has several stages as grounds or parts of its presentation. First, the foundation principle is this: That the species of animals and of plants, the species of organic beings, as well as the various natural divisions into which these organic beings fall, have not always been as we see them to-day, but they have been produced by a process of change which has progressed from age to age through the influence of natural laws; that, therefore, the species which now exist are the descendants of other species which have existed heretofore, by the ordinary processes of reproduction; and that all the various structures of organic beings which make them what they are, and which compel them to act as they now act, are the result of gradual or sudden modifications and changes during the periods of geologic time. That is the first phase or aspect which meets the naturalist or biologist.

Another phase of the question relates to the origin itself of that life which is supposed to inhabit or possess organic beings. There is an hypothesis of evolution which derives this life from no-life, which derives vitality from non-vitality. That is another branch of the subject, to which I cannot devote much attention to-day. There is still another department of the subject, which relates to the origin of mind, and which derives the mental organisation of the higher animals, especially of man, from pre-existent types of mental organisation. This gives us a genealogy of mind, a history of the production or creation of mind, as it is now presented in its more complex aspects as a function of the human brain. This aspect of the subject is, of course, interesting, and upon that I can touch with more confidence than upon the question of the origin of life.

Coming now to the question of the origin of structures, we have by this time accumulated a vast number of facts which have been collated by laborious and faithful workers, in many countries and during many years; so that we can speak with a good deal of confidence on this subject also. As to the phenomena which meet the student of zoology and botany at every turn, I would merely repeat what every one knows—and I beg pardon of my biological friends for telling them a few well-known truths, for there may be those present who are not in the Biological Section—that the phenomena which meet the student of biology come under two leading classes: the one is the remarkable fidelity of species in reproducing their like. "Like produces like," is the old theorem, and is true in a great many cases; just as coins are struck from the die, just as castings are turned out from a common mould. It is one of the most wonderful phenomena of nature, that such complex organisms, consisting of so many parts, should be repeated from age to age, and from generation to generation, with such surprising fidelity and precision. This fact is the first that strikes the student of these sciences. The general impression of the ordinary person would be that these things must continue unchanged. When I began to study zoology and botany, I was remarkably surprised to find there was a science of which I had no conception, and that was this remarkable reproduction of types one after another in succession. After a man has had this idea thoroughly assimilated by his honest and conscientious studies, he will be again struck with another class of facts. He will find, not unfrequently, that this doctrine does not apply. He will find a series of facts which show that many individuals fail to coincide with their fellows precisely, the most remarkable variations and the most remarkable half-way attitudes and double-sided aspects occurring; and he will come to the conclusion, sooner or later, that like does not produce like with the same precision and fidelity with which he had supposed it did. So that we have these two classes of facts,—the one relating to, and expressing, the law of heredity; the other, which expresses the law of metamorphosis. I should not like to say which class of facts is the most numerously presented to the student. In the present fauna we find many groups of species and varieties before us; but how many species we have, how many genera we have, and families, we cannot definitely state. The more precise and exact a person is in his definition and in his analysis, the more definite his science becomes, and the more precise and scientific his work. It is a case of analysis and forms. What the scales are to the chemist and the physicist, the rule and measure are to the biologist. It is a question of dimension, it is a question of length and breadth and thickness, a question of curves, a question of crooked shapes or simple shapes,—rarely simple shapes, mostly crooked, generally bilateral. It requires that one should have a mechanical eye, and should have also something of an artistic eye to appreciate these forms, to measure them, and to be able to compare and weigh them.

Now, when we come to arrange our shapes and our measurements, we find, as I said before, a certain number of identities, and a certain number of variations. This question of variation is so common and so remarkable, that it becomes perfectly evident to the specialist in each department that like does not at all times produce like. It is perfectly clear, and I will venture the assertion that nearly all the biologists in this room will bear me witness, that variability is practically unlimited in its range, unlimited in the number of its examples, unlimited in the degree to which it extends. That is to say, the species vary by failing to retain certain characteristics, and generic and other characters are found to be absent or present in accordance with some law to be discussed further on.

I believe that this is the simplest mode of stating and explaining the law of variation: that some forms acquire something which their parents do not possess; and that those which acquire something additional have to pass through more numerous stages than those which have not acquired so much had themselves passed through.

Of course we are met with the opposite side of the case,—this law of heredity. We are told that the facts there are not accounted for in that way; that we cannot pass from one class of facts to the other class of facts; that what we find in one class is not applicable to the other. Here is a question of rational processes, of ordinary reason. If the rules of chemistry are true in America, I imagine they are true in Australia and Africa, although I have not been there to see. If the law of gravitation is effective here, I do not need to go to Australia or New Zealand to ascertain whether it is true there. So, if we find in a group of animals a law sufficient to account for their creation, it is not necessary to know that others of their relatives have gone through a similar process. I am willing to allow the ordinary practical law of induction, the practical law of inference, to carry me over these gaps, over these interruptions. And I state the case in that way, because this is just where some people differ from me, and that is just where I say the simple question of rationality comes in. I cannot believe that nature's laws are so dissimilar, so irregular, so inexact, that those which we can see and understand in one place are not true in another; and that the question of geological likelihood is similar to the question of geographical likelihood. If a given process is true in one of the geological periods it is true in another; if it is true in one part of the world it is true in another; because I find interruptions in the series here, it does not follow that there need be interruptions clear through from age to age. The assumption is on the side of that man who asserts that transitions have not taken place between forms which are now distinct.

We are told that we find no sort of evidence of that transition in past geological periods; we are assured that such changes have not taken place; we are even assured that no such sign of such transition from one species to another has ever been observed,—a most astonishing assertion to make *to* a biologist, or *by* a biologist; and such persons have even the temerity to cite special cases, as between the wolf and the dog. Many of our domestic dogs are nothing but wolves, which have been modified by the hand of man to a very slight extent indeed. Many dogs, in fact nearly all dogs, are descendants of wild species of various countries, and are but slightly modified.

To take the question of the definition of species. Supposing we have several species well defined, say four or five. In the process of investigation we obtain a larger number of individuals, many of which betray characters which invalidate the definitions. It becomes necessary to unite the four or five species into one. And so then, because our system requires that we shall have accurate definitions (the whole basis of the system is definitions—you know the very comprehension of the subject requires definitions), we throw them all together, because we cannot define all the various special forms as we did before, until we have but one species. And the critic of the view of evolution tells us, "I told you so! There is but one species, after all. There is no such thing as connection between species; you never will find it." Now, how many discoveries of this kind will be necessary to convince the world that there are connections between species? How long are we to go on finding connecting links, and putting them together, as we have to do for the sake of the definition, and then be told that we have nevertheless no intermediate forms between species? The matter is too plain for further comment. We throw them together simply because our definitions require it. If we knew all the known individuals which have lived, we should have no species, we should have no genera. That is all there is of it. It is simply a question of a universal accretion of material and the collection of information. I do not believe that the well-defined groups will be found to run together, as we call it, in any one geological period, certainly in no one recent period. We recognise, however, that they diverge to a wonderful extent; one group has diverged at one period, and another one has become diversified in a different period; and so each one has its history, some beginning farther back than others, some reaching far back beyond the very beginning of the time when fossils could be preserved. I call attention to this view because it is a very easy matter for us to use words for the purpose of confusing the mind; for, next to the power of language to express clear ideas, is its power of expressing no ideas at all. As we all know, we can *say* many things which we cannot *think*. It is a very easy thing to say twice two is equal to six, but it is impossible to think it.

I would cite what I mean by variations of species in one of its phases: I would just mention a genus of snakes, Ophibolus, which is found in the United States. If we take the species of this snake genus as found in the Northern States, we have a good many species well defined. If we go to the Gulf States and examine our material, we see we have certain other species well defined, and they are very nicely defined and distinguished. If now we go to the Pacific coast, to Arizona and New Mexico, we shall find another set of species well defined indeed. If we take all these different types of our specimens of different localities together, our species, as the Germans say, all tumble together; definitions disappear, and we have to recognise, out of the preliminary list of thirteen or fourteen, only four or five. That is simply a case of the kind of fact with which every biologist is perfectly familiar.

When we come to the history of the extinct forms of life, it is perfectly true then that we cannot observe the process of descent in actual operation, because, forsooth, fossils are necessarily dead. We cannot perceive any activities because fossils have ceased to act. But if this doctrine be true we should get the series, if there be such a thing; and we do, as a matter of fact, find longer or shorter series of structures, series of organisms proceeding from one thing into another form, which are exactly as they ought to be, if this process of development by descent had taken place.

I am careful to say this, because it is literally true, as we all must admit, that the system must fall into some kind of order or other. You could not collect bottles, you could not collect old shoes, but you could make some kind of a serial order of them. There are no doubt characters, by which such and such shoes could be distinguished from other shoes, these bottles from other bottles; but it is also true that we have, in recent forms of life in zoology and botany, irrefragable proofs of the metamorphoses, and transformations, and changes of the species, in accordance with the doctrine which we commenced with.

We now come to the second chapter of our subject. With the assumption, as I take it, already satisfactorily proven, of species having changed over into others—in considering this matter of geological succession or biological succession, I bring you face to face with the nature and mode of the change, and hence we may get a glance, perhaps, at its laws.

I have on the board a sketch or table which represents the changes which took place in certain of the mammalia. I give you a summary of the kind of thing which we find in one of the branches of palæontology. I have here two figures, one representing a restoration, and the other an actual picture, of two extinct species that belong to the early Eocene period. One represents the ancestor of the horse line, Hyracotherium, which has four toes on his anterior feet, and three behind; and the other, a type of animal, Phenacodus, which antedated all the horse series, the elephant series, the hog, the rhinoceros, and all of the other series of hoofed animals. Each presents us with the primitive position in which they first come to our knowledge in the history of geological time.

I have also arranged here a series of some leading forms of the three principal epochs of the Mesozoic times, and six of the leading ones of the Tertiary time. I have added some dates to show you the time when the faunæ which are entombed in those beds were discovered in the course of our studies; and you will easily see how unsafe it is to say that any given type of life has never existed, and assert that such and such a form is unknown; and it is still more unsafe, I think, to assert that any given form of life properly defined, or that a specific intermediate form of life, will not be found. I think it is much safer to assert that such and such intermediate forms will be found. I have frequently had the pleasure of realising anticipations of this kind. I have asserted that certain types would be found, and they have been found. You will see that I attend to the matter of time closely, because there have been a great many things discovered in the last ten or fifteen years in this department. In these forms I give the date of the discovery of the fauna in which they are embraced.

Formation	No. of toes	Feet	Astragalus	Carpus and Tarsus	Ulno-radius	Superior Molars	Zygapophyses	Brain
Miocene... ...	1–1 2–2	Digitigrade (Plantigrade)	Grooved (Flat)	Interlocking (Opposite)	Faceted	4-tubercles, crested and cemented	Doubly involute	Hemispheres larger, convoluted
Upper ... ... (Loup Fork)	3–3 4–4 (5·5)						Singly do.	
Middle ... ... (John Day)	2–2 3–3 4–4	Digitigrade	Grooved	Interlocking	Faceted Smooth	4-tubercles, and crested	Singly involute Doubly do.	Hemispheres larger, convoluted
Lower ... ... (White River)	3–3 4–3 4–4	Digitigrade Plantigrade	Grooved	Interlocking	Smooth Faceted	4-tubercles, and crested	? Singly involute	Hemispheres small; and larger
Eocene	3–3	(Digitigrade)	Grooved	Opposite	Smooth	4-tubercles	Singly involute	
Upper ... ... (Bridger)	4–3 4–5 5–5	Plantigrade	(Flat)	Interlocking		3-tubercles, and crested	Plane	Hemispheres small
Middle ... ... (Wasatch)	4–3 4–5 5–5	Plantigrade (Digitigrade)	Flat (Grooved)	Opposite Interlocking	Smooth	4-tubercles 3-tubercles, a few crested	Plane Singly involute	Hemispheres small; mesencephalon sometimes exposed
Lower ... ... (Puerco)	5–5	Plantigrade	Flat	Opposite	Smooth	3-tubercles (4-tubercles), none crested	Plane	Mesencephalon exposed; hemisphere small and smoother

Here we have the White River fauna discovered in 1856; then we skip a considerable period of time, and the next one was in 1869, when the Cretaceous series was found. Six or seven Cretaceous faunæ have been found. Thus we have the Bridger fauna in 1870, the Wasatch fauna in 1874. Next we have, in 1877, the Equus beds and the fauna which they embrace, which also was found in 1878. The Permian fauna, which is one of the last, is 1879; and the last, the Puerco, which gives the oldest and ancestral types of the modern forms of mammalia, was only found in 1881. When I first commenced the study of this subject, about 1860, there were perhaps 250 species known.

There are now something near 2000, and we are augmenting them all the time. I have found many myself: if they were distributed through the days of the year I think in some years I should have had several every day. But the accessions to knowedge which are constantly being made make it unsafe to indulge in any prophecies that, because such and such things have not been found, therefore such and such things cannot be; for we find such and such things really have been and really are discovered.

The successive changes that we have in the mammalia have taken place in the feet, teeth, and brain, and the vertebral column. The parts which present us the greatest numbers of variations are those in which many parts are concerned, as in the limbs and feet. In the Lower Eocene (Puerco) the toes were 5–5. In the Loup Fork fauna some possess toes but 1–1. Prior to this period no such reduction was known, though in the Loup Fork fauna a very few species were 5–5. Through this entire series we have transitions steady and constant, from 5–5, to 4–5, to 4–4, to 4–3, to 3–3, to 2–2, to 1–1. In the Puerco period there was not a single mammal of any kind which had a good ankle-joint, which had an ankle-joint constructed as ankle-joints ought to be, with tongue and groove. The model ankle-joint is a tongue-and-groove arrangement. In this period they were all perfectly flat. As time passes on, we get them more and more grooved, until in the Loup Fork fauna and the White River fauna they are all grooved. In the sole of the foot, in the Puerco fauna, they are all flat; but in the Loup Fork fauna the sole of the foot is in the air, and the toes only are applied to the ground, with the exception of the line of monkeys, in which the feet have not become erect on the toes, and the elephant, in which the feet are nearly flat also, and the line of bears, where they are also flat. As regards the ungulation between the small bones of the palm and of the sole there is not a single instance in which the bones of the toes are locked in the Lower Eocene, as they are in the later and latest Tertiary.

When we come to the limbs, the species of the Puerco fauna have short legs. They have gradually lengthened out, and in the late periods they are nearly all relatively long.

(*To be continued.*)

SOCIETIES AND ACADEMIES

LONDON

Royal Society, Dec. 13, 1883.—"On the Figure of Equilibrium of a Planet of Heterogeneous Density," by G. H. Darwin, F.R.S., Plumian Professor of Astronomy in the University of Cambridge.

If a rotating planet be formed of compressible fluid, the strata of equal pressure are of equal density, and the ellipticity of the strata increases from the centre outwards. Since it is supposed that the earth consolidated into its present form from a fluid or semi-fluid condition, the determination of the arrangement of internal density and of the law of ellipticity in such a planet is often called the problem of the figure of the earth. When the law of compressibility of the fluid is known, the laws of density and ellipticity are determinate, but the differential equations involved are of such complexity that only one solution of the problem is well known, viz. that associated with the names of Legendre and Laplace.[1]

In this solution the modulus of compressibility varies as the square of the density, but the assumption of this law appears to have been dictated more by the necessity of solving a certain differential equation than by physical considerations.

The comparison of the solution of the problem with the observed facts with regard to the earth may be made in several ways. The constant which determines the rate of the earth's precessional motion gives us information with regard to the arrangement of density in the interior, and the ellipticity of the surface is determined by geodesy and by the amount of a certain inequality in the moon's motion. Now, in order that the solution of the problem of the earth's figure may be satisfactory, the same arrangement of internal density must give the observed amounts both to the precessional constant and to the ellipticity of the surface.

Laplace's solution is highly satisfactory in this respect; and at the same time it makes the mean density of the whole earth about twice as great as the density of the surface stratum. The density of rock is about 2·8, and that of the whole earth is about 5·6.

In this state of our knowledge another solution of this celebrated problem possesses some interest, even if its results are not quite so satisfactory as those of Laplace's theory.

In the present paper such a solution is offered. The law of compressibility of the fluid is such, that the modulus varies as a power of the density, which power may range from negative infinity to $\frac{4}{3}$. When the power is zero, we have constant compressibility; and when the power is unity, we have the same law of compressibility as in a gas.

The solution is expressible in a far simpler algebraic form than that of Laplace, and it differs from his solution in placing a far larger proportion of the mass of the planet in the central regions.

It is remarkable that this solution affords for the case of the earth a correspondence between the precessional constant and the surface ellipticity equally good with that of Laplace. To obtain this correspondence we have to assume the compressibility of the fluid to be nearly constant.

The density of the surface layer appears however to be 3·7, and this is considerably greater than that of ordinary rocks. This result tells adversely to the acceptability of the proposed solution, but the discrepancy is not so serious as might appear at first sight. It appears from pendulum experiments on the Himalayan plateau and on the Andes, that there is a considerable deficiency of density underneath those great ranges. This would favour the view that our continents are a mere intumescence of the surface layers. In this case there must be a somewhat abrupt change in the law of density at only a few miles below the surface. The theory of the earth's figure can take no account of a sudden change of density on passing into a swollen superficial layer, and the value of the surface density to be used is that which is to be found immediately below the swollen part.

The author therefore points out that whilst the solution now offered cannot be held to be quite as satisfactory as that of Laplace, yet its inferiority is not of a kind to render altogether unacceptable the contention that it may be somewhere near the truth.

Linnean Society, December 20, 1883.—Alfred W. Bennett, M.A., in the chair.—Messrs. N. Cantley, W. Dobson, F. G. Smart, and Rev. R. Thom were elected Fellows of the Society —Mr. S. O. Ridley exhibited and made remarks on a series of 177 vertical sections of sponges collected in the neighbourhood of Point de Galle, Ceylon, by Dr. W. C. Ondaatje, F.L.S., and transmitted to England by him in letters. They are in most instances sufficient for the identification of the genera and some species.—Mr. F. Maule Campbell showed the web of a spider (*Tegenaria guyonii*) which had been spun in the centre of a pasteboard cylinder, the peculiarity being the manner in which the solid part of the web was medially swung, whereas in this species of spider it is more usually on the sides of objects.—A paper was read by Mr. F. O. Bower on the structure of the stem of *Rhynchopetalum montanum*. The plant is a native of Abyssinia, growing in districts 11,000 to 13,000 feet above the level of the sea. It differs from its ally Lobelias in being perennial. Internally it is succulent when young, but afterwards the surface becomes scarred as the leaves drop off, and exteriorly is hardened by a thick corky deposit. *Rhynchopetalum*, the author shows in detail, has certain peculiarities in the arrangement of the tissue of its leaf bundles, since the cortical system does not consist of branches of bundles of the leaf trace, but are cauline bundles, in this respect differing widely from such forms as *Lathyrus casuarina*, many *Begonias*, &c. *Rhynchopetalum*, moreover, has the cortical bundles running obliquely, and forming a regular four-sided meshed network related to the leaf bases and bundles of leaf trace. In these respects it approaches *Cycas*, in which latter the bundles of the accessory cortical system are not so regular and are almost vertically arranged. Some Cycads and *Rhynchopetalum* also agree in the exterior appearance of their stem, so that palæontologists might be deceived in their judgment if two well-preserved specimens were examined by them.—A communication was read on the auditory ossicles of *Rhytina stelleri* by Alban Doran. This was based on skeletons obtained by the *Vega* expedition, and shown at the late International Fisheries Exhibition by the Swedish Government. The author arrives at the conclu-

[1] The late M. Roche seems to have also solved the problem in 1848, and his paper is published in the *Memoirs of the Academy of Montpellier*.

sion that the malleus of *Rhytina* is larger than in *Manatus*, and therefore it is the largest and bulkiest malleus to be found in the whole section of the animal kingdom where such a bone exists, that in the characters of its body it resembles *Manatus* rather than *Halicore*, and that in the manubrium it differs from the other Sirenia, and is far more generalised. The incus is of the *Manatus* type, and so is the stapes, which is also the largest and bulkiest stapes to be found in any animal.—A paper on the organs of secretion in the Hypericaceæ, by Mr. J. R. Green, was read. He concludes (1) that the view advocated by Link, Martinet, and De Bary, of the lysigenous origin of the reservoirs of ethereal oil in these plants is the correct one; (2) that there exists in many parts of the plants a series of ducts or passages differing only slightly from these reservoirs, the differences being that they are not globular and isolated, but are generally connected more or less intimately with each other, and that their secretion is not a clear ethereal oil, but a viscid or resinous liquid, the points of agreement being those connected with their development and function; (3) that at least in some species there is also a series of schizogenous ducts confined to certain portions of the phloem; (4) that the dark glands which have been described are in intimate relationship with the fibrovascular system; (5) that the formation of resin and kindred secretions in these plants is confined to the parts where metabolism is active, and where there is a primary meristem. That all such parts give evidence of such formation with the exception of the roots.—A paper on the glands of *Coprosma baueriana*, by Mr. Walter Gardiner, was read. These glands are externally well developed and very typical. The so-called stipular body is placed immediately behind each leaf, and in the young condition the stipule arches over the leaf, and the glands with which it is provided secrete copiously a mucilaginous fluid, which bathes and surrounds the young leaf structure. As to the development of the glands, they arise as protrusions of the stipule parenchyma, which are covered by an epidermis. Each epidermal cell then rapidly grows out at right angles to the protuberance. In *Coprosma* the glands are situated on the sides of the stipules, but it more usually occurs in other genera that they are distributed over the inner face of the base of the stipular organ.—The last paper taken was on the development of starch grains in the laticiferous cells of the Euphorbiaceæ, by M. C. Potter. It is pointed out that while the discovery of the existence of starch-forming corpuscles had been made by Kruger, yet he had failed to interpret their function, which Mr. Potter's researches now fully prove in the case of the Euphorbiaceæ, where the development of rod or spindle-shaped grains of starch lying within cell protoplasm has been clearly demonstrated.

Chemical Society, Dec. 6, 1883.—Dr. Perkin, president, in the chair.—The following gentlemen were elected Fellows:—F. A. Blair, T. J. Barr, C. J. Baker, L. Briant, R. G. Durrant, Kamchundra Datta, L. L. Garbutt, A. E. Harris, T. Hart, W. Irwin, S. Johnson, R. Jackson, H. C. Lee, W. H. Martin, C. E. Potter, B. M. K. Rogers, C. W. Stephens, P. H. Wright, H. A. Wetzel, and W. G. Whittam.—The following papers were read:—On the constitution of the fulminates, by E. Divers and M. Kawakita. When moist mercury fulminate is treated with much strong hydrochloric acid, hydroxyammonium chloride and hydrocyanic acid are formed; if the fulminate be dry, no prussic acid is formed. The carbon is completely converted into formic acid. No oxalic acid is produced.—Theory of the constitution of the fulminates, by E. Divers.—On Liebig's production of fulminating silver without the use of nitric acid, by E. Divers and M. Kawakita. When nitrous acid is passed into an alcoholic solution of nitrate of silver, crystals separate; these are not, as Liebig stated, fulminating silver, but nitrate of silver.—Note on the constitution of the fulminates, by H. E. Armstrong.—Experimental investigation on the value of iron sulphate as a manure for certain crops, by A. B. Griffiths. The author obtained from an experimental plot of land manured with ferrous sulphate fifty-six bushels of beans; a similar plot in its normal state gave thirty-five bushels. The ash of the plants also contained more iron and phosphoric acid in the first case.

Physical Society, December 8.—Prof. G. Carey Foster, in the chair.—New members:—Major McGregor, R.E., Mr. James Walker, M.A., Mr. W. B. Gregory, B.A.—Prof. Silvanus P. Thompson, D.Sc., read a paper on the static induction telephone as an instrument of research. The author had employed Dolbear's telephone in investigating the action of influence machines such as those of Holtz and Wimshurst or Toepler. This was done by holding the end of a wire (connected to one terminal of the telephone) near the electrified parts of the machine, for example the "carriers" in the Toepler apparatus. The carriers induced a change in the telephone, whose other terminal was to earth, as they passed, and the pitch of the note heard in the telephone increased with the speed at which the machine was driven. Useful results were obtained leading to modifications of some machines. The same telephone was also applied to the measurement of capacities of condensers arranged like the resistances of a Wheatstone balance, and the telephone taking the place of a galvanometer. For the "divided coil" of the balance Prof. Thompson substituted a double condenser, or rather two condensers, so joined that the earth-plates were separate, while the other plates were in one. This device was made from two glass tubes with tinfoil round their outsides and a brass tube sliding into both interiors in such a way that the relative capacities of the two condensers thus combined could be altered by sliding the tube between them. A modification of this plan was suggested by Mr. Starling, the author's assistant, which was analogous to Prof. Foster's arrangement of the Wheatstone balance, that is to say, six condensers were used, the two extra ones being included between the battery connections and the sliding tube. The battery was in this case an induction coil having no condenser, as a discontinuous current is necessary to give sounds. The author also showed that the Dolbear telephone could be used instead of the quadrant electrometer in such experiments as those of Mr. J. E. H. Gordon on specific inductive capacity. The author also showed how he had applied it to explore the equipotential surfaces round conductors charged statically by an induction current. With two wires from the terminals of a telephone silence is produced when both ends are on the same equipotential surfaces; and sounds when they are not.—Prof. Thompson then read a note on a new insulating stem. This consisted of a glass tube with one end blown into a flat foot, which was planted on the bottom of a glass bottle and cemented there by a little wax paraffin. The upper and open end of the tube served to hold the stems of brass plates, or other electrified bodies. Paraffin oil or strong sulphuric acid could be used in the bottom of the bottle. A cap of rubber or percha made to slide up the stem served as a dust cover.—Prof. Thompson next made a communication on the first law of electrostatics, and illustrated his remarks with experiments showing how a series of floating magnet poles of like name repelling one another tend to produce equal distribution of the poles. Prof. Thompson, arguing from the second law of electrostatics (inverse squares), sought to explain the first law in a rational manner, on the hypothesis of self-repelling molecules, which tend to uniform distribution. When there is a surplus in one part and a deficit in another, the molecules are urged towards each other, *i.e.* attract. This was shown by putting a surplus of floating magnets at one part of the basin. By the movements of these magnets when confined by barriers, and with surplus and deficit purposely made, the author imitated the effects of a Leyden jar, induction, a battery current, &c., the motions and arrangement of the poles illustrating the hypothetical behaviour of electricity. The author was led by the hypothesis to infer that either the ether is electricity, or that the ether is electrified, and the former seemed the simpler conclusion.—Dr. Monkman showed some experiments illustrating the attraction and repulsion of bodies in motion. The attraction of a light balanced body to a vibrating tuning-fork was shown; also the attraction between two disks of paper revolving parallel and in the same direction. The author showed that two smoke-rings travelling abreast in the same direction attracted each other, and that two paper rings revolving in the same direction close together attract, while if revolving in opposite directions they repel.—Mr. Walter Baily exhibited his new integrating anemometer in action by means of a small electric motor, which took the place of the Robinson cups. The apparatus sums up, or integrates, the wind velocities on the lines of the four cardinal points. An electric counter is attached.

PARIS

Academy of Sciences, December 17, 1883.—M. Blanchard, president, in the chair.—Preliminary report on the expedition of the *Talisman* to the Atlantic Ocean, by M. Alph. Milne-Edwards.—On the preparation and manner of employing artificially developed virus attenuated by heat, intended to be used in prophylactic inoculations against charbon, by M. A. Chauveau. —On the remarkable sunsets observed during the months of November and December, 1883, by M. P. de Gasparin. The author considers that these luminous effects cannot be due to falling stars, and must be referred to the solar light acting on an atmosphere charged with particles of matter whose nature has not yet been determined.—On the determination of elastic forces, by M. Fontaneau.—On the processes adopted by M. Mandon and M. Aman-Vigié in the treatment of vines affected by phylloxera, by M. F. Henneguy. The process of Dr. Mandon, which consists in saturating the sap with a solution of phenic acid, appears to have little or no effect on the parasite. That by M. Aman-Vigié, an injection of a mixture of vapours of sulphur and sulphuric acid into the ground, has been tried on too limited a scale to warrant any definite judgment as to its efficacy. but the experiments already made do not appear to have proved very beneficial, because the vapours of sulphuric acid do not penetrate to a sufficient depth into the ground, and evaporate too rapidly.—Observations of the Pons-Brooks comet made at the Paris Observatory with the bent equatorial, by M. Périgaud. —Observations of the planet 235 Carolina and of the Pons-Brooks comet made at the Paris Observatory (west equatoreal in the garden), by MM. Henry.—On the multipliers of linear differential equations, by M. Halphen.—On a point in the theory of elliptical functions, by M. Lipschitz.—On a theorem of M. Liouville in mathematical analysis, by M. Stieltjes. In continuation of his previous paper, the author here shows how the theory of elliptical functions leads to the theorem of M. Liouville.—On algebraic equations, by M. H. Poincaré.—Demonstration of the fundamental properties of the system of geodesic polar coordinates, by M. G. Ossian-Bonnet.—On a method of generating the ovals of Descartes proposed by Chasles in his "Aperçu Historique," by M. Maurice d'Ocagne.—On the measurement of the specific heats and variations of temperature of two bodies in contact, by M. Morisot.—On a practicable method available for the photometric comparison of the usual sources diversely coloured, by M. J. Macé de Lépinay.—On the influence of colour on the sensitiveness of the eye to different degrees of luminosity, by M. Aug. Charpentier.—Researches on the permanence of the solidification of superfused sulphur (continued), by M. D. Gernez.—Second note on chromic selenite; preparation of biselenite, by M. Ch. Taquet. The author has obtained a biselenite of chromium by the action of nitric acid on neutral selenite. It is almost insoluble in water, but soluble in acids, and decomposable by heat.—Note on the action of bromium on pilocarpine ($C_{22}H_{16}Az_2O_4$), by M. Chaasting.—On emetics of mucic and saccharic acids, by M. D. Klein.—Third note to serve as a contribution to the history of the formation of coal; genus *Arthropitus*, Gœppert, by M. B. Renault.—On the artificial reproduction of schistosity and slate layers, by M. Ed. Jannettaz.—Experiment relative to the mode of formation of bauxite and gypsum, by M. Stan. Meunier.—On the glaucous amphibolic schists of the island of Groix, by M. Barrois.—On an anorthite rock discovered at Saint Clément, Canton of Saint-Anthême (Puy-de-Dôme), by M. F. Gonnard.—On the fall of cosmic dust, by M. E. Young.—On the coincidence of the recent phenomenal after-glows with the passage of the cosmic meteors, by M. Chapel.

December 24, 1883.—M. Blanchard, president, in the chair. —The President announced the painful loss sustained by the Academy in the person of M. Yvon Villarceau, member of the Section for Geography and Navigation, who died after a short illness on December 23. Funeral orations on the deceased savant were pronounced by Col. Perrier in the name of the Academy, by M. Faye in the name of the Bureau of Longitudes, and by M. Tisserand in the name of the Paris Observatory.—Separation of gallium (continued): separation from terbium, ytterbium, and the earth provisionally called y_a by M. de Marignac, from scandium and fluor, by M. Lecoq de Boisbaudran.—Observations of the comet Pons-Brooks, made at the Observatory of Algiers by MM. Trépied and Rambaud.—Observations of the same comet made at the Lyons Observatory (Brunner equatorial of 0·160 metre), by M. Gonnessiat.—On a special development of the perturbing function

$$\frac{1}{\Delta^s} = \frac{1}{(r^2 - 2rr' \cos v + r'^2)^{\frac{s}{2}}},$$

by M. O. Backlund.—On the purely trigonometrical series connected with M. Linstedt's new solution of the problem of three bodies, by M. H. Poincaré.—On the generation of geometrical surfaces, by MM. J. S. and M. N. Vanecek.—On the gauging of galvanometers, by M. E. Ducretet.—Researches on the permanency of the solidification of superfused sulphur (continued); production of a new crystallised variety of sulphur, by M. D. Gernez.—On the decomposition undergone in the presence of water by the acid phosphates of the alkaline earthy bases, by M. A. Joly.—Determination of the neutralising heat for the fluorhydric acid of the alkaline and alkaline-earthy bases, by M. Guntz.—On the kreatines and kreatinines, fourth note, by M. E. Duvillier.—Action of ammoniacal gas on the nitrate of methyl, by MM. E. Duvillier and H. Malbot.—Researches on the compound oxygenised ammonias, by M. Reboul. — On some haloid derivatives of ethane, by M. L. Henry.—On the pathologic anatomy of the phlegmon, and especially on the seat of the bacteria in this affection, by M. Cornil.—On the species of Arctic mollusks found by the *Talisman* Expedition at great depths in the intertropical waters of the Atlantic Ocean, by M. P. Fischer.—On the morphology of the plumicole Sarcoptides, by MM. E. L. Trouessart and P. Mégnin.—On a rapid and economical method of treating vines affected by Peronospora, by M. Senderens.—On a parasitic Nematode of the common onion, by M. Joannes Chatin.—On the cultivation of beetroot and some other plants in solutions of organic substances in decomposition, by M. V. Jodin.—On the relations of the Serpentine rocks to saline substances, especially in the Pyrenees, by M. Dieulafait.—On a chlorosilicate of lime, by M. Le Chatelier.—Experimental researches on the velocity of aqueous or atmospheric currents capable of holding in suspense mineral particles, by M. J. Thoulet.—Note on the sunset glows recently reported to the Academy, by M. E. Marchand.—Observation of the after-glows witnessed at Valence on the evening of December 2, by M. P. du Boys.—Remarks on the sunsets observed at Rambouillet on the evenings of December 15 and 18, by M. A. Laugier.—Letter on the sunsets observed at Christiania towards the end of November, by M. Fearnley, director of the Christiania Observatory.

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